W^m Bradford Jun

[illegible] B[illegible]

TRANSACTIONS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY,

HELD AT

PHILADELPHIA,

FOR PROMOTING

USEFUL KNOWLEDGE.

VOLUME II.

PHILADELPHIA:

PRINTED AND SOLD BY *ROBERT AITKEN*, AT POPE's HEAD
IN MARKET STREET.

M.DCC.LXXXVI.

ADVERTISEMENT.

THE peculiar circumſtances of America, ſince the publication of the firſt volume of the Tranſactions of this Society, will be a ſufficient apology for the long delay in publiſhing a ſecond. The Society having, however, reſumed their former labours in promoting uſeful knowledge, which were neceſſarily ſuſpended during the late war: and finding themſelves in poſſeſſion of materials more than ſufficient for a ſecond volume of Tranſactions, appointed a Committee to ſelect ſuch pieces as might be moſt proper for that purpoſe: The Committee have made that ſelection, which is here offered to the public. Several pieces ſtill remain worthy of publication, which will probably appear in a future volume.

It

It may not be amiſs in this place to inſert the Rules which the Society have adopted for the direction of their Committees in the choice of papers for publication.

FIRST, "That the grounds of the Com-"mittee's choice of papers for the preſs, ſhould "always be the importance or ſingularity of "the ſubjects, or the advantageous manner of "treating them, without pretending to an-"ſwer, or to make the ſociety anſwerable, "for the certainty of the facts, or propriety "of the reaſonings, contained in the ſeveral "papers ſo publiſhed, which muſt ſtill reſt on "the credit or judgment of their reſpective "authors.

SECONDLY, "That neither the Society, nor "the Committee of the preſs, do ever give "their opinion as a body, upon any paper "they may publiſh, or upon any ſubject of "Art or Nature that comes before them."

LAWS AND REGULATIONS,

OF THE

American PHILOSOPHICAL SOCIETY

HELD AT *PHILADELPHIA*, FOR PROMOTING

USEFUL KNOWLEDGE.

TWO ſocieties having formerly ſubſiſted in Philadelphia, whoſe views and ends were the ſame, viz. "*the advancement of uſeful knowledge*," it was judged that their union would be of public advantage; and they were accordingly united *January* 2d, 1769, by a certain *Fundamental Agreement*; the chief *Articles* of which are,

Firſt, That the name of the *United Society* ſhall be *The American Philoſophical Society, held at Philadelphia, for promoting uſeful Knowledge.*

Secondly, That there ſhall be the following officers of the ſociety, viz. one *Patron*, one *Preſident*, three *Vice-Preſidents*, one *Treaſurer*, four *Secretaries*, and three *Curators*.

Thirdly, That all the above officers ſhall be choſen annually by ballot, at the firſt meeting of the Society in January; excepting only that inſtead of electing a Patron, the Governor of the Province be requeſted to be Patron.

Other Laws were to be made by the *United Society;* and accordingly the following LAWS, &c. were paſſed *February* 3d, 1769.

I. *Of the annual Payments to be made by Members.*

Every Member of this Society ſhall ſubſcribe *Ten Shillings* yearly, to be applied by the Society to ſuch purpo-

ſes

ſes as they ſhall direct; and no Member ſhall be intitled to a vote in the annual election of officers, unleſs it appears that he has paid into the hands of the Treaſurer, the ſubſcription of the preceding year, and all former arrears, if any there were.

Every Member hereafter to be choſen, agreeable to the Laws of this Society, ſhall pay *Ten Shillings* admiſſion money, and alſo ſubſcribe for the yearly payment of *Ten Shillings*, before he is intitled to have any vote in the buſineſs of the Society at their meetings.

II. *Of the Election of Members.*

The election of new Members ſhall be by ballot, and that only on the third Friday in the months of January, April, July and October; and in order to ſuch election at leaſt twenty Members muſt be preſent.

Any Member may, at any meeting, propoſe ſuch perſon or perſons, as he thinks proper to be a Member or Members of this Society; but no perſon ſhall be ballotted for, unleſs his name, together with the name or names of the Member or Members who propoſed him, has been fixed up by the Secretaries for the view of the Society, at the two meetings preceding the time of election. Nor ſhall any perſon be deemed duly choſen unleſs three-fourths of the votes of the whole Members be in his favour.

III. *Of the Officers, and manner of their election.*

The election of ſuch Officers as are to be choſen in this Society, ſhall be on the firſt Friday in January every year, by ballot or written ticket, between the hours of two and five in the afternoon, at ſuch place in this city as ſhall be fixed by the Society at their previous meeting on the third Friday in December every year; of which notice ſhall be given in the Gazette, or ſuch other public papers as the Society

Society ſhall order, at leaſt one week before the day of election.

Before opening the election, the company that ſhall be met at half an hour after two, ſhall appoint three Members of the Society as judges of the election, and alſo two clerks or ſecretaries, for taking down the names of the voters. And in caſe of an equality of votes for any Officer, after caſting up the ballots, the deciſion ſhall be by lots, to be drawn by one of the judges.

IV. *Of the Preſident.*

The Preſident is to preſide at all meetings, to preſerve order, to regulate the debates, and to ſtate and put queſtions, agreeable to the ſenſe and intention of the Members.

V. *Of the Vice-Preſidents.*

In the abſence of the *Preſident*, his duty ſhall devolve on the *Vice-Preſidents*, ſo that they ſhall preſide alternately at meetings. But if the Vice-Preſident, whoſe turn it is to preſide at any meeting, ſhould be abſent, his place ſhall be ſupplied by any of the other Vice-Preſidents, who ſhall be preſent, according as he may be next in turn. If only one Vice-Preſident be preſent, he ſhall of courſe preſide; and if neither the Preſident, nor any Vice-Preſident be preſent, the Members met, ſhall appoint one of their number to take the chair for that meeting.

VI. *Of the Treaſurer.*

The *Treaſurer* ſhall receive the ſubſcriptions of the Members, and all other monies that may become due to the Society, and ſhall pay the ſame agreeable to their orders, certified to him by the Preſident, Vice-Preſident or Member, who was in the chair when the order was made.

The

The Treaſurer ſhall keep a regular account of all monies received and paid by him as aforeſaid; and once every year, or oftener if required by the Society, he ſhall render an account to them of the ſtock in his hands, and the diſburſements made by their order, and ſhall deliver up to his ſucceſſor the books and all papers belonging to them, together with the balance of caſh in his hands. And for the faithful diſcharge of his truſt, he ſhall, before he enters on his office, give bond and ſecurity to the Preſident and Vice-Preſidents, in double the ſum which they, or any three of them, ſhall judge he may probably become entruſted with during his ſaid office.

VII. *Of the Secretaries.*

The *Secretaries* ſhall ſo ſettle matters as to take equal ſhares of all buſineſs, and ſo as that two of them ſhall ſerve at every meeting, viz. one to take the minutes, and one to read all letters and papers that may be communicated to the Society. It is alſo the buſineſs of the two Secretaries of each particular meeting, to copy into the minute-book the proceedings of that meeting, in order to produce the ſame fair to the next meeting. They are further to copy into the proper books all ſuch letters, papers and eſſays, as the Society may think fit to preſerve on record, and to have the ſame ready to be laid before the next meeting.

The other two Secretaries are, in the mean while, to give notice to new members of their election, and agreeable to the directions of the Society, to write or anſwer letters; and, in general, to *manage all matters of correſpondence.*

The Secretaries may, for their own eaſe, change places; ſo that the two who have ſerved as *correſponding Secretaries*, for one month or limited time, ſhall take their turn to ſerve for the like time as *ſitting or attending Secretaries.*

VIII. *Of*

VIII. *Of the Curators.*

The bufinefs of the *Curators* fhall be to take charge of, and preferve, all *Specimens of natural Productions*, whether of the *Animal*, *Vegetable* or *Foffil* kingdom; all models of machines and inftruments, and all other matters and things belonging to the Society, which fhall be committed to them; to clafs and arrange them in their proper order, and keep an exact lift of them, with the names of the refpective donors, in a book provided for that purpofe; which book fhall be laid before the Society, as often as called for.

The Curators, on entering upon their office, fhall give fuch a receipt for every thing that is committed to their charge, as the Society fhall think proper; and, at the end of their term, fhall deliver up the fame to their fucceffors. For the faithful performance of their duty, and of the truft repofed in them, they fhall give bond to the Prefidents and Vice-Prefidents, in fuch a fum as they, or any three of them, fhall require.

IX. *Of the Meetings of the Society.*

The ordinary meetings of the Society fhall be on the firft and third Fridays of every month, from October to May, both inclufive, at fix o'clock in the evening, and on the third Friday in each of the other four months at feven o'clock.

No meeting fhall be continued after ten o'clock, nor any new matter be introduced by motion, or otherwife, after nine o'clock.

X. *Of the Difpofition of Money, and making new Laws.*

No part of the Society's ftock fhall be difpofed of in *Premiums*, or otherwife, nor fhall any new laws be made,

 until

until the ſame have been propoſed at one meeting, and are agreed to by two-thirds of twenty or more Members preſent at a ſubſequent meeting.

XI. *Of other Proceedings of the Society.*

No queſtion ſhall be put on a *motion*, unleſs the motion be *ſeconded*; and the determination of any queſtion ſhall be by *ballot*, inſtead of open ſuffrage, if deſired by any four Members. In caſe of an equality of votes on any queſtion, the ſame ſhall be deferred to another meeting.

When any Member ſpeaks he ſhall ſtand up, and addreſs himſelf to the chair, and the reſt ſhall remain ſilent in their ſeats. When two or more offer to ſpeak at the ſame time, the preſiding Member, in that, as in other matters of order, ſhall regulate and determine who ſhall ſpeak firſt.

XII. *Of Committees.*

The Members of this Society ſhall be claſſed into one or more of the following *Committees.*

1. Geography, Mathematics, Natural Philoſophy and Aſtronomy.
2. Medicine and Anatomy.
3. Natural Hiſtory and Chemiſtry.
4. Trade and Commerce.
5. Mechanics and Architecture.
6. Huſbandry and American Improvements.

Theſe *Committees* ſhall meet on their own adjournments, and at ſuch other times as the Society ſhall appoint, for the conſideration of any matters referred to them, and ſhall have power to chuſe their own chairman. But no Committee, as ſuch, ſhall take up any new buſineſs of the Society, but ſhall confine themſelves only to the ſubjects for which they are appointed, and to matters referred to them by the Society.

An ACT for Incorporating the American Philoſophical Society, held at Philadelphia, for Promoting uſeful Knowledge.

WHEREAS the cultivation of uſeful knowledge, and the advancement of the liberal arts and ſciences in any country, have the moſt direct tendency towards the improvement of agriculture, the enlargement of trade, the eaſe and comfort of life, the ornament of ſociety, and the increaſe and happineſs of mankind. *And whereas* this country of North-America, which the goodneſs of Providence hath given us to inherit, from the vaſtneſs of its extent, the variety of its climate, the fertility of its ſoil, the yet unexplored treaſures of its bowels, the multitude of its rivers, lakes, bays, inlets, and other conveniencies of navigation, offers to theſe United States one of the richeſt ſubjects of cultivation, ever preſented to any people upon earth. *And whereas* the experience of ages ſhews that improvements of a public nature, are beſt carried on by ſocieties of liberal and ingenious men, uniting their labours, without regard to nation, ſect or party, in one grand purſuit, alike intereſting to all, whereby mutual prejudices are worn off, a humane and philoſophical ſpirit is cheriſhed, and youth are ſtimulated to a laudable diligence and emulation in the purſuit of wiſdom. *And whereas*, upon theſe principles, divers public ſpirited gentlemen in Pennſylvania, and other American ſtates, did heretofore unite themſelves, under certain regulations, into one voluntary ſociety, by the name of "*The American Philoſophical Society, held at Philadelphia for promoting uſeful Knowledge*," and by their ſucceſsful la-

 bours

bours and investigations, to the great credit of America, have extended their reputation so far, that men of the first eminence in the republic of letters in the most civilized nations in Europe, have done honour to their publications, and desired to be enrolled among their members. *And whereas* the Society, after having been long interrupted in their laudable pursuits by the calamities of war, and the distresses of our country, have found means to revive their design, in hopes of being able to prosecute the same with their former success, and being further encouraged therein by the public, for which purpose they have prayed us, *The Representatives of the Freemen of the Commonwealth of Pennsylvania*, that they may be created one body politic and corporate forever, with such powers, privileges, and immunities, as may be necessary for answering the valuable purposes which the said Society had originally in view.

Wherefore, in order to encourage the said Society in the prosecution and advancement of all useful branches of knowledge, for the benefit of their country, and of mankind; *Be it enacted, and it is hereby enacted, by the Representatives of the Freemen of the Commonwealth of Pennsylvania in General Assembly met, and by the authority of the same*, That the members of the said American Philosophical Society heretofore voluntarily associated for promoting useful knowledge, and such other persons as have been duly elected members and officers of the same agreeably to the fundamental laws and regulations of the said Society, comprised in twelve sections, prefixed to their volume of Transactions, published in Philadelphia by *William* and *Thomas Bradford*, in the year of our Lord one thousand seven hundred and seventy-one; and who shall in all respects conform themselves to the said laws and regulations, and such other laws, regulations and ordinances, as shall hereafter be duly made and enacted by the said Society, according to the tenor hereof, be, and forever

ever hereafter ſhall be one body corporate and politic in deed, by the name and ſtyle of *The American Philoſophical Society, held at Philadelphia, for promoting uſeful knowledge*, and by the ſame name they are hereby conſtituted and confirmed one body corporate and politic, to have perpetual ſucceſſion, and by the ſame name they and their ſucceſſors are hereby declared and made able and capable in law, to have, hold, receive, and enjoy lands, tenements, rents, franchiſes, hereditaments, gifts and bequeſts of what nature ſoever, in fee-ſimple, or for term of life, lives, years, or otherwiſe, and alſo to give, grant, let, ſell, alien, or aſſign the ſame lands, tenements, hereditaments, goods, chattels, and premiſes, according to the nature of the reſpective gifts, grants and bequeſts, made to them the ſaid Society, and of their eſtate therein.

Provided, That the amount of the clear yearly value of ſuch real eſtate do not exceed the value of ten thouſand buſhels of good merchantable wheat.

And be it further enacted by the authority aforeſaid, That the ſaid Society be, and ſhall be for-ever hereafter able and capable in law to ſue, and be ſued, plead and be impleaded, anſwer and be anſwered unto, defend and be defended, in all or any of the courts or other places, and before any judges, juſtices, and other perſon and perſons, in all manner of actions, ſuits, complaints, pleas, cauſes and matters, of what nature or kind ſoever, within this commonwealth; and that it ſhall and may be lawful to and for the ſaid Society, for-ever hereafter to have and uſe one common ſeal in their affairs, and the ſame at their will and pleaſure to break, change, alter and renew.

And be it further enacted by the authority aforeſaid, That for the well governing the ſaid Society, and ordering their affairs, they ſhall have the following officers, that is to ſay, one Patron, who ſhall be his Excellency the Preſident of the Supreme Executive Council of this commonwealth, for the time being, and likewiſe one Preſident, fident, three Vice-Prefidents, four Secretaries, three Curators, one Treafurer, together with a Council of twelve Members; and that on the firft Friday of January next, between the hours of two and five in the afternoon, as many of the members of the faid Society as fhall have paid up their arrears due to the Society, and fhall declare their willingnefs to conform to the laws, regulations and ordinances of the Society, then duly in force, according to the tenor hereof, by fubfcribing the fame, and who fhall attend in the hall, or place of meeting of the faid Society, within the time aforefaid, fhall choofe by ballot, agreeably to the fundamental laws and regulations herein before referred to, one Prefident, four Secretaries, three Curators, and one Treafurer, and at the fame time and place, the Members met and qualified as aforefaid, fhall in like manner choofe four Members for the Council, to hold their offices for one year, four more Members for the Council to hold their offices for two years, and four more Members for the Council to hold their offices for three years. And on the firft Friday in January, which fhall be in the year of our Lord one thoufand feven hundred and eighty-two, and fo likewife on the firft Friday of January, yearly and every year thereafter, between the hours of two and five in the afternoon, the Members of the faid Society met and qualified as aforefaid, fhall choofe one Prefident, three Vice-Prefidents, four Secretaries, three Curators and one Treafurer, to hold their refpective offices for one year; and four Councilmen, to hold their offices for three years. *Provided*, That no perfon refiding within the United States fhall be capable of being Prefident, Vice-Prefident, Secretary, Treafurer, or Member of the Council, or of electing to any of the faid offices, who is not capable of electing and being elected to civil offices within the ftate in which he refides. *Provided alfo*, That nothing herein contained, fhall be confidered as intended to exclude any of the faid officers or counfellors, whofe times fhall be expired, from being re-elected, according to the pleaſure of the ſaid Society; and of the day, hours, and place of all ſuch elections, due notice ſhall be given by the Secretaries, or ſome one of them, in one or more of the public news-papers of this ſtate, agreeably to the ſaid fundamental laws and regulations before referred to.

And be it further enacted by the authority aforeſaid, That the officers and council of the ſaid Society ſhall be capable of exerciſing ſuch power for the well governing and ordering the affairs of the Society, and of holding ſuch occaſional meetings for that purpoſe, as ſhall be deſcribed, fixed, and determined by the ſtatutes, laws, regulations and ordinances of the ſaid Society, hereafter to be made. *Provided always*, That no ſtatute, law, regulation or ordinance ſhall ever be made or paſſed by the ſaid Society, or be binding upon the Members thereof, or any of them, unleſs the ſame hath been duly propoſed, and fairly drawn up in writing, at one ſtated meeting of the Society, and enacted or paſſed at a ſubſequent meeting at leaſt the ſpace of fourteen days after the former meeting, and upon due notice in ſome of the public news-papers, that the enacting of ſtatutes and laws, or the making and paſſing ordinances and regulations, will be part of the buſineſs of ſuch meeting; nor ſhall any ſtatute, law, regulation or ordinance be then or at any time enacted or paſſed, unleſs thirteen Members of the ſaid Society or ſuch greater number of Members as may be afterwards fixed by the rules of the Society be preſent, beſides ſuch quorum of the officers and council as the laws of the Society for the time being may require, and unleſs the ſame be voted by two-thirds of the whole body then preſent; all which ſtatutes, laws, ordinances and regulations ſo as aforeſaid duly made, enacted and paſſed, ſhall be binding upon every Member of the ſaid Society, and be from time to time inviolably obſerved, according to the tenor and effect thereof; provided they be not repugnant or contrary

trary to the laws of this commonwealth, for the time being in force and effect.

And whereas nations truly civilized (however unhappily at variance on other accounts) will never wage war with the arts and ſciences and the common intereſts of humanity.

Be it further enacted by the authority aforeſaid, That it ſhall and may be lawful for the ſaid Society, by their proper officers, at all times, whether in peace or war, to correſpond with learned ſocieties, as well as individual learned men, of any nation or country, upon matters merely belonging to the buſineſs of the ſaid Society; ſuch as the mutual communication of their diſcoveries and proceedings in philoſophy and ſcience; the procuring books, apparatus, natural curioſities, and ſuch other articles and intelligence as are uſually exchanged between learned bodies for furthering their common purſuits. *Provided always*, That ſuch correſpondence of the ſaid Society be at all times open to the inſpection of the Supreme Executive Council of this commonwealth.

(Signed) JOHN BAYARD, *Speaker.*

Enacted into a Law at Philadelphia, on Wedneſday the fifteenth day of March, Anno Domini one thouſand ſeven hundred and eighty.

(Signed) THOMAS PAINE, *Clerk of the General Aſſembly.*

(Copy)

A Law to encreaſe the annual Subſcriptions of the Members of the American Philoſophical Society held at Philadelphia for promoting uſeful knowledge, and alſo to encreaſe the Depoſites of newly elected Members on their admiſſion into the ſaid Society.

WHEREAS the cuſtomary annual payment of *Ten Shillings* from each Member of the American Philoſophical Society, and alſo the cuſtomary depoſite of *Ten Shillings* by each newly elected Member, on his admiſſion into the ſaid Society, hath been found inadequate to the neceſſary and occaſional expenditures of the Society.

Be it therefore enacted, and it is hereby enacted by the American Philoſophical Society, held at Philadelphia for promoting uſeful knowledge, by virtue of the chartered rights to the ſaid Society granted, and by authority of the ſame, That for the future, that is to ſay, from and after the firſt of March next, the payments to be made by every Member of the ſaid Society ſhall be *Four Dollars* annually, and the depoſite to be made by every newly elected Member, from and after the ſaid firſt of March, ſhall be *Ten Dollars*. And no Member who ſhall be in arrear for his annual ſubſcription or depoſite, ſhall be eligible to any office in the ſaid Society, or be permitted to vote at elections, or at the ordinary meetings of the Society, until he ſhall have fully paid up his ſaid arrears.

And be it further enacted, That no newly elected Member ſhall receive a certificate of his election, or be admitted as a Member of the ſaid Society, until he ſhall have paid into the treaſury the ſaid depoſite of *Ten Dollars* and alſo his arrearages of *Four Dollars* per annum, if any ſuch ſhall have accrued from the time of his election.

 Provided

Provided always, That honorary Members in foreign parts, ſhall not be ſubject to this law, nor ſhall their certificates of election be with-held on account of their not paying the depoſite or annual ſubſcription aforeſaid. Nevertheleſs, if any ſuch foreign Member ſhould happen to come to the city of Philadelphia with a view of ſettlement or reſidence, then ſuch Member ſhall pay the depoſite money as before directed, and ſhall thenceforth be liable for the annual ſubſcription in common with other reſident Members.

And be it further enacted, That all former laws, ordinances or cuſtoms inconſiſtent with or contradictory to this act, be, and the ſame are hereby repealed.

Enacted into a law at a meeting of the American Philoſophical Society, according to Charter, this ſixth day of January, Anno Domini, one thouſand ſeven hundred and eighty-ſix.

Ex-

Extracts from the Minutes of the American Philosophical Society, respecting a Donation proposed by Mr. J. H. de Magellan, *of London.*

January 1786,

Mr. J. H. de Magellan of London, having in a letter, dated the 17th of September last, and communicated to the society by Mr. Vaughan, one of the Vice-Presidents, made an offer to the society of *two hundred guineas*, to be vested in a permanent fund, that the interest arising therefrom may be disposed of in annual premiums, to the authors of the best discoveries or most useful improvements relating to navigation, or to natural philosophy, mere natural history only excepted: And the society having most thankfully accepted the generous offer, appointed a committee to frame rules and conditions for the disposition of the proposed premiums, agreeable to the intention of the donor, expressed in his letter, but more precise in the terms; which being done, and approved of by the society, were immediately transmitted in a letter to Mr. Magellan, for his confirmation or amendment. They are as follow, viz.

1. The candidate shall send his discovery, invention or improvement, addressed to the President or a Vice-President of the Society, free of postage or other charges; and shall distinguish his performance by some motto, device or signature, at his pleasure. Together with his discovery, invention or improvement, he shall also send a sealed letter, containing the same motto, device or signature, and subscribed with the real name and place of residence of the author.

2. Persons

2. Perſons of any nation, ſect, or denomination whatever, ſhall be admitted as candidates for this premium.

3. No diſcovery, invention or improvement ſhall be entitled to this premium, which hath been already publiſhed, or for which the author hath been publicly rewarded elſewhere.

4. The candidate ſhall communicate his diſcovery, invention or improvement either in the Engliſh, French, German, or Latin language.

5. All ſuch communications ſhall be publicly read or exhibited to the Society, at ſome ſtated meeting, not leſs than one month previous to the day of adjudication; and ſhall at all times be open to the inſpection of ſuch members as ſhall deſire it. But no member ſhall carry home with him the communication, deſcription or model, except the officer to whom it ſhall be entruſted: nor ſhall ſuch officer part with the ſame out of his cuſtody, to any but the judges, who may demand it for conſideration.

6. The twelve counſellors, together with the other officers annually elected according to the charter and laws of the Society, ſhall be judges of the merits of the ſeveral communications, and award the premium. Which adjudication ſhall be determined by a majority of judges met; provided that ſuch majority be not leſs than ſeven concurring votes.

7. And for this purpoſe the counſellors and other officers, or at leaſt ſeven of them, ſhall meet on the ſecond Monday in December, in every year, to form their judgment and award the premium. After due conſideration had, a vote ſhall firſt be taken on this queſtion, viz. "Whether any of the communications then under inſpection, are worthy of the propoſed premium?" If this ſhall be determined in the *negative*, the whole buſineſs ſhall be deferred till another year: But if in the *affirmative*, the judges ſhall then proceed to determine, by vote, the diſcovery, invention or improvement moſt uſeful and worthy.

And

And that difcovery, invention or improvement which fhall be found to have the greateft number of concurring votes (being not lefs than feven) in its favour, fhall be fuccefsful. Whereupon a certificate in writing fhall be forthwith drawn of this adjudication, and figned by thofe who voted for the crowned fubject: And then, *and not till then*, the fealed letter accompanying the crowned performance, fhall be opened and the name of the author announced; which certificate fhall be prefented to the Society at their next ftated meeting, and delivered to the Secretary to be entered on record, in a bound book provided for this purpofe.

8. A full account of the crowned fubject fhall be publifhed by the Society as foon as may be, after the adjudication, either in a feparate publication, or in the next fucceeding volume of their Tranfactions, or in both.

9. The unfuccefsful performances fhall lie over for confideration, and remain, as candidates for the premium, for *five* fucceeding years next after their prefentment; unlefs the author or authors fhall think fit to withdraw them or any of them: And the Society fhall publifh annually an abftract of the titles, object or fubject matter of the communications fo under confideration, fuch only excepted as the counfellors and other officers fhall, by vote as aforefaid, have determined not worthy of public notice.

10. No counfellor or officer who is a candidate fhall fit in judgment, or give his vote.

11. The letters containing the names of authors whofe performances fhall be rejected, or fhall be found unfuccefsful after a trial of five years, fhall be burnt without breaking the feals.

12. In cafe there fhould be a failure, in any year, of any communication worthy of the propofed premium, there will then be two premiums awarded in the next year. But no accumulation of premiums fhall entitle an author to more than one premium for any one difcovery, invention or improvement.

13. The

13. The premium ſhall conſiſt of an oval plate of ſolid ſtandard gold, of the value of *Ten Guineas*. On one ſide thereof ſhall be neatly engraved the following motto, ——————— together with theſe words, *The donation of* —— *of London, eſtabliſhed in the year* 1786. And on the other ſide of the plate ſhall be engraved theſe words, *Awarded by the A. P. S. to* —— —— *for his diſcovery of* —— —— *A. D.* 17—. ——— *Preſident.* And the ſeal of the Society ſhall be annexed to the ſaid golden plate, by a ribbon paſſing through a ſmall hole near the lower edge thereof.

LIST

LIST OF THE OFFICERS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY,

Held at PHILADELPHIA, for promoting uſeful Knowledge,

For the Year 1786.

PATRON. His Ex^cy the Preſident of the Supreme Executive Council of the Commonwealth, for the time being.

PRESIDENT. His Excellency Dr. Benjamin Franklin, Eſquire, L. L. D F. R. S. &c. &c.

VICE-PRESIDENTS.
- Rev. Dr. John Ewing, Provoſt of the Univerſity of Pennſylvania.
- Rev. Dr. William White.
- Samuel Vaughan, Eſq.

SECRETARIES.
- Dr. James Hutchinſon.
- Mr. Robert Patterſon, Profeſſor of Mathematics in the Univerſity of Pennſylvania.
- Rev. Dr. Samuel Magaw, Vice-Provoſt of the Univerſity of Pennſylvania.
- Dr. John Foulk.

CURATORS.
- Dr. Samuel Duffield.
- Dr. Barnabas Binney.
- William Bradford, Eſq. Attorney General of Pennſylvania.

TREASURER.
- The Honorable Francis Hopkinſon, Eſq. Judge of the Admiralty in Pennſylvania.

COUN-

COUNSELLORS.

- The Hon. Thomas M'Kean, Esquire, L. L. D. Chief Justice of the Supreme Court of Pennsylvania.
- The Hon. George Bryan, Esquire, a Justice of the Supreme Court of Pennsylvania.
- Sieur Barbé de Marbois.
- Mr. Samuel Caldwell.
- Jared Ingersoll, Esq.
- Jonathan Bayard Smith, Esq.
- Rev. Mr. Robert Blackwell.
- David Rittenhouse, Esq.
- Dr. Benjamin Rush, Professor of Chemistry in the University of Pennsylvania.
- Dr. John Jones.
- Dr. Adam Kuhn, Professor of Materia Medica in the University of Pennsylvania.
- Rev. Mr. George Duffield.

LIST of MEMBERS of the AMERICAN PHILOSOPHICAL SOCIETY, elected since the Publication of the First Volume of Transactions, viz. since the 18th of January, 1771.

A

JOSEPH Astolinge, Esq. of Georgia.

B

Mr. Gerard Banker.
Dr. William Bryant, of New-Jersey. *
Mr. James Bringhurst.
Honorable Thomas Bee, Esq. of South-Carolina.
John Beale Boardley, Esq. of Maryland.
Rev. Jeremy Belknap, of New-Hampshire.
Dr. Barnabas Binney.
Rev. Mr. Robert Blackwell.

William

Note, Those Members whose places of residence are not specified, are of Philadelphia; and those marked with an * are deceased.

William Bradford, Eſq. Attorney-General of Pennſylvania.

C

Dr. John Carſon.
Rev. Manaſſeh Cutler, of Ipſwich, Maſſachuſetts.

D

Sharp Delany, Eſq.
Rev. Dr. Robert Davidſon, of Carliſle.
Mr. John Dunlap.

E

Dr. Jonathan Elmer, of New-Jerſey.
Joſeph Ellicott, Eſq. Bucks county. *
Andrew Ellicott, Eſq. of Maryland.

F

Mr. George Fox.
Dr. John Foulk.

G

Mr. George Gauld, of Penſacola.
Iſaac Gray, Eſq.
George Gray, Eſq.
Mr. Archibald Gamble, Profeſſor of Engliſh and Oratory, Univerſity, Philadelphia. *
Dr. Samuel Powell Griffitts.

H

Dr. James Hutchinſon.
Thomas Hutchins, Eſq. Geographer to the United States.
Ebenezer Hazard, Eſq. Poſtmaſter-General.
Samuel Huntington, Eſq. of Connecticut. *
Rev. Dr. Juſt. Hen. Chriſt. Helmuth, Prof. of the Germ. and Oriental Languages, Univerſity, Philadelphia.
Thomas Hayward, jun. Eſq. of South-Carolina.

I

Dr. Walter Jones, of Virginia.
Dr. John Jones, of Maryland.
Jared Ingerſoll, Eſq.
His Excellency Thomas Jefferſon, Eſq. of Virginia, Miniſter Plenipotentiary to the Court of France.

K

Rev. Dr. John C. Kunze, of New-York.

L

Mr. Jeſſe Lukens. *
Henry Laurens, Eſq. of South-Carolina.
Rev. Mr. William Ludlam, of Leiceſter.

Dr. James Lloyd, of Boston.

M

Mr. Archibald M'Clean, of York county. *
Capt. John Matreson, of New-York.
Timothy Matlack, Esq.
Dr. George Millegan, of South-Carolina.
James M'Clurg, M. D. Williamsburgh, Virginia.
Mr. Benjamin Morgan.
Rev. Dr. Samuel Magaw, Vice-Provost of Univer. Phila.
Hon. Dr. James M'Henry, Esq. of Baltimore.
Rev. Dr. James Madison, President of the College of William and Mary, Virginia.
Rev. Mr. Henry Muhlenberg, of Lancaster.

P

Dr. John Perkins, of Boston, Massachusetts.
Dr. Thomas Park.
Mr. Robert Patterson, Prof. Math. Univer. Philadelphia.
Hon. Mann Page, Esq. of Fredericksburgh, Virginia.
Thomas Paine, Esq. of Bordentown.
Charles Pettit, Esq.

R

Mr. Bernard Romans, of Pensacola.

S

Dr. Hugh Shiell. *
Jonathan Bayard Smith, Esq.
Jonathan Dickinson Sergeant, Esq.
Rev. Dr. Samuel Smith, Vice-President of the College in New-Jersey.

T

Dr. James Tilton, of Dover.
Mr. John Ternant.

V

Samuel Vaughan, Esq.
Mr. John Vaughan.

W

His Excellency General Washington, Virginia.
Rev. Samuel Williams, L. L. D. Hol. Prof. Mor. and Nat. Philos. College of Cambridge, Massachusetts.
Dr. Nicholas Way, of Wilmington.
George Wall, jun. Esq. of Bucks county.
Hon. Anthony Wayne, Esq. Gen. in the Armies of the United States.
Mr. Benjamin Workman, Teacher of Math. University, Philadelphia.

FOREIGN

FOREIGN MEMBERS.

A

Monſieur le Marquis d'Angeville, of Paris.

Dr. Adams, of Barbadoes.

Lieut. Stephen Adye, of the Royal Artillery.

B

Dr. Forbern Bergman, Prof. Math. Stockholm.

Major Frederick F. S. de Brahm, Triers.

His Excellency M. J. Peter Van Berckel, Miniſter Plenipotentiary from the United Provinces of the Netherlands.

Frederick Eugene Francis Baron de Beelen Bertholf, Imperial Councellor of Commerce to the United States, Bruſſels.

C

Le Chevalier de Chaſtellux, Marſhal of the Field in the Armies of France, Chevalier of the Royal Military Order of St. Louis, and one of the forty members of the French Academy.

Count de Campomanes, Fiſcal of the Council of Caſtile.

Dr. Adair Crawford, Phyſician to St. Thomas's Hoſpital, London.

Dr. Coſte.

Rev. Thomas Coombe, Ireland.

Daniel Coxe, Eſq. England.

D

Mr. Peter Dollond, of London.

Dr. Andrew Duncan, of Edinburgh.

Monſieur Daubenton, of the Royal Academy of Sciences at Paris.

Monſieur Dubourg, of Paris. *

Chev. Danmours, Conſul of France for the Southern Department.

E

Hon. John Ellis, Eſq. of Jamaica. *

Hon. Bryan Edwards, Eſq. of Jamaica.

F

Le Marquis de la Fayette, Major General in the Armies of the United States of America.

Abbé Fontana, Director of the Great Duke's Cabinet of Natural Hiſtory.

G

Rev. Thomas Gibbons, D. D. of London.

Count de Guichen, Lieut. Gen. in the French Army.

H

Hon. Samuel Guſtavus Baron Hermelin, of Stockholm.

William Herſchel, Eſq. F. R. S. of Bath, England.

I

Dr. Hugh James, of Jamaica.

K

Timothy Baron de Kleingſtedt, Councellor of State to the Empreſs of Ruſſia.

Brigadier-General Thadeus Koſcuzko.

L

Le Chev. de la Luzerne.

Monſieur Lavoiſier, of the Academy of Sciences in Paris.

M

Monſieur Barbé de Marbois, Intendant of St. Domingo.

Lord Mahone.

Monſieur Macquer, of Paris. *

Samuel Moore, Eſq. of London.

Dr. Benjamin Moſley, of Jamaica.

Mr. John Hyacinth de Magellan, F. R. S. and Member of ſeveral Academies, London.

Chriſt. Fred. Michaelis, M. D. of Gottenburgh.

Mr. John Mandrillon, Merchant, Amſterdam.

P

Rev. Joſeph Prieſtley, L. L. D. F. R. S. of Birmingham, England.

Rev. Richard Price, D. D. F. R. S. of London.

Dr. Robert Percival, Prof. Chym. Trinity College, Dublin.

Mr. William Parker, of London.

R

Monſieur le Roy, Vice-Director of the Academy of Sciences in Paris.

Monſieur le Roux.

Abbé Raynal.

Abbé Roſier, of the Academy of Sciences in Lyons.

S

Monſieur Süe, Profeſſor Royal of Anatomy, &c. at Paris.

Monſieur Jean Baptiſte Süe, Prof. Surgery in Paris.

Right Hon. Earl of Stanhope. *

Mr. Alexander Small, of London.

Mr. James Six, of Canterbury. England.

V

Monſieur le Count de Vergennes, Miniſter of State for Foreign Affairs, France.

W

Fortunatus de Warris, Eſq.

William Wright, M. D. F.R.S. in the Pariſh of Trelawney, Jamaica.

CONTENTS

OF

VOLUME II.

XII. *Ac*

CONTENTS.

XXV. *An*

CONTENTS.

TRANS-

TRANSACTIONS

OF THE

American PHILOSOPHICAL SOCIETY, &c.

N° I.

A Letter from Dr. B. FRANKLIN *to Dr.* INGENHAUSZ, *Physician to the Emperor, at Vienna.*

Dear Friend, At sea, August 28th, 1785.

Read 21st Oct. 1785. IN one of your letters, a little before I left France, you desire me to give you in writing my thoughts upon the construction and use of chimneys, a subject you had sometimes heard me touch upon in conversation. I embrace willingly this leisure afforded by my present situation to comply with your request, as it will not only show my regard to the desires of a friend, but may at the same time be of some utility to others; the doctrine of chimneys appearing not to be as yet generally well understood, and mistakes respecting them being attended with constant inconvenience, if not remedied; and with fruitless expence, if the true remedies are mistaken.

Those who would be acquainted with this subject should begin by considering on what principle smoke ascends in any chimney. At first many are apt to think that smoke is is in its nature and of itſelf ſpecifically lighter than air, and riſes in it for the ſame reaſon that cork riſes in water. Theſe ſee no cauſe why ſmoke ſhould not riſe in the chimney, though the room be ever ſo cloſe. Others think there is a power in chimneys to *draw* up the ſmoke, and that there are different forms of chimneys which afford more or leſs of this power. Theſe amuſe themſelves with ſearching for the beſt form. The equal dimenſions of a funnel in its whole length is not thought artificial enough, and it is made, for fancied reaſons, ſometimes tapering and narrowing from below upwards, and ſometimes the contrary, &c. &c. A ſimple experiment or two may ſerve to give more correct ideas. Having lit a pipe of tobacco, plunge the ſtem to the bottom of a decanter half filled with cold water; then putting a rag over the bowl, blow through it and make the ſmoke deſcend in the ſtem of the pipe, from the end of which it will riſe in bubbles through the water; and being thus cooled, will not afterwards riſe to go out through the neck of the decanter, but remain ſpreading itſelf and reſting on the ſurface of the water. This ſhows that ſmoke is really heavier than air, and that it is carried upwards only when attached to, or acted upon, by air that is heated, and thereby rarefied and rendered ſpecifically lighter than the air in its neighbourhood.

Smoke being rarely ſeen but in company with heated air, and its upward motion being viſible, though that of the rarefied air that drives it is not ſo, has naturally given riſe to the error.

I need not explain to you, my learned friend, what is meant by rarefied air; but if you make the public uſe you propoſe of this letter, it may fall into the hands of ſome who are unacquainted with the term and with the thing. Theſe then may be told, that air is a fluid which has weight as well as others, though about eight hundred times lighter than water. That heat makes the particles of air recede

recede from each other and take up more ſpace, ſo that the ſame weight of air heated will have more bulk, than equal weights of cold air which may ſurround it, and in that caſe muſt riſe, being forced upwards by ſuch colder and heavier air, which preſſes to get under it and take its place. That air is ſo rarefied or expanded by heat, may be proved to their comprehenſion by a lank blown bladder, which laid before a fire will ſoon ſwell, grow tight and burſt.

Another experiment may be to take a glaſs tube about an inch in diameter, and twelve inches long, open at both ends and fixed upright on legs ſo that it need not be handled, for the hands might warm it. At the end of a quill faſten five or ſix inches of the fineſt light filament of ſilk, ſo that it may be held either above the upper end of the tube or under the lower end, your warm hand being at a diſtance by the length of the quill. If there were any motion of air through the tube, it would manifeſt itſelf by its effect on the ſilk; but if the tube and the air in it are of the ſame temperature with the ſurrounding air, there will be no ſuch motion, whatever may be the form of the tube, whether crooked or ſtrait, narrow below and widening upwards, or the contrary; the air in it will be quieſcent. Warm the tube, and you will find as long as it continues warm, a conſtant current of air entering below and paſſing up through it, till diſcharged at the top; becauſe the warmth of the tube being communicated to the air it contains, rarefies that air and makes it lighter than the air without, which therefore preſſes in below, forces it upwards, follows and takes its place, and is rarefied in its turn. And, without warming the tube, if you hold under it a knob of hot iron, the air thereby heated will riſe and fill the tube, going out at its top, and this motion in the tube will continue as long as the knob remains hot, becauſe the air entering the tube below is heated and rarefied by paſſing near and over that knob.

Plate I. Figure I.

That

That this motion is produced merely by the difference of ſpecific gravity between the fluid within and that without the tube, and not by any fancied form of the tube itſelf, may appear by plunging it into water contained in a glaſs jar a foot deep, through which ſuch motion might be ſeen. The water within and without the tube being of the ſame ſpecific gravity, balance each other, and both remain at reſt. But take out the tube, ſtop its bottom with a finger and fill it with olive oil, which is lighter than water, then ſtopping the top, place it as before, its lower end under water, its top a very little above. As long as you keep the bottom ſtopt, the fluids remain at reſt, but the moment it is unſtopt, the heavier enters below, forces up the lighter, and takes its place. And the motion then ceaſes, merely becauſe the new fluid cannot be ſucceſſively made lighter, as air may be by a warm tube.

In fact, no form of the funnel of a chimney has any ſhare in its operation or effect reſpecting ſmoke, except its height. The longer the funnel, if erect, the greater its force when filled with heated and rarefied air, to *draw* in below and drive up the ſmoke, if one may, in compliance with cuſtom, uſe the expreſſion *draw*, when in fact it is the ſuperior weight of the ſurrounding atmoſphere that *preſſes* to enter the funnel below, and ſo *drives up* before it the ſmoke and warm air it meets with in its paſſage.

I have been the more particular in explaining theſe firſt principles, becauſe, for want of clear ideas reſpecting them, much fruitleſs expence has been occaſioned; not only ſingle chimneys, but in ſome inſtances, within my knowledge, whole ſtacks having been pulled down and rebuilt with funnels of different forms, imagined more powerful in *drawing* ſmoke; but having ſtill the ſame height and the ſame opening below, have performed no better than their predeceſſors.

What is it then which makes a *ſmoky chimney*, that is, a chimney which inſtead of conveying up all the ſmoke, diſcharges difcharges a part of it into the room, offending the eyes and damaging the furniture?

The caufes of this effect, which have fallen under my obfervation, amount to *nine*, differing from each other, and therefore requiring different remedies.

1. *Smoky chimneys in a new houfe, are fuch, frequently from mere want of air.* The workmanfhip of the rooms being all good, and juft out of the workman's hand, the joints of the boards of the flooring, and of the pannels of wainfcotting are all true and tight, the more fo as the walls, perhaps not yet thoroughly dry, preferve a dampnefs in the air of the room which keeps the wood-work fwelled and clofe. The doors and the fafhes too, being worked with truth, fhut with exactnefs, fo that the room is as tight as a fnuff-box, no paffage being left open for air to enter, except the key-hole, and even that is fometimes covered by a little dropping fhutter. Now if fmoke cannot rife but as connected with rarefied air, and a column of fuch air, fuppofe it filling the funnel, cannot rife, unlefs other air be admitted to fupply its place; and if, therefore, no current of air enter the opening of the chimney, there is nothing to prevent the fmoke coming out into the room. If the motion upwards of the air in a chimney that is freely fupplied, be obferved by the rifing of the fmoke or a feather in it, and it be confidered that in the time fuch feather takes in rifing from the fire to the top of the chimney, a column of air equal to the content of the funnel muft be difcharged, and an equal quantity fupplied from the room below, it will appear abfolutely impoffible that this operation fhould go on if the tight room is kept fhut; for were there any force capable of drawing conftantly fo much air out of it, it muft foon be exhaufted like the receiver of an air pump, and no animal could live in it. Thofe therefore who ftop every crevice in a room to prevent the admiffion of frefh air, and yet would have their chimney carry up the fmoke, require inconfiftencies, and

and expect impoffibilities. Yet under this fituation, I have feen the owner of a new houfe, in defpair, and ready to fell it for much lefs than it coft, conceiving it uninhabitable, becaufe not a chimney in any one of its rooms would carry off the fmoke, unlefs a door or window were left open. Much expence has alfo been made, to alter and amend new chimneys which had really no fault; in one houfe particularly that I knew, of a nobleman in Weftminfter, that expence amounted to no lefs than three hundred pounds, *after* his houfe had been, as he thought, finifhed and all charges paid. And after all, feveral of the alterations were ineffectual, for want of underftanding the true principles.

Remedies. When you find on trial, that opening the door or a window, enables the chimney to carry up all the fmoke, you may be fure that want of air *from without*, was the caufe of its fmoking. I fay *from without*, to guard you againft a common miftake of thofe who may tell you, the room is large, contains abundance of air, fufficient to fupply any chimney, and therefore it cannot be that the chimney wants air. Thefe reafoners are ignorant, that the largenefs of a room, if tight, is in this cafe of fmall importance, fince it cannot part with a chimney full of its air without occafioning fo much vacuum; which it requires a great force to effect, and could not be borne if effected.

It appearing plainly, then, that fome of the outward air muft be admitted, the queftion will be, how much is *abfolutely neceffary;* for you would avoid admitting more, as being contrary to one of your intentions in having a fire, viz. that of warming your room. To difcover this quantity, fhut the door gradually while a middling fire is burning, till you find that, before it is quite fhut, the fmoke begins to come out into the room, then open it a little till you perceive the fmoke comes out no longer. There hold the door, and obferve the width of the open crevice between

tween the edge of the door and the rabbit it ſhould ſhut into. Suppoſe the diſtance to be half an inch, and the door eight feet high, you find thence that your room requires an entrance for air equal in area to ninety ſix half inches, or forty eight ſquare inches, or a paſſage of ſix inches by eight. This however is a large ſuppoſition, there being few chimneys, that, having a moderate opening and a tolerable height of funnel, will not be ſatisfied with ſuch a crevice of a quarter of an inch; and I have found a ſquare of ſix by ſix, or thirty ſix ſquare inches, to be a pretty good medium, that will ſerve for moſt chimneys. High funnels with ſmall and low openings, may indeed be ſupplied through a leſs ſpace, becauſe, for reaſons that will appear hereafter, the *force of levity*, if one may ſo ſpeak, being greater in ſuch funnels, the cool air enters the room with greater velocity, and conſequently more enters in the ſame time. This however has its limits, for experience ſhows that no increaſed velocity ſo occaſioned, has made the admiſſion of air through the key-hole equal in quantity to that through an open door; though through the door the current moves ſlowly, and through the key-hole with great rapidity.

It remains then to be conſidered how and where this neceſſary quantity of air from without is to be admitted ſo as to be leaſt inconvenient. For, if at the door, left ſo much open, the air thence proceeds directly to the chimney, and in its way comes cold to your back and heels as you ſit before your fire. If you keep the door ſhut, and raiſe a little the ſaſh of your window, you feel the ſame inconvenience. Various have been the contrivances to avoid this, ſuch as bringing in freſh air through pipes in the jams of the chimney, which pointing upwards ſhould blow the ſmoke up the funnel; opening paſſages into the funnel above, to let in air for the ſame purpoſe. But theſe produce an effect contrary to that intended: For as it is the conſtant current of air paſſing from the room *through the*

the opening of the chimney into the funnel which prevents the ſmoke coming out into the room, if you ſupply the funnel by other means or in other ways with the air it wants, and eſpecially if that air be cold, you diminiſh the force of that current, and the ſmoke in its efforts to enter the room finds leſs reſiſtance.

The wanted air muſt then *indiſpenſably* be admitted into the room, to ſupply what goes off through the opening of the chimney. M. Gauger, a very ingenious and intelligent French writer on the ſubject, propoſes with judgment to admit it *above* the opening of the chimney; and to prevent inconvenience from its coldneſs, he directs its being made to paſs in its entrance through winding cavities made behind the iron back and ſides of the fire-place, and under the iron hearth-plate; in which cavities it will be warmed, and even heated, ſo as to contribute much, inſtead of cooling, to the warming of the room. This invention is excellent in itſelf, and may be uſed with advantage in building new houſes; becauſe the chimneys may then be ſo diſpoſed, as to admit conveniently the cold air to enter ſuch paſſages: But in houſes built without ſuch views, the chimneys are often ſo ſituated, as not to afford that convenience, without great and expenſive alterations. Eaſy and cheap methods, though not quite ſo perfect in themſelves, are of more general utility; and ſuch are the following.

In all rooms where there is a fire, the body of air warmed and rarefied before the chimney is continually changing place, and making room for other air that is to be warmed in its turn. Part of it enters and goes up the chimney, and the reſt riſes and takes place near the ceiling. If the room be lofty, that warm air remains above our heads as long as it continues warm, and we are little benefited by it, becauſe it does not deſcend till it is cooler. Few can imagine the difference of climate between the upper and lower parts of ſuch a room, who have not tried it by the thermometer,

thermometer, or by going up a ladder till their heads are near the ceiling. It is then among this warm air that the wanted quantity of outward air is beſt admitted, with which being mixed, its coldneſs is abated, and its inconvenience diminiſhed ſo as to become ſcarce obſervable. This may be eaſily done, by drawing down about an inch the upper ſaſh of a window; or, if not moveable, by cutting ſuch a crevice through its frame; in both which caſes, it will be well to place a thin ſhelf of the length, to conceal the opening, and ſloping upwards to direct the entering air horizontally along and under the ceiling. In ſome houſes the air may be admitted by ſuch a crevice made in the wainſcot, corniſh or plaſtering, near the ceiling and over the opening of the chimney. This, if practicable, is to be choſen, becauſe the entering cold air will there meet with the warmeſt riſing air from before the fire, and be ſooneſt tempered by the mixture. The ſame kind of ſhelf ſhould alſo be placed here. Another way, and not a very difficult one, is to take out an upper pane of glaſs in one of your ſaſhes, ſet it in a tin frame, giving it two ſpringing angular ſides, and then replacing it, with hinges below on which it may be turned to open more or leſs above. It will then have the appearance of an internal ſky light. By drawing this pane in, more or leſs, you may admit what air you find neceſſary. Its poſition will naturally throw that air up and along the ceiling. This is what is called in France a *Was iſt das?* As this is a German queſtion, the invention is probably of that nation, and takes its name from the frequent aſking of that queſtion when it firſt appeared. In England, ſome have of late years cut a round hole about five inches diameter in a pane of the ſaſh and placed againſt it a circular plate of tin hung on an axis, and cut into vanes, which being ſeparately bent a little obliquely, are acted upon by the entering air, ſo as to force the plate continually round like the vanes of a windmill. This admits

Plate I. Figure 2.

mits the outward air, and by the continual whirling of the vanes, does in ſome degree diſperſe it. The noiſe only, is a little inconvenient.

2. A ſecond cauſe of the ſmoking of chimneys is, *their openings in the room being too large;* that is, too wide, too high or both. Architects in general have no other ideas of proportion in the opening of a chimney, than what relate to ſymmetry and beauty, reſpecting the dimenſions of the room*; while its true proportion, reſpecting its function and utility depends on quite other principles; and they might as properly proportion the ſtep in a ſtair-caſe to the height of the ſtory, inſtead of the natural elevation of men's legs in mounting. The proportion then to be regarded, is what relates to the height of the funnel. For as the funnels in the different ſtories of a houſe are neceſſarily of different heights or lengths, that from the loweſt floor being the higheſt or longeſt, and thoſe of the other floors ſhorter and ſhorter, till we come to thoſe in the garrets, which are of courſe the ſhorteſt; and the force of draft being, as already ſaid, in proportion to the height of funnel filled with rarefied air; and a current of air from the room into the chimney, ſufficient to fill the opening, being neceſſary to oppoſe and prevent the ſmoke coming out into the room; it follows that the openings of the longeſt funnels may be larger, and that thoſe of the ſhorter funnels ſhould be ſmaller. For if there be a large opening to a chimney that does not draw ſtrongly, the funnel may happen to be furniſhed with the air it demands by a partial current entering on one ſide of the opening, and leaving the other ſide free of any oppoſing current, may permit the ſmoke to iſſue there into the room. Much too of the force of draft in a funnel depends on the degree of rarefaction in the air it contains, and that depends on the nearneſs to the fire of its paſſage in entering the funnel. If it can enter far from the fire on each ſide, or far above the fire, in a wide or high opening, it receives little heat in

* See Appendix, N° 1.

in paſſing by the fire, and the contents of the funnel is by that means leſs different in levity from the ſurrounding atmoſphere, and its force in drawing conſequently weaker. Hence if too large an opening be given to chimneys in upper rooms, thoſe rooms will be ſmoky: On the other hand, if too ſmall openings be given to chimneys in the lower rooms, the entering air operating too directly and violently on the fire, and afterwards ſtrengthening the draft as it aſcends the funnel, will conſume the fuel too rapidly.

Remedy. As different circumſtances frequently mix themſelves in theſe matters, it is difficult to give preciſe dimenſions for the openings of all chimneys. Our fathers made them generally much too large; we have leſſened them; but they are often ſtill of greater dimenſion than they ſhould be, the human eye not being eaſily reconciled to ſudden and great changes. If you ſuſpect that your chimney ſmokes from the too great dimenſion of its opening, contract it by placing moveable boards ſo as to lower and narrow it gradually, till you find the ſmoke no longer iſſues into the room. The proportion ſo found will be that which is proper for that chimney, and you may employ the bricklayer or maſon to reduce it accordingly. However, as, in building new houſes, ſomething muſt be ſometimes hazarded, I would make the openings in my lower rooms about thirty inches ſquare and eighteen deep, and thoſe in the upper, only eighteen inches ſquare and not quite ſo deep; the intermediate ones diminiſhing in proportion as the height of funnel diminiſhed. In the larger openings, billets of two feet long, or half the common length of cord-wood, may be burnt conveniently; and for the ſmaller, ſuch wood may be ſawed into thirds. Where coals are the fuel, the grates will be proportioned to the openings. The ſame depth is nearly neceſſary to all, the funnels being all made of a ſize proper to admit a chimney-ſweeper. If in large and elegant rooms cuſtom or fancy ſhould require the appearance of a larger chimney, it may be formed of expenſive marginal decorations, in marble, &c. In time perhaps that which is fitteſt in the nature of things, may come to be thought handſomeſt. But at preſent when men and women in different countries ſhow themſelves diſſatisfied with the forms God has given to their heads, waiſts and feet, and pretend to ſhape them more perfectly, it is hardly to be expected that they will be content always with the beſt form of a chimney. And there are ſome I know ſo bigotted to the fancy of a large noble opening, that rather than change it, they would ſubmit to have damaged furniture, ſore eyes and ſkins almoſt ſmoked to bacon.

3. Another cauſe of ſmoky chimneys is, *too ſhort a funnel.* This happens neceſſarily in ſome caſes, as where a chimney is required in a low building; for, if the funnel be raiſed high above the roof, in order to ſtrengthen its draft, it is then in danger of being blown down, and cruſhing the roof in its fall.

Remedies. Contract the opening of the chimney, ſo as to oblige all the entering air to paſs through or very near the fire; whereby it will be more heated and rarefied, the funnel itſelf be more warmed, and its contents have more of what may be called the force of levity, ſo as to riſe ſtrongly and maintain a good draft at the opening.

Or you may in ſome caſes, to advantage, build additional ſtories over the low building, which will ſupport a high funnel.

If the low building be uſed as a kitchen, and a contraction of the opening therefore inconvenient, a large one being neceſſary, at leaſt when there are great dinners, for the free management of ſo many cooking utenſils; in ſuch caſe I would adviſe the building of two more funnels joining to the firſt, and having three moderate openings, one to each funnel, inſtead of one large one. When there is occaſion to uſe but one, the other two may be kept ſhut by by sliding plates, hereafter to be described*; and two or all of them may be used together when wanted. This will indeed be an expence, but not an useless one, since your cooks will work with more comfort, see better than in a smoky kitchen what they are about, your victuals will be cleaner dressed and not taste of smoke, as is often the case; and to render the effect more certain, a stack of three funnels may be safely built higher above the roof than a single funnel.

The case of too short a funnel is more general than would be imagined, and often found where one would not expect it. For it is not uncommon, in ill-contrived buildings, instead of having a funnel for each room or fireplace, to bend and turn the funnel of an upper room so as to make it enter the side of another funnel that comes from below. By this means the upper room funnel is made short of course, since its length can only be reckoned from the place where it enters the lower room funnel; and that funnel is also shortened by all the distance between the entrance of the second funnel and the top of the stack: For all that part being readily supplied with air through the second funnel, adds no strength to the draft, especially as that air is cold when there is no fire in the second chimney. The only easy remedy here is, to keep the opening shut of that funnel in which there is no fire.

4. Another very common cause of the smoking of chimneys; is, *their overpowering one another*. For instance, if there be two chimneys in one large room, and you make fires in both of them, the doors and windows close shut, you will find that the greater and stronger fire shall overpower the weaker, and draw air down its funnel to supply its own demand; which air descending in the weaker funnel will drive down its smoke, and force it into the room. If, instead of being in one room, the two chimneys are in two different rooms, communicating by a door, the case is the same whenever that door is open. In a very tight

* See Appendix, N° II.

tight houſe, I have known a kitchen chimney on the loweſt floor, when it had a great fire in it, overpower any other chimney in the houſe, and draw air and ſmoke into its room, as often as the door was opened communicating with the ſtaircaſe.

Remedy. Take care that every room have the means of ſupplying itſelf from without, with the air its chimney may require, ſo that no one of them may be obliged to borrow from another, nor under the neceſſity of lending. A variety of theſe means have been already deſcribed.

5. Another cauſe of ſmoking is, *when the tops of chimneys are commanded by higher buildings, or by a hill*, ſo that the wind blowing over ſuch eminences falls like water over a dam, ſometimes almoſt perpendicularly on the tops of the chimneys that lie in its way, and beats down the ſmoke contained in them.

Remedy. That commonly applied to this caſe, is a turncap made of tin or plate iron, covering the chimney above and on three ſides, open on one ſide, turning on a ſpindle, and which being guided or governed by a vane, always preſents its back to the current. This I believe may be generally effectual, though not certain, as there may be caſes in which it will not ſucceed. Raiſing your funnels if practicable, ſo as their tops may be higher, or at leaſt equal with the commanding eminence, is more to be depended on. But the turning cap, being eaſier and cheaper, ſhould firſt be tried. If obliged to build in ſuch a ſituation, I would chuſe to place my doors on the ſide next the hill, and the backs of my chimneys on the furtheſt ſide; for then the column of air falling over the eminence, and of courſe preſſing on that below and forcing it to enter the doors, or *Was-iſt-das*es on that ſide, would tend to balance the preſſure down the chimneys, and leave the funnels more free in the exerciſe of their functions.

6. There is another caſe of command, the reverſe of that laſt mentioned. It is where the commanding eminence

nence is farther from the wind than the chimney commanded. To explain this a figure may be neceſſary. Suppoſe then a building whoſe ſide A, happens to be expoſed to the wind, and forms a kind of dam againſt its progreſs. The air obſtructed by this dam will like water preſs and ſearch for paſſages through it; and finding the top of the chimney B, below the top of the dam, it will force itſelf down that funnel, in order to get through by ſome door or window open on the other ſide of the building. And if there be a fire in ſuch chimney, its ſmoke is of courſe beat down, and fills the room. Plate I. Figure 3.

Remedy. I know of but one, which is to raiſe ſuch funnel higher than the roof, ſupporting it, if neceſſary, by iron bars. For a turn-cap in this caſe has no effect, the dammed up air preſſing down through it in whatever poſition the wind may have placed its opening.

I know a city in which many houſes are rendered ſmoky by this operation. For their kitchens being built behind, and connected by a paſſage with the houſes, and the tops of the kitchen chimneys lower than the top of the houſes, the whole ſide of a ſtreet when the wind blows againſt its back, forms ſuch a dam as above deſcribed; and the wind ſo obſtructed forces down thoſe kitchen chimneys, (eſpecially when they have but weak fires in them) to paſs through the paſſage and houſe, into the ſtreet. Kitchen chimneys ſo formed and ſituated, have another inconvenience. In ſummer, if you open your upper room windows for air, a light breeze blowing over your kitchen chimney towards the houſe, though not ſtrong enough to force down its ſmoke as aforeſaid, is ſufficient to waft it into your windows, and fill the rooms with it; which, beſides the diſagreeableneſs, damages your furniture.

7. Chimneys, otherwiſe drawing well, are ſometimes made to ſmoke by *the improper and inconvenient ſituation of a door.* When the door and chimney are on the ſame ſide of the room as in the figure, if the door A, being in the

Plate I. Figure 4. the corner is made to open againſt the wall, which is common, as being there, when open, more out of the way, it follows, that when the door is only opened in part, a current of air ruſhing in paſſes along the wall into and acroſs the opening of the chimney B, and flirts ſome of the ſmoke out into the room. This happens more certainly when the door is ſhutting, for then the force of the current is augmented, and becomes very inconvenient to thoſe who, warming themſelves by the fire, happen to ſit in its way.

The *Remedies* are obvious and eaſy. Either put an intervening ſkreen from the wall round great part of the fire-place; or, which is perhaps preferable, ſhift the hinges of your door, ſo as it may open the other way, and when open throw the air along the other wall.

8. A room that has no fire in its chimney, is ſometimes filled with *ſmoke which is received at the top of its funnel and deſcends into the room.* In a former paper * I have already explained the deſcending currents of air in cold funnels; it may not be amiſs however to repeat here, that funnels without fires have an effect according to their degree of coldneſs or warmth, on the air that happens to be contained in them. The ſurrounding atmoſphere is frequently changing its temperature; but ſtacks of funnels covered from winds and ſun by the houſe that contains them, retain a more equal temperature. If, after a warm ſeaſon, the outward air ſuddenly grows cold, the empty warm funnels begin to draw ſtrongly upward; that is, they rarefy the air contained in them, which of courſe riſes, cooler air enters below to ſupply its place, is rarefied in its turn and riſes; and this operation continues, till the funnel grows cooler, or the outward air warmer, or both, when the motion ceaſes. On the other hand, if after a cold ſeaſon, the outward air ſuddenly grows warm and of courſe lighter, the air contained in the cool funnels, being heavier, deſcends into the room; and the warmer

* See Appendix, N° II.

warmer air which enters their tops being cooled in its turn, and made heavier, continues to defcend; and this operation goes on, till the funnels are warmed by the paffing of warm air through them, or the air itfelf grows cooler. When the temperature of the air and of the funnels is nearly equal, the difference of warmth in the air between day and night is fufficient to produce thefe currents, the air will begin to afcend the funnels as the cool of the evening comes on, and this current will continue till perhaps nine or ten o'clock the next morning, when it begins to hefitate; and as the heat of the day approaches, it fets downwards, and continues fo till towards evening, when it again hefitates for fome time, and then goes upwards conftantly during the night, as before mentioned. Now when fmoke iffuing from the tops of neighbouring funnels paffes over the tops of funnels which are at the time drawing downwards, as they often are in the middle part of the day, fuch fmoke is of neceffity drawn into thefe funnels, and defcends with the air into the chamber.

The *Remedy* is to have a fliding plate, hereafter defcribed*, that will fhut perfectly the offending funnel.

9. Chimneys which generally draw well, do neverthelefs fometimes give fmoke into the rooms, *it being driven down by ftrong winds paffing over the tops of their funnels*, though not defcending from any commanding eminence. This cafe is moft frequent where the funnel is fhort, and the opening turned from the wind. It is the more grievous, when it happens to be a cold wind that produces the effect, becaufe when you moft want your fire, you are fometimes obliged to extinguifh it. To underftand this, it may be confidered that the rifing light air, to obtain a free iffue from the funnel, muft pufh out of its way or oblige the air that is over it to rife. In a time of calm or of little wind this is done vifibly, for we fee the fmoke that is brought up by that air rife in a column above the chimney. But when a violent current of air, that is, a

C ftrong

* See Appendix, N° II.

ſtrong wind, paſſes over the top of a chimney, its particles have received ſo much force, which keeps them in a horizontal direction and follow each other ſo rapidly, that the riſing light air has not ſtrength ſufficient to oblige them to quit that direction and move upwards to permit its iſſue. Add to this, that ſome of the current paſſing over that ſide of the funnel which it firſt meets with, viz. at A, having been compreſſed by the reſiſtance of the funnel, may expand itſelf over the flue, and ſtrike the interior oppoſite ſide at B, from whence it may be reflected downwards and from ſide to ſide in the direction of the pricked lines c c c.

Plate I. Figure 5.

Remedies. In ſome places, particularly in Venice, where they have not ſtacks of chimneys but ſingle flues, the cuſtom is, to open or widen the top of the flue rounding in the true form of a funnel; which ſome think may prevent the effect juſt mentioned, for that the wind blowing over one of the edges into the funnel may be ſlanted out again on the other ſide by its form. I have had no experience of this; but I have lived in a windy country, where the contrary is practiſed, the tops of the flues being *narrowed* inwards, ſo as to form a ſlit for the iſſue of the ſmoke, long as the breadth of the funnel, and only four inches wide. This ſeems to have been contrived on a ſuppoſition that the entry of the wind would thereby be obſtructed, and perhaps it might have been imagined, that the whole force of the riſing warm air being condenſed, as it were, in the narrow opening, would thereby be ſtrengthened, ſo as to overcome the reſiſtance of the wind. This however did not always ſucceed; for when the wind was at north-eaſt and blew freſh, the ſmoke was forced down by fits into the room I commonly ſat in, ſo as to oblige me to ſhift the fire into another. The poſition of the ſlit of this funnel was indeed north-eaſt and ſouth-weſt. Perhaps if it had lain acroſs the wind, the effect might have been different. But on this I can give no certainty,

Plate I. Figure 6.

tainty. It ſeems a matter proper to be referred to experiment. Poſſibly a turn-cap might have been ſerviceable, but it was not tried.

Chimneys have not been long in uſe in England. I formerly ſaw a book printed in the time of queen Elizabeth, which remarked the then modern improvements of living, and mentioned among others the convenience of chimneys. " Our forefathers," ſaid the author, " had no chimneys. " There was in each dwelling houſe only one place for a " fire, and the ſmoke went out through a hole in the " roof; but now there is ſcarce a gentleman's houſe in " England that has not at leaſt one chimney in it."— When there was but one chimney, its top might then be opened as a funnel, and perhaps, borrowing the form from the Venetians, it was then the flue of a chimney got that name. Such is now the growth of luxury, that in both England and France we muſt have a chimney for every room, and in ſome houſes every poſſeſſor of a chamber, and almoſt every ſervant, will have a fire; ſo that the flues being neceſſarily built in ſtacks, the opening of each as a funnel is impracticable. This change of manners ſoon conſumed the firewood of England, and will ſoon render fuel extremely ſcarce and dear in France, if the uſe of coals be not introduced in the latter, kingdom as it has been in the former, where it at firſt met with oppoſition; for there is extant in the records of one of queen Elizabeth's parliaments, a motion made by a member, reciting, " that " many dyers, brewers, ſmiths, and other artificers of Lon- " don, had of late taken to the uſe of pitcoal for their fires, " inſtead of wood, which filled the air with noxious va- " pours and ſmoke, very prejudicial to the health, parti- " cularly of perſons coming out of the country; and there- " fore moving that a law might paſs to prohibit the uſe " of ſuch fuel (at leaſt during the ſeſſion of parliament) " by thoſe artificers."—It ſeems it was not then commonly uſed in private houſes. Its ſuppoſed unwholeſomeneſs was an objection. Luckily the inhabitants of London have got over that objection, and now think it rather contributes to render their air ſalubrious, as they have had no general peſtilential diſorder ſince the general uſe of coals, when, before it, ſuch were frequent. Paris ſtill burns wood at an enormous expence continually augmenting, the inhabitants having ſtill that prejudice to overcome. In Germany you are happy in the uſe of ſtoves, which ſave fuel wonderfully: Your people are very ingenious in the management of fire; but they may ſtill learn ſomething in that art from the Chineſe*, whoſe country being greatly populous and fully cultivated, has little room left for the growth of wood, and having not much other fuel that is good, have been forced upon many inventions during a courſe of ages, for making a little fire go as far as poſſible.

I have thus gone through all the common cauſes of the ſmoking of chimneys that I can at preſent recollect as having fallen under my obſervation; communicating the remedies that I have known ſucceſsfully uſed for the different caſes, together with the principles on which both the diſeaſe and the remedy depend, and confeſſing my ignorance wherever I have been ſenſible of it. You will do well, if you publiſh, as you propoſe, this letter, to add in notes, or as you pleaſe, ſuch obſervations as may have occurred to your attentive mind; and if other philoſophers will do the ſame, this part of ſcience, though humble, yet of great utility, may in time be perfected. For many years paſt, I have rarely met with a caſe of a ſmoky chimney, which has not been ſolvable on theſe principles, and cured by theſe remedies, where people have been willing to apply them; which is indeed not always the caſe; for many have prejudices in favour of the noſtrums of pretending chimney-doctors and fumiſts, and ſome have conceits and fancies of their own, which they rather chuſe to try, than to lengthen a funnel, alter the ſize of an opening, or admit air into a room, however neceſſary; for ſome are as much

* See Appendix, N° III.

much afraid of fresh air as persons in the hydrophobia are of fresh water. I myself had formerly this prejudice, this *aerophobia*, as I now account it, and dreading the supposed dangerous effects of cool air, I considered it as an enemy, and closed with extreme care every crevice in the rooms I inhabited. Experience has convinced me of my error. I now look upon fresh air as a friend: I even sleep with an open window. I am persuaded that no common air from without, is so unwholesome as the air within a close room that has been often breathed and not changed. Moist air too, which formerly I thought pernicious, gives me now no apprehensions: For considering that no dampness of air applied to the outside of my skin, can be equal to what is applied to and touches it within, my whole body being full of moisture, and finding that I can lie two hours in a bath twice a week, covered with water, which certainly is much damper than any air can be, and this for years together, without catching cold, or being in any other manner disordered by it, I no longer dread mere moisture, either in air or in sheets or shirts: And I find it of importance to the happiness of life, the being freed from vain terrors, especially of objects that we are every day exposed inevitably to meet with. You physicians have of late happily discovered, after a contrary opinion had prevailed some ages, that fresh and cool air does good to persons in the small pox and other fevers. It is to be hoped that in another century or two we may all find out, that it is not bad even for people in health. And as to moist air, here I am at this present writing in a ship with above forty persons, who have had no other but moist air to breathe for six weeks past; every thing we touch is damp, and nothing dries, yet we are all as healthy as we should be on the mountains of Switzerland, whose inhabitants are not more so than those of Bermuda or St. Helena, islands on whose rocks the waves are dashed into millions of particles, which fill the air with damp, but produce no dif-

eases

eafes, the moifture being pure, unmixed with the poifonous vapours arifing from putrid marfhes and ftagnant pools, in which many infects die and corrupt the water. Thefe places only, in my opinion, (which however I fubmit to yours) afford unwholefome air; and that it is not the mere water contained in damp air, but the volatile particles of corrupted animal matter mixed with that water, which renders fuch air pernicious to thofe who breathe it. And I imagine it a caufe of the fame kind that renders the air in clofe rooms, where the perfpirable matter is breathed over and over again by a number of affembled people, fo hurtful to health. After being in fuch a fituation, many find themfelves affected by that *febricula*, which the Englifh alone call *a cold*, and, perhaps from the name, imagine that they caught the malady by *going out* of the room, when it was in fact by being in it.

You begin to think that I wander from my fubject, and go out of my depth. So I return again to my chimneys.

We have of late many lecturers in experimental philofophy. I have wifhed that fome of them would ftudy this branch of that fcience, and give experiments in it as a part of their lectures. The addition to their prefent apparatus need not be very expenfive. A number of little reprefentations of rooms compofed each of five panes of fafh glafs, framed in wood at the corners, with proportionable doors, and moveable glafs chimneys, with openings of different fizes, and different lengths of funnel, and fome of the rooms fo contrived as to communicate on occafion with others, fo as to form different combinations, and exemplify different cafes; with quantities of green wax taper cut into pieces of an inch and half, fixteen of which ftuck together in a fquare, and lit, would make a ftrong fire for a little glafs chimney, and blown out would continue to burn and give fmoke as long as defired. With fuch an apparatus all the operations of fmoke and rarefied air in rooms and chimneys might be feen through their tranfparent

rent ſides; and the effect of winds on chimneys, commanded or otherwiſe, might be ſhown by letting the entering air blow upon them through an opened window of the lecturer's chamber, where it would be conſtant while he kept a good fire in his chimney. By the help of ſuch lectures our fumiſts would become better inſtructed. At preſent they have generally but one remedy, which perhaps they have known effectual in ſome one caſe of ſmoky chimneys, and they apply that indiſcriminately to all the other caſes, without ſucceſs,—but not without expence to their employers.

With all the ſcience, however, that a man ſhall ſuppoſe himſelf poſſeſſed of in this article, he may ſometimes meet with caſes that ſhall puzzle him. I once lodged in a houſe at London, which, in a little room, had a ſingle chimney and funnel. The opening was very ſmall, yet it did not keep in the ſmoke, and all attempts to have a fire in this room were fruitleſs. I could not imagine the reaſon, till at length obſerving that the chamber over it, which had no fire-place in it, was always filled with ſmoke when a fire was kindled below, and that the ſmoke came through the cracks and crevices of the wainſcot; I had the wainſcot taken down, and diſcovered that the funnel which went up behind it, had a crack many feet in length, and wide enough to admit my arm, a breach very dangerous with regard to fire, and occaſioned probably by an apparent irregular ſettling of one ſide of the houſe. The air entering this breach freely, deſtroyed the drawing force of the funnel. The remedy would have been, filling up the breach or rather rebuilding the funnel: But the landlord rather choſe to ſtop up the chimney.

Another puzzling caſe I met with at a friend's country houſe near London. His beſt room had a chimney in which, he told me, he never could have a fire, for all the ſmoke came out into the room. I flattered myſelf I could eaſily find the cauſe, and preſcribe the cure. I had a fire

made

made there, and found it as he ſaid. I opened the door, and perceived it was not want of air. I made a temporary contraction of the opening of the chimney, and found that it was not its being too large that cauſed the ſmoke to iſſue. I went out and looked up at the top of the chimney: Its funnel was joined in the ſame ſtack with others, ſome of them ſhorter, that drew very well, and I ſaw nothing to prevent its doing the ſame. In fine, after every other examination I could think of, I was obliged to own the inſufficiency of my ſkill. But my friend, who made no pretenſion to ſuch kind of knowledge, afterwards diſcovered the cauſe himſelf. He got to the top of the funnel by a ladder, and looking down, found it filled with twiggs and ſtraw cemented by earth, and lined with feathers. It ſeems the houſe, after being built, had ſtood empty ſome years before he occupied it; and he concluded that ſome large birds had taken the advantage of its retired ſituation to make their neſt there. The rubbiſh, conſiderable in quantity, being removed, and the funnel cleared, the chimney drew well, and gave ſatisfaction.

In general, ſmoke is a very tractable thing, eaſily governed and directed when one knows the principles, and is well informed of the circumſtances. You know I made it *deſcend* in my Pennſylvania ſtove. I formerly had a more ſimple conſtruction, in which the ſame effect was produced, but viſible to the eye. It was compoſed of two plates A B and C D, placed as in the figure. The lower plate A B reſted with its edge in the angle made by the hearth with the back of the chimney. The upper plate was fixed to the breaſt, and lapt over the lower about ſix inches, leaving a ſpace of four inches wide and the length of the plates (near two feet) between them. Every other paſſage of air into the funnel was well ſtopped. When therefore a fire was made at E, for the firſt time with charcoal, till the air in the funnel was a little heated through the plates, and then wood

Plate I. Figure 7.

wood laid on, the ſmoke would riſe to A, turn over the edge of that plate, deſcend to D, then turn under the edge of the upper plate, and go up the chimney. It was pretty to ſee, but of no great uſe. Placing therefore the under plate in a higher ſituation, I removed the upper plate C D, and placed it perpendicularly, ſo that the upper edge of the lower plate A B came within about three inches of it, and might be puſhed farther from it, or ſuffered to come nearer to it by a moveable wedge between them. The flame then aſcending from the fire at E, was carried to ſtrike the upper plate, made it very hot, and its heat roſe and ſpread with the rarefied air into the room.

Plate I. Figure 8.

I believe you have ſeen in uſe with me, the contrivance of a ſliding-plate over the fire, ſeemingly placed to oppoſe the riſing of the ſmoke, leaving but a ſmall paſſage for it, between the edge of the plate and the back of the chimney. It is particularly deſcribed, and its uſes explained, in my former printed letter, and I mention it here only as another inſtance of the tractability of ſmoke*.

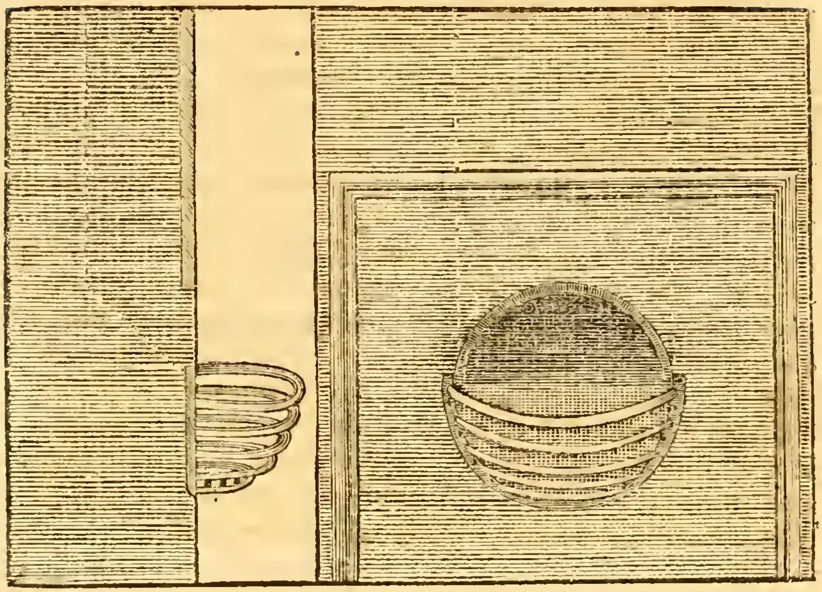
What is called the Staffordſhire chimney, affords an example of the ſame kind. The opening of the chimney is bricked up, even with the fore-edge of its jams, leaving open only a paſſage over the grate of the ſame width, and perhaps eight inches high. The grate conſiſts of ſemicircular bars, their upper bar of the greateſt diameter, the others under it ſmaller and ſmaller, ſo that it has the appearance of half a round baſket. It is, with the coals it contains, wholly without the wall that ſhuts up the chimney; yet the ſmoke bends and enters the paſſage above it, the draft being ſtrong, becauſe no air can enter that is not obliged to paſs near or through the fire, ſo that all that the funnel is filled with is much heated, and of courſe much rarefied.

D Much

* See Appendix, N° II.

STAFFORDSHIRE FIRE-PLACE.

SIDE VIEW. FRONT VIEW.



Much more of the profperity of a winter country depends on the plenty and cheapnefs of fuel, than is generally imagined. In travelling I have obferved, that in thofe parts where the inhabitants can have neither wood nor coal nor turff but at exceffive prices, the working people live in miferable hovels, are ragged, and have nothing comfortable about them. But where fuel is cheap, (or where they have the art of managing it to advantage) they are well furnifhed with neceffaries, and have decent habitations. The obvious reafon is, that the working hours of fuch people are the profitable hours, and they who cannot afford fufficient fuel have fewer fuch hours in the twenty four, than thofe who have it cheap and plenty: For much of the domeftic work of poor women, fuch as fpinning, ſpinning, ſewing, knitting; and of the men in thoſe manufactures that require little bodily exerciſe, cannot well be performed where the fingers are numbed with cold: Thoſe people, therefore, in cold weather, are induced to go to bed ſooner, and lie longer in a morning, than they would do if they could have good fires or warm ſtoves to ſit by; and their hours of work are not ſufficient to produce the means of comfortable ſubſiſtence. Thoſe public works, therefore, ſuch as roads, canals, &c. by which fuel may be brought cheap into ſuch countries from diſtant places, are of great utility; and thoſe who promote them may be reckoned among the benefactors of mankind.

I have great pleaſure in having thus complied with your requeſt, and in the reflection that the friendſhip you honour me with, and in which I have ever been ſo happy, has continued ſo many years without the ſmalleſt interruption. Our diſtance from each other is now augmented, and nature muſt ſoon put an end to the poſſibility of my continuing our correſpondence: But if conſciouſneſs and memory remain in a future ſtate, my eſteem and reſpect for you, my dear friend, will be everlaſting.

B. F.

APPENDIX.

NOTES FOR THE LETTER UPON CHIMNEYS.

N° I.

THE lateſt work on architecture that I have ſeen, is that entitled NUTSHELLS, which appears to be written by a very ingenious man, and contains a table of the proportions of the openings of chimneys; but they relate ſolely to the proportions he gives his rooms, without the ſmalleſt regard to the funnels. And he remarks, reſpecting thoſe proportions, that they are ſimilar to the harmonic diviſions of a monochord*. He does not indeed lay much ſtreſs on this; but it ſhows that we like the appearance of principles; and where we have not true ones, we have ſome ſatisfaction in producing ſuch as are imaginary.

N° II.

THE deſcription of the ſliding plates here promiſed, and which hath been ſince brought into uſe under various names, with ſome immaterial changes, is contained in a former letter to J. B. Eſq. as follows:

To J. B. *Eſq. at Boſton, in New-England.*

Dear Sir, London, Dec. 2, 1758.

I HAVE executed here an eaſy ſimple contrivance, that I have long ſince had in ſpeculation, for keeping rooms warmer

* "It may be juſt remarked here, that upon comparing theſe proportions with thoſe ariſing "from the common diviſions of the monochord, it happens that the firſt anſwers to uniſon, "and although the ſecond is a diſcord, the third anſwers to the third minor, the fourth to the "third major, the fifth to the fourth, the ſixth to the fifth, and the ſeventh to the octave." NUTSHELLS, page 85.

warmer in cold weather than they generally are, and with lefs fire. It is this. The opening of the chimney is contracted, by brick-work faced with marble flabs, to about two feet between the jams, and the breaft brought down to within about three feet of the hearth.—An iron frame is placed juft under the breaft, and extending quite to the back of the chimney, fo that a plate of the fame metal may flide horizontally backwards and forwards in the grooves on each fide of the frame. This plate is juft fo large as to fill the whole fpace, and fhut the chimney entirely when thruft quite in, which is convenient when there is no fire. Drawing it out, fo as to leave a fpace between its further edge and the back, of about two inches; this fpace is fufficient for the fmoke to pafs; and fo large a part of the funnel being ftopt by the reft of the plate, the paffage of warm air out of the room, up the chimney, is obftructed and retarded, and by that means much cold air is prevented from coming in through crevices, to fupply its place. This effect is made manifeft three ways. Firft, when the fire burns brifkly in cold weather, the howling or whiftling noife made by the wind, as it enters the room through the crevices, when the chimney is open as ufual, ceafes as foon as the plate is flid in to its proper diftance. Secondly, opening the door of the room about half an inch, and holding your hand againft the opening, near the top of the door, you feel the cold air coming in againft your hand, but weakly, if the plate be in. Let another perfon fuddenly draw it out, fo as to let the air of the room go up the chimney, with its ufual freedom where chimneys are open, and you immediately feel the cold air rufhing in ftrongly. Thirdly, if fomething be fet againft the door, juft fufficient, when the plate is in, to keep the door nearly fhut, by refifting the preffure of the air that would force it open: Then, when the plate is drawn out, the door will be forced open by the increafed preffure of the outward cold air endeavouring to get in to fupply the place of the

warm

warm air, that now paſſes out of the room to go up the chimney. In our common open chimneys, half the fuel is waſted, and its effect loſt; the air it has warmed being immediately drawn off. Several of my acquaintance having ſeen this ſimple machine in my room, have imitated it at their own houſes, and it ſeems likely to become pretty common. I deſcribe it thus particularly to you, becauſe I think it would be uſeful in *Boſton*, where firing is often dear.

Mentioning chimneys puts me in mind of a property I formerly had occaſion to obſerve in them, which I have not found taken notice of by others; it is, that in the ſummer time, when no fire is made in the chimneys, there is, nevertheleſs, a regular draft of air through them; continually paſſing upwards, from about five or ſix o'clock in the afternoon, till eight or nine o'clock the next morning, when the current begins to ſlacken and heſitate a little, for about half an hour, and then ſets as ſtrongly down again, which it continues to do till towards five in the afternoon, then ſlackens and heſitates as before, going ſometimes a little up, then a little down, till in about a half an hour it gets into a ſteady upward current for the night, which continues till eight or nine the next day; the hours varying a little as the days lengthen and ſhorten, and ſometimes varying from ſudden changes in the weather; as if, after being long warm, it ſhould begin to grow cool about noon, while the air was coming down the chimney, the current will then change earlier than the uſual hour, *&c.*

This property in chimneys I imagine we might turn to ſome account, and render improper, for the future, the old ſaying, *as uſeleſs as a chimney in ſummer.* If the opening of the chimney, from the breaſt down to the hearth, be cloſed by a ſlight moveable frame or two, in the manner of doors, covered with canvas, that will let the air through, but keep out the flies; and another little frame ſet within upon the hearth, with hooks on which to hang joints of meat, fowls, *&c.* wrapt well in wet linen cloths, three or four

four fold, I am confident that if the linen is kept wet, by ſprinkling it once a day, the meat would be ſo cooled by the evaporation, carried on continually by means of the paſſing air, that it would keep a week or more in the hotteſt weather. Butter and milk might likewiſe be kept cool, in veſſels or bottles covered with wet cloths. A ſhallow tray, or keeler, ſhould be under the frame to receive any water that might drip from the wetted cloths. I think, too, that this property of chimneys might, by means of ſmoke-jack vanes, be applied to ſome mechanical purpoſes, where a ſmall but pretty conſtant power only is wanted.

If you would have my opinion of the cauſe of this changing current of air in chimneys, it is, in ſhort, as follows. In ſummer time there is generally a great difference in the warmth of the air at mid-day and midnight, and, of courſe, a difference of ſpecific gravity in the air, as the more it is warmed the more it is rarefied. The funnel of a chimney being for the moſt part ſurrounded by the houſe, is protected, in a great meaſure, from the direct action of the ſun's rays, and alſo from the coldneſs of the night air. It thence preſerves a middle temperature between the heat of the day, and the coldneſs of the night. This middle temperature it communicates to the air contained in it. If the ſtate of the outward air be cooler than that in the funnel of the chimney, it will, by being heavier, force it to riſe, and go out at the top. What ſupplies its place from below, being warmed, in its turn, by the warmer funnel, is likewiſe forced up by the colder and weightier air below, and ſo the current is continued till the next day, when the ſun gradually changes the ſtate of the outward air, makes it firſt as warm as the funnel of the chimney can make it, (when the current begins to heſitate) and afterwards warmer. Then the funnel being cooler than the air that comes into it, cools that air, makes it heavier than the outward air, of courſe it deſcends; and what ſucceeds it from above, being cooled in its turn, the deſcending current continues

tinues till towards evening, when it again heſitates and changes its courſe, from the change of warmth in the outward air, and the nearly remaining ſame middle temperature in the funnel.

Upon this principle, if a houſe were built behind *Beacon-hill*, an adit carried from one of the doors into the hill horizontally, till it met with a perpendicular ſhaft ſunk from its top, it ſeems probable to me, that thoſe who lived in the houſe, would conſtantly, in the heat even of the calmeſt day, have as much cool air paſſing through the houſe, as they ſhould chuſe; and the ſame, though reverſed in its current, during the ſtilleſt night.

I think, too, this property might be made of uſe to miners; as where ſeveral ſhafts or pits are ſunk perpendicularly into the earth, communicating at bottom by horizontal paſſages, which is a common caſe, if a chimney of thirty or forty feet high were built over one of the ſhafts, or ſo near the ſhaft, that the chimney might communicate with the top of the ſhaft, all air being excluded but what ſhould paſs up or down by the ſhaft, a conſtant change of air would, by this means, be produced in the paſſages below, tending to ſecure the workmen from thoſe damps which ſo frequently incommode them. For the freſh air would be almoſt always going down the open ſhaft, to go up the chimney, or down the chimney to go up the ſhaft. Let me add one obſervation more, which is, that if that part of the funnel of a chimney, which appears above the roof of a houſe, be pretty long, and have three of its ſides expoſed to the heat of the ſun ſucceſſively, viz. when he is in the eaſt, in the ſouth, and in the weſt, while the north ſide is ſheltered by the building from the cool northerly winds; ſuch a chimney will often be ſo heated by the ſun, as to continue the draft ſtrongly upwards, through the whole twenty four hours, and often for many days together. If the outſide of ſuch a chimney be painted black, the effect will be ſtill greater, and the current ſtronger.

N° III.

N° III.

IT is ſaid the northern Chineſe have a method of warming their ground floors, which is ingenious. Thoſe floors are made of tile a foot ſquare and two inches thick, their corners being ſupported by bricks ſet on end, that are a foot long and four inches ſquare, the tiles, too, join into each other, by ridges and hollows along their ſides. This forms a hollow under the whole floor, which on one ſide of the houſe has an opening into the air, where a fire is made, and it has a funnel riſing from the other ſide to carry off the ſmoke. The fuel is a ſulphurous pitcoal, the ſmell of which in the room is thus avoided, while the floor and of courſe the room is well warmed. But as the underſide of the floor muſt grow foul with ſoot, and a thick coat of ſoot prevents much of the direct application of the hot air to the tiles, I conceive that burning the ſmoke by obliging it to deſcend through red coals, would in this conſtruction be very advantageous, as more heat would be given by the flame than by the ſmoke, and the floor being thereby kept free from ſoot would be more heated with leſs fire. For this purpoſe I would propoſe erecting the funnel cloſe to the grate, ſo as to have only an iron plate between the fire and the funnel, through which plate the air in the funnel being heated, it will be ſure to draw well, and force the ſmoke to deſcend, as in the figure
Plate I. Figure 9. where A is the funnel or chimney, B the grate on which the fire is placed, C one of the apertures through which the deſcending ſmoke is drawn into the channel D of figure 10, along which channel it is conveyed by a circuitous rout, as deſignated by the arrows, until it arrives at the ſmall aperture E, figure 10, through which it enters the funnel F. G in both figures is the iron plate againſt which the fire is made, which being heated thereby, will rarefy the air in that part of the funnel, and cauſe the ſmoke to aſcend rapidly. The flame thus dividing

E from

from the grate to the right and left, and turning in paſſages diſpoſed, as in figure 13, ſo as that every part of the floor may be viſited by it before it enters the funnel F, by the two paſſages E E, very little of the heat will be loſt, and a winter room thus rendered very comfortable.

N° IV.

PAGE 8. *Few can imagine*, &c. It is ſaid the Icelanders have very little fuel, chiefly drift wood that comes upon their coaſt. To receive more advantage from its heat, they make their doors low, and have a ſtage round the room above the door, like a gallery, wherein the women can ſit and work, the men read or write, &c. The roof being tight, the warm air is confined by it and kept from riſing higher and eſcaping; and the cold air which enters the houſe when the door is opened, cannot riſe above the level of the top of the door, becauſe it is heavier than the warm air above the door, and ſo thoſe in the gallery are not incommoded by it. Some of our too lofty rooms might have a ſtage ſo conſtructed as to make a temporary gallery above, for the winter, to be taken away in ſummer. Sedentary people would find much comfort there in cold weather.

N° V.

PAGE 26. *Where they have the art of managing it*, &c. In ſome houſes of the lower people among the northern nations of Europe, and among the poorer ſort of Germans in Pennſylvania, I have obſerved this conſtruction, which appears very advantageous. A is the kitchen with its chimney; B an iron ſtove in the ſtove-room. In a corner of the chimney is a hole through the back into the ſtove, to put in fuel, and another hole above it to let the ſmoke of the ſtove come back into the chimney. As ſoon as the cooking is over, the brands in the kitchen chimney are put

Plate I. Figure II.

put through the hole to ſupply the ſtove, ſo that there is ſeldom more than one fire burning at a time. In the floor over the ſtove-room, is a ſmall trap door, to let the warm air riſe occaſionally into the chamber. Thus the whole houſe is warmed at little expence of wood, and the ſtove-room kept conſtantly warm; ſo that in the coldeſt winter nights, they can work late, and find the room ſtill comfortable when they riſe to work early. An Engliſh farmer in America who makes great fires in large open chimneys, needs the conſtant employment of one man to cut and haul wood for ſupplying them; and the draft of cold air to them is ſo ſtrong, that the heels of his family are frozen while they are ſcorching their faces, and the room is never warm, ſo that little ſedentary work can be done by them in winter. The difference in this article alone of œconomy, ſhall, in a courſe of years, enable the German to buy out the Engliſhman, and take poſſeſſion of his plantation.

MISCELLANEOUS OBSERVATIONS.

CHIMNEYS whoſe funnels go up in the north wall of a houſe and are expoſed to the north winds, are not ſo apt to draw well as thoſe in a ſouth wall; becauſe when rendered cold by thoſe winds, they draw downwards.

Chimneys encloſed in the body of a houſe are better than thoſe whoſe funnels are expoſed in cold walls.

Chimneys in ſtacks are apt to draw better than ſeparate funnels, becauſe the funnels that have conſtant fires in them, warm the others in ſome degree that have none.

One of the funnels in a houſe I once occupied, had a particular funnel joined to the ſouth ſide of the ſtack, ſo that three of its ſides were expoſed to the ſun in the courſe of the day, viz. the eaſt ſide E during the morning, the ſouth ſide S in the middle part of the day, and the weſt ſide W during the afternoon, while its north ſide was ſhelter-

Plate I. Figure 12.

ed by the ſtack from the cold winds. This funnel, which came from the ground floor, and had a conſiderable height above the roof, was conſtantly in a ſtrong drawing ſtate day and night, winter and ſummer.

Blacking of funnels expoſed to the ſun, would probably make them draw ſtill ſtronger.

In Paris I ſaw a fire-place ſo ingeniouſly contrived as to ſerve conveniently two rooms, a bedchamber and a ſtudy. The funnel over the fire was round. The fire-place was of caſt iron, having an upright back A, and two horizontal ſemicircular plates B C, the whole ſo ordered as to turn on the pivots D E. The plate B always ſtopped that part of the round funnel that was next to the room without fire, while the other half of the funnel over the fire was always open. By this means a ſervant in the morning could make a fire on the hearth C, then in the ſtudy, without diſturbing the maſter by going into his chamber; and the maſter when he roſe, could with a touch of his foot turn the chimney on its pivots, and bring the fire into his chamber, keep it there as long as he wanted it, and turn it again when he went out into his ſtudy. The room which had no fire in it, was alſo warmed by the heat coming through the back plate, and ſpreading in the room as it could not go up the chimney.

Plate I. Figure 13.

Explanation

Plate I.

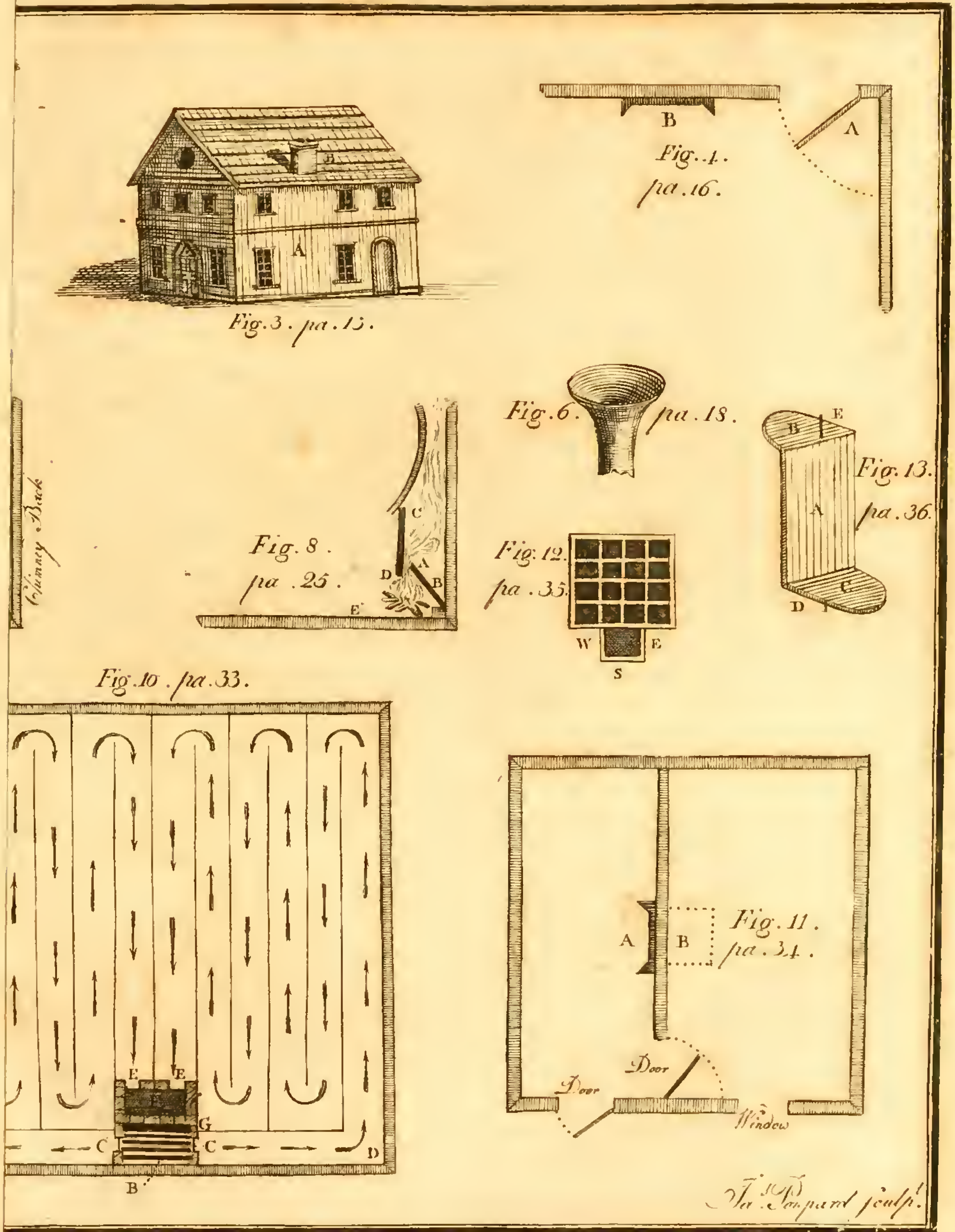


Fig. 3. pa. 15.
A
B
Fig. 4. pa. 16.
Fig. 6. pa. 18.
Fig. 8. pa. 25.
Chimney Back
Fig. 12. pa. 35.
W E S
Fig. 13. pa. 36.
Fig. 10. pa. 33.
C D E F G
Fig. 11. pa. 34.
Door
Door
Window
Ja. Poupard sculp.

Plate I.

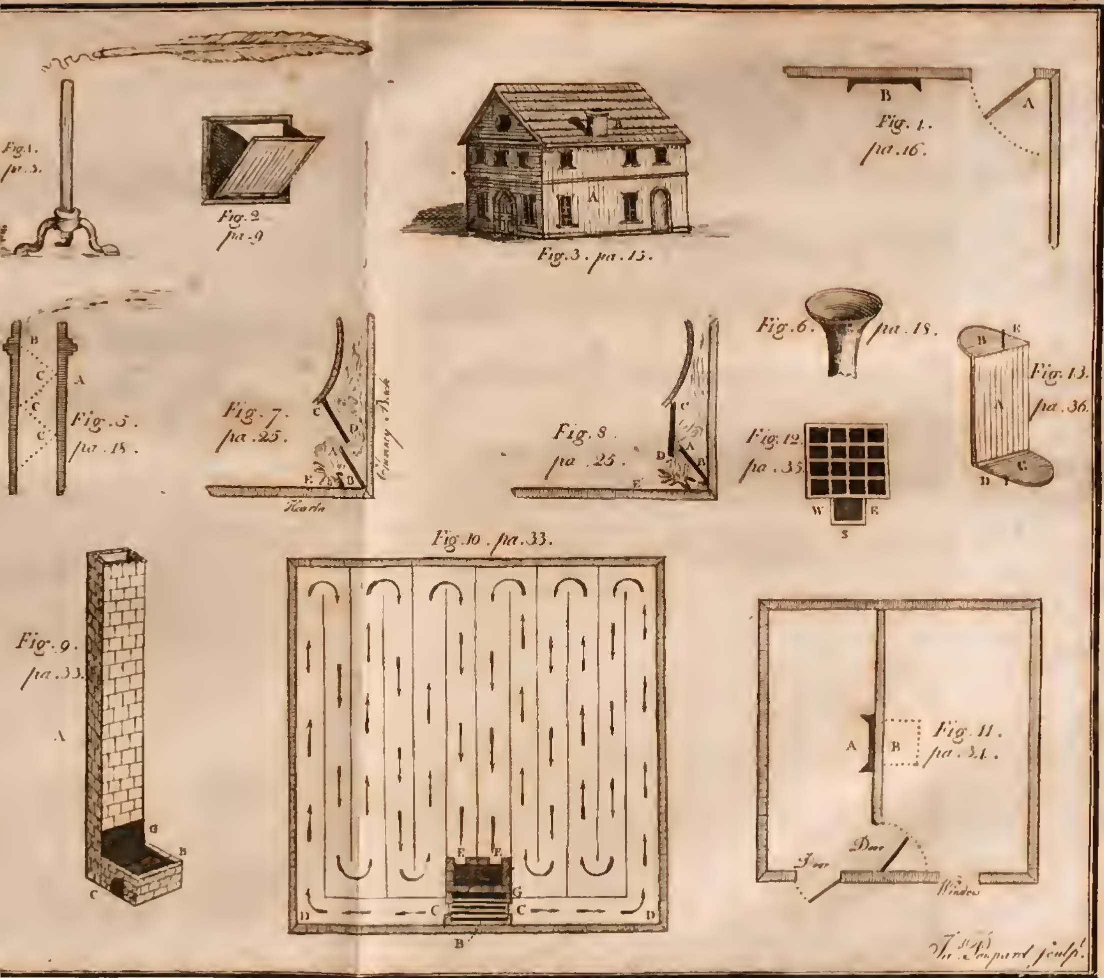


Fig. 1. pa. 16.
Fig. 2 pa. 9
Fig. 3. pa. 15.
Fig. 5. pa. 18.
Fig. 6. pa. 18.
Fig. 7. pa. 25.
Fig. 8. pa. 25.
Fig. 9. pa. 33.
Fig. 10. pa. 33.
Fig. 11. pa. 34.
Fig. 12. pa. 35.
Fig. 13. pa. 36.
Hearth
Door
Door
Window

N° II.

Explanation of an Optical Deception.

BY D. RITTENHOUSE.

Read March 3, 1780.

SOME experiments were long ago communicated to the Royal Society of London, ſhewing, that through the double microſcope, the ſurfaces of bodies ſometimes appear to be reverſed, that is, thoſe parts which are elevated ſeem depreſſed, and the contrary. But the cauſe of this appearance, for any thing I know, remains ſtill to be explained.

In order to produce this effect, no other apparatus is neceſſary than two convex lenſes placed in a tube, at a diſtance from each other nearly equal to the ſum of their focal diſtances. Through theſe glaſſes, objects that appear diſtinctly, always appear inverted; for they are not ſeen directly, but by means of an image formed either between the two glaſſes, or between both of them and the eye.

If we look through ſuch glaſſes at corniſhes, picture frames and other mouldings in carpenters work, and ſome ſorts of carved work, thoſe parts which are raiſed generally appear depreſſed, and thoſe parts which are depreſſed appear raiſed. But a very ready object, and which ſucceeds as well as any thing I know of, is a brick pavement; whether it be a chimney-hearth, or pavement out of doors. Viewed through the tube above deſcribed, every little cavity in the bricks, and the chinks between them, almoſt always appear to be ſo many elevations above the ſurface of the bricks.

When I conſidered this odd appearance, the firſt probable cauſe that offered was, that thoſe parts of the object which are ſunk, and fartheſt from the eye, might have their correſpondent parts of the image formed by the glaſſes neareſt to the eye, and therefore would appear raiſed.

But

But this is not the cafe; for thofe parts which are fartheft from the eye in the object, will always be fartheft from the eye in the image, and often in a much greater proportion. After fome time I concluded it to be a neceffary confequence of the apparent inverfion of the object; and many things tended to confirm me in this opinion, before I made the experiments which feem perfectly decifive.

It has often been matter of furprize to me, when viewing the moon through a good telefcope, in company with perfons not accuftomed to fuch obfervations, that whilft the cavities and eminences of the moon's furface appeared to me marked out with the utmoft certainty by their light and fhades, my companions generally conceived it to be a plain furface of various degrees of brightnefs. The reafon I fuppofe to be this; the aftronomer knows from the moon's fituation with refpect to the fun, and even from the figure of its enlightened part, precifely in what direction the light falls on its furface, and therefore judges rightly of its hills and vallies, from their different degrees of light, according to thofe rules which are imperceptibly formed in the mind, and confirmed by long experience. But a perfon unacquainted with aftronomy knows nothing of the direction of the fun's light on the moon, nor does he attend to the moon's globular figure, and is befides, perhaps, poffeffed with a notion of its being felf-luminous; no wonder then that the fame object has a very different effect on his imagination. It feems to be thofe rules of judging, which we begin to form in our earlieft infancy, which we fet afide, re-eftablifh, alter, correct and confirm, and at length rely on with the utmoft confidence, even without knowing that we do fo, or that we have any fuch rules: It is thefe rules, of fuch infinite general ufe to us, that fometimes miflead us on new and extraordinary occafions, and particularly in the cafe now before us. A perfon entering into a room perceives, at a fingle glance, whence the light comes which illuminates the objects before

fore him; and that without remaining confcious for a moment that he has attended to this circumftance: But the effect remains, and will influence his judgment. If on looking at a brick hearth he perceives that thofe lines which divide the bricks have a dark fhade on that fide oppofite to the light, and a bright ftreak on the contrary fide next to the light, he muft at the fame time perceive that they have the property which he has conftantly obferved in ridges, not in furrows. And fince the appearance of the hearth will be fuch, through the glaffes, in confequence of their inverting the fituation of its feveral parts, with refpect to the light, the obferver will inftantly pronounce the chinks between the bricks, and every little cavity in them, to be fo many perfect elevations above the common furface, nor can any effort of the mind correct the imagination or alter the appearance.

Though I was well fatisfied of the truth of this explanation, I refolved neverthelefs to bring it to the teft of experiment, which I did in the following manner.

In order to give my experiment fair play, I fhut all the windows of my chamber excepting one directly oppofite to the chimney. I then took the tube, with two convex glaffes, and looking through it at the hearth, all the bricks appeared depreffed and the clefts between them elevated, as ufual. I then placed a looking-glafs againft the chimney back, fo that it reflected the light from the window upon the hearth, and fet up a fmall board before the hearth to intercept the direct light of the window from it. Then looking at the hearth through the glaffes, I was much pleafed to find it appear in its natural ftate, with the bricks elevated. I then fat down on a chair at the edge of the hearth, and looking through the tube which I held to my eye with one hand, whilft with the other I moved the board fo as to make it fometimes intercept the direct light of the window, and at other times the reflected light of the looking-glafs, I conftantly found that when the hearth

was

was illuminated by reflected light, it appeared in its natural ftate, and when illuminated by the direct light, in its unnatural ftate; for fo I call it when the bricks appear depreffed and the chinks between them elevated.

I then confidered that fince the hearth appeared in its natural ftate by reflected light, and in its unnatural ftate by direct light, only in confequence of the inverting property of the glaffes, the appearance ought to be directly the contrary when it was viewed with the naked eye. And accordingly I found, upon taking out both of the glaffes, and looking through the open tube, that the hearth appeared as perfectly, and as conftantly in its unnatural ftate by reflected light, and in its natural ftate by direct light, as it had before done the reverfe through the glaffes. But it muft be obferved that fomething like a tube is neceffary to confine the fight from other adjoining objects, which not being in the fame circumftances would otherwife correct the imagination.

If we look through fuch a tube and glaffes at the hearth or other object, fuppofe a piece of chocolate, the furrows in it appear fo many ridges, on removing the tube they fink into furrows, on applying it they again rife into ridges, and the illufion might perhaps be repeated a thoufand times, without the mind being at all able to conceive the object to appear through the tube like what it really is. But if whilft you are looking through the tube, and the object appears in its unnatural ftate, that is, when its furrows appear ridges, you apply your finger and feel that they really are furrows, the deception vanifhes in a moment and the object appears in its natural ftate. This I at firft fuppofed to arife from the fuperior confidence which we have in the fenfe of touching, as knowing by experience that this fenfe more perfectly reprefents the figure of bodies than the fight does. But I was, at leaft in part, miftaken. For if whilft you fee the object in its unnatural ftate, another perfon puts his finger to the part you are looking

looking at, the deception vaniſhes as well as in the former caſe. The application of a writing pen or pencil will produce the ſame effect. And, which is very remarkable, after the mind has been undeceived by theſe means once or twice, it does not readily admit of the impoſition again: Though, as I obſerved before, if it be done by removing the glaſſes, the deception will return again as often as you pleaſe. The truth ſeems to be, that the mind chuſes the leaſt difficulty; and though in conſequence of the judgment it has formed concerning the direction of the light, it will ſubmit to ſuch a ſmall impoſition as to ſuppoſe one piece of chocolate may have ridges where others uſually have furrows, when indeed it has not, yet it will not readily endure ſuch a groſs one, as to ſuppoſe it to have cavities of the figure and colour of a finger or a writing pen. Or perhaps the viſible motion attending ſuch application produces the principal effect in convincing the mind that thoſe bodies are really elevated*, and then their ſhades and modifications of the light, ſhew in what direction it falls on them; and the miſtake of the mind in that particular being rectified, the whole object muſt aſſume its natural appearance.

The explanation I have given of this phænomenon will account for an odd circumſtance mentioned (I think) by Mr. Short; which once appeared ſo whimſical to me as neither to merit credit or attention. Mr. Short carefully examined the Caſſegrain teleſcope, and in all probability ſet it by the ſide of one of the Gregorian form, in order to determine its comparative merits: He gives the preference to the Gregorian, and mentions as a principal defect of the Caſſegrain teleſcope, that it repreſents the mountains in the moon as vallies, and the contrary. I doubt not but this,

 otherwiſe

* Whilſt I was making theſe experiments, I thought of a carved ſilver ſhoe buckle, as a very proper object to prevent a deception of this ſort from taking place. But placing it on a brick pavement, and looking at it through the glaſſes, it nevertheleſs appeared perfectly depreſſed. Preciſely as if you had taken a buckle and ſtrewing on it a white ſhining powder, had preſſed it into the brick whilſt ſoft, and then removing the buckle, the glittering powder had remained in the impreſſion.

otherwiſe unaccountable appearance, was occaſioned intirely by its inverting the object, for the reaſons above given. If it be aſked, why then do not the common long refractors, which generally invert, produce the ſame deception? I anſwer, very probably they would do ſo if ſet beſide a Gregorian reflector and the eye applied alternately to the one and to the other*.

N° III.

Deſcription of the White Mountains in New-Hampſhire.

BY THE REV. JEREMY BELKNAP OF NEW-HAMPSHIRE.

Read Oct. 15, 1784.

THE white mountains in the northern part of New-Hampſhire have, from the earlieſt ſettlement of the country, attracted the attention of all ſorts of perſons. They are undoubtedly the higheſt lands in New-England, and are diſcovered in clear weather by veſſels coming on the eaſtern coaſt, before any other land; but by reaſon of their bright appearance are frequently miſtaken for clouds. They are ſeen on ſhore at the diſtance of ſixty or eighty miles on the ſouth and ſouth-eaſt ſides, and are ſaid to be plainly viſible in the neighbourhood of Quebec. The Indians had a ſuperſtitious veneration for them as the habitation of inviſible beings, and for this reaſon never ventured to aſcend their ſummits, and always endeavoured to diſcourage every perſon who attempted it. From them, and the captives whom they formerly led to Canada through the paſs of theſe mountains, many fictions have been propagated through the country which have in time ſwelled to marvellous and incredible ſtories; particularly,

* The above was written in 1774, when I had no achromatic aſtronomical teleſcope in my poſſeſſion.

larly, it has been reported that carbuncles have been feen at immenfe heights, and inacceffible fpots which give a luftre in the night.

Thofe who have attempted to give an account of thefe mountains, have afcribed their brightnefs to fhining rocks or white mofs, and the higheft fummit has been reprefented as inacceffible by reafon of the extreme cold which threatens to freeze the traveller in the midft of fummer. They have alfo differed fo widely from each other, and their accounts have been embellifhed with fo many marvellous circumftances, and on the whole have been fo unfatisfactory, that I have long wifhed for an opportunity to vifit thefe mountains in company with fome gentlemen of a philofophical turn, furnifhed with proper inftruments and materials for a full exploration of the phenomena that might occur. This pleafure I have in part enjoyed the prefent fummer; and though the roughnefs of the way which prevented the ufe of convenient carriages, proved fatal to fome of our inftruments, and the almoft continual cloudinefs of the weather while we were in that region hindered us from making fome obfervations which we intended; yet till a better account can be obtained, I flatter myfelf that what follows will prove more fatisfactory than any which has yet been publifhed or reported.

The white mountains are the higheft part of a ridge which extends north-eaft and fouth-weft to an unknown length. The area of their bafe is an irregular figure fomewhat refembling an ifoceles triangle, whofe longeft extremity is toward the fouth, and whofe whole circuit cannot be lefs than fifty miles. The number of fummits within this area cannot be afcertained at prefent, the country round them being a thick wildernefs. On the northweft fide feven fummits are in plain view, and this is the greateft number that can be feen at once from any ftation that is cleared of woods. Of thefe, four at leaft are bald. The higheft of them is on the eaftern fide of the clufter, on which ſide we aſcended, having firſt gained the height of land between the waters of Saco and Amariſcogin rivers, to which there is a gradual aſcent for twelve miles from the plains of Pigwacket. At this height of land there is a meadow which was formerly a beaver-pond with a dam at each end. The water iſſues out of a mountain on its eaſtern ſide in the form of ſprings, and meandering through the channels of the meadow appears ſtagnant in the middle but dividing its courſe, at the ſouth end of the meadow it runs into Ellis river, a branch of Saco; and at the north end into Peabody river, a branch of Amariſcogin. From this meadow there is an uninterrupted aſcent on a ridge between two deep gullies to the higheſt ſummit.

The ſides of the mountains are covered with ſpruce trees; the ſurface is compoſed of looſe rocks covered with very long green moſs, which reaches from rock to rock, and is in many places ſo thick and ſtrong as to ſupport a man's weight. This immenſe bed of moſs, ſpread over the ſurface of theſe mountains ſerves, as a ſpunge to retain the moiſture brought by the clouds and vapors which are continually riſing and gathering round the mountains; the thick growth of ſpruce prevents the ſun's rays from penetrating to exhale it; ſo that there is a conſtant ſupply of water to the numberleſs ſprings with which this region abounds, and an unceaſing circulation of fluid, the proceſs of which is highly entertaining to the ſpectator; for no ſooner has a ſhower deſcended from the clouds, but the vapor riſes from the leaves of the foreſt in innumerable little columns, which, having gained a certain height in the atmoſphere, collect and converge toward the mountains, where they either fall again in ſhowers or are imbibed by the moſs and depoſited in the crevices of the rocks, ſeeking their way to the hard ſtratum or pan which is impenetrable, and which guides them till they find vent in ſprings. The ſame liquid tribute is daily exhaled from the rivers, ponds and low grounds, and attracted to the mountains,

mountains, which by theſe means are always repleniſhed with water in every part.

The rocks, of which theſe mountains are compoſed, are in ſome parts ſlate, in others flint, but toward the top a dark grey ſtone, which, when broken, ſhows ſpecks of iſing-glaſs. On the bald parts of the mountains the ſtones are covered with a ſhort grey moſs, and at the very ſummit the moſs is of a yellowiſh colour and adheres firmly to the rock.

Eight of our company aſcended the higheſt mountain on the 24th of July, and were ſix hours and fifty one minutes in gaining the ſummit, deducting one hour and thirty eight minutes for the neceſſary ſtops. The ſpruce and firs, as you aſcend, grow ſhorter till they degenerate to ſhrubs and buſhes, then you meet with low vines bearing a red and a blue berry, and laſtly a ſort of graſs called winter graſs mixed with the moſs.

Having aſcended the ſteepeſt precipice, you come to what is called the plain, where the aſcent becomes gentle and eaſy. This plain is compoſed of rocks, covered with winter graſs and moſs, and looks like the ſurface of a dry paſture or common. In ſome openings between the rocks you meet with water, in others dry gravel. The plain is an irregular figure, its area uncertain, but from its eaſtern edge to the foot of the ſugar-loaf, is upwards of a mile; on the weſtern ſide it extends farther. The ſugar-loaf is a pyramidal heap of looſe grey rocks, not leſs than three hundred feet in perpendicular height, but the aſcent is not ſo difficult as the precipice below the plain. From this ſummit in clear weather is a noble view, extending to the ocean on the ſouth-eaſt; to the highlands on the weſt and north-weſt, which ſeparate the waters of Connecticut river from thoſe of lake Champlain and St. Laurence; on the ſouth it extends to Winipiſeogee lake, and the highlands ſouthward of Pemigewaſſet river..

In

It happened unfortunately for our company, that a thick cloud covered the mountain almoſt the whole time that they were on it, ſo that ſome of the inſtruments which, with much labor they had carried up, were uſeleſs. In the barometer the mercury ranged at 22.6 inches, in 44 degrees of heat by Fahrenheit's thermometer. It was our intention to have placed one of each of theſe inſtruments at the foot of the mountain, at the ſame time that others were carried to the top; but they were unhappily broken in the courſe of our journey, and the barometer which was carried to the ſummit, had ſuffered ſo much agitation that an allowance was neceſſary to be made in calculating the height of the mountain, which our ingenious companion, the Rev. Mr. Cutler, of Ipſwich, eſtimates in round numbers at 5500 feet above the meadow, the meadow being 3500 feet above the level of the ſea, and this ſeems to be as low an eſtimation as can be admitted. We intended to have made a geometrical menſuration of the altitude, but in one place where we attempted it, we could not obtain a baſe of ſufficient length, and in another, where this convenience was removed, we were prevented by the almoſt continual obſcurations of the mountains by clouds.

On every ſide of theſe mountains are many long winding gullies, beginning at the precipice below the plain and deepening in the deſcent; they are from one hundred to one thouſand feet deep, and perhaps more. In winter, the ſnow driving with the north-weſt winds over the tops of the mountains, is lodged in theſe gullies, and forms a compact body which is not eaſily diſſolved by the vernal ſun. It is obſerved to lie longer on the ſouth, than on the north-weſt ſides; which is the caſe with moſt other hills in this part of the country. In 1774 ſome men who were at work on a road under the eaſtern ſide of the mountain, aſcended to the ſummit on the 6th of June, and upon the ſouth ſide found a body of ſnow thirteen feet deep, and ſo hard as to

bear

bear them. The man from whom I had this account, and who had the direction of the work, afcended the mountain on the 19th of June, with fome of the fame party, and in the fame fpot the fnow was five feet deep. On the 22d of July this year, we were affured by perfons who live within plain view of the mountains, on the fouth fide, at the diftance of fixteen miles, that the fnow had not been gone more than ten days. We were alfo credibly informed that two men, who attempted to afcend the mountain the firft week of September laft year, found the bald top fo covered with fnow and ice, then newly made, that they could not gain the fummit; but this does not happen every year fo foon, for the mountain has been afcended fo late as the firft week in October, when no fnow was upon it; and fometimes the firft fnows that come diffolve before the winter fets in; but generally the mountains begin to be covered with fnow and ice, either in the latter part of September, or the beginning of October, and it never wholly leaves them till July. During this period of nine or ten months, they exhibit more or lefs of that bright appearance, from which they are denominated *white*. In the fpring when the fnow is partly diffolved, they appear of a pale blue ftreaked with white; and after it is wholly gone, at the diftance of forty or fixty miles, they are altogether of a pale blue inclining to the colour of the fky; while viewed at the diftance of only ten miles, they are of the grey colour of the rock inclining to brown. Thefe changes are obferved by people who live within conftant view of them, and from thefe facts and obfervations it may juftly be concluded that the whitenefs of them is to be afcribed wholly to the fnow and ice and not to any other white fubftance, for in reality there is none. There are indeed in the fummer months fome ftreaks which appear brighter than other parts, but thefe, when viewed through a telefcope, I have plainly difcerned to be the enlightened edges or fides of the long deep gullies, and the dark parts the fhaded fides

of

of them; and in the courfe of a day thefe fpots may be feen to vary according to the pofition of the fun.

It may not be amifs to query here, if fo great a quantity of fnow is accumulated and remains on thefe mountains, may it not be fuppofed to add a keennefs to the winds which blow over them? And how many more mountains may there be toward the north and weft, whofe hoary fummits contain the like or greater bodies of fnow and ice, fome of which, at the remoteft regions, may remain undiffolved through the year? May we not then afcribe the piercing cold of our north-weft winds to the infinite ranges of frozen mountains, rather than to the lakes and forefts?

Thefe immenfe heights which I have been defcribing, being copioufly replenifhed with water, exhibit a variety of beautiful cafcades, fome of which fall in a perpendicular fheet or fpout, others are winding and narrow, others fpread on the level furface of fome wide rock and then gufh in cataracts over its edge. A romantic imagination may find full gratification amidft thefe rugged fcenes, if its ardor be not checked by the fatigue of the approach. Three of the largeft rivers in New-England receive a great part of their waters from this region. Amonoofuck and Ifrael rivers, two principal branches of Connecticut, fall from the weftern fide of the mountains, Peabody river and another branch of Amarifcogin from the north-eaftern fide, and almoft the whole of Saco defcends from the fouthern fide. The declivities being very fteep caufe this latter river to rife very fuddenly in a time of rain, and as fuddenly to fubfide.

On the weftern part of thefe mountains is a pafs which in the narroweft place meafures but twenty two feet between two perpendicular rocks. Here a road is conftructing with great labor and expence, which is the fhorteft rout to the upper Cohofs on Connecticut river, and to that part of Canada which borders on the river St. Francis. At the height of this narrow pafs the river Saco takes its rife.

A brook

A brook deſcends from the mountain, and meanders through a meadow which was formerly a beaver-pond, and is ſurrounded by ſteep, and on one ſide, perpendicular rocks—a ſtrikingly picturesque ſcene! the rivulet glides along the weſtern ſide of the defile, (the eaſtern being formed into a road) and tributary ſtreams augment its waters, one of which is called the Flume, from the near reſemblance it bears to the flume of a mill. The paſs between the mountains widens as you deſcend; but for eight or ten miles they are ſo near as only to leave room for the river and its intervals. In the courſe of this deſcent you ſee at immenſe heights, and in ſpots perfectly inacceſſible, ſeveral rocks, ſome of a whitiſh and ſome of a reddiſh hue, whoſe faces are poliſhed by the continual trickling of water over them. Theſe, when incruſted with ice, being open to the ſouth and weſt, are capable in the night of reflecting the moon and ſtar-beams to the wondering traveller, buried in the dark valley below; and theſe are ſufficient, by the help of imagination, to give riſe to the fiction of carbuncles.

We found no ſtones of any higher quality than flint*; no limeſtone, though we tried the moſt likely with aqua fortis. It is ſaid there is a part of the mountain where the magnetic needle refuſes to traverſe; this may contain rock ore, but our guide could not find the place. It is alſo ſaid that a mineral, ſuppoſed to be lead ore, has been diſcovered on the eaſtern ſide. One of the ſprings which we met with in our aſcent on that ſide afforded a thick frothy ſcum and a ſaponaceous taſte. All ſearches for ſubterranean treaſures in theſe mountains have as yet proved fruitleſs. The moſt certain riches which they yield are the freſhets which bring down the ſoil to the intervals below, and form a fine mould, producing corn, grain and herbage in the moſt luxuriant plenty.

* Some ſpecimens of rock-chryſtal have been found lately by other perſons, but we did not hear of it till after our return.

N° IV.

Defcription of a remarkable Rock *and* Cascade, *near the weftern fide of the Youghiogeny river, a quarter of a mile from Crawford's ferry, and about twelve miles from Union-Town, in Fayette county, in the ftate of Pennfylvania.*

BY THO. HUTCHINS.

Read January 28, 1786.

THIS cafcade is occafioned by a rock of a femicircular form, the chord of which, from one extreme end of the arch to the other, is nearly one hundred yards; the arch or circular part is extenfive, and upwards of twenty feet in height, exhibiting a grand and romantic appearance. This very curious production is compofed of ftone of variegated colours, and a fpecies of marble beautifully chequered with veins running in different directions, prefenting on a clofe infpection a faint refemblance of a variety of mathematical figures of different angles and magnitudes. The operations of nature in this ftructure feems to be exceedingly uniform and majeftic; the layers or rows of ftone of which it is compofed are of various lengths and thickneffes, more refembling the effects of art than nature. A flat thin ftone from eight to ten inches thick, about twenty feet wide, forms the upper part of this amphitheatre, over which the ftream precipitates. The whole front of this rock is made up from top to bottom, as well as from one extremity of the arch to the other, of a regular fucceffion, principally, of limeftone, ftrata over ftrata, and each ftratum or row, projecting in an horizontal direction a little further out than its bafe, until it terminates into one entire flat, thin, extenfive piece, as already mentioned; and which jets out at right angles or or in a parallel line with the bottom, over which it impends fifteen or twenty feet, and that without columns or even a ſingle pillar for its ſupport. This circumſtance, together with the grand circular walk between the front of the rock and the ſheet of water falling from the ſummit, exhibits ſo noble and ſingular an appearance, that a ſpectator cannot behold it without admiration and delight.

N° V.

Letter to Mr. NAIRNE, *of London.*

Paſſy, near Paris, Nov. 13th, 1780.

SIR,

Read January 28, 1786.

THE qualities hitherto ſought in a hygrometer, or inſtrument to diſcover the degrees of moiſture and dryneſs in the air, ſeem to have been, an aptitude to receive humidity readily from a moiſt air, and to part with it as readily to a dry air. Different ſubſtances have been found to poſſeſs more or leſs of this quality; but when we ſhall have found the ſubſtance that has it in the greateſt perfection, there will ſtill remain ſome uncertainty in the concluſions to be drawn from the degree ſhown by the inſtrument, ariſing from the actual ſtate of the inſtrument itſelf as to heat and cold. Thus, if two bottles or veſſels of glaſs or metal being filled, the one with cold and the other with hot water, are brought into a room, the moiſture of the air in the room will attach itſelf in quantities to the ſurface of the cold veſſel, while if you actually wet the ſurface of the hot veſſel, the moiſture will immediately quit it, and be abſorbed by the ſame air. And thus in a ſudden change of the air from cold to warm, the inſtrument remaining longer cold may condenſe and abſorb more moiſture, and mark the air as having become more humid than it is in reality, and the contrary in a change from warm to cold.

But if ſuch a ſuddenly changing inſtrument could be freed from theſe imperfections, yet when the deſign is to diſcover the different degrees of humidity in the air of different countries, I apprehend the quick ſenſibility of the inſtrument to be rather a diſadvantage; ſince, to draw the deſired concluſions from it, a conſtant and frequent obſervation day and night in each country will be neceſſary for a year or years, and the mean of each different ſet of obſervations is to be found and determined. After all which ſome uncertainty will remain reſpecting the different degrees of exactitude with which different perſons may have made and taken notes of their obſervations.

For theſe reaſons, I apprehend that a ſubſtance which, though capable of being diſtended by moiſture and contracted by dryneſs, is ſo ſlow in receiving and parting with its humidity that the frequent changes in the atmoſphere have not time to affect it ſenſibly, and which therefore ſhould gradually take nearly the medium of all thoſe changes and preſerve it conſtantly, would be the moſt proper ſubſtance of which to make ſuch an hygrometer.

Such an inſtrument, you, my dear ſir, though without intending it, have made for me; and I, without deſiring or expecting it, have received from you. It is therefore with propriety that I addreſs to you the following account of it; and the more, as you have both a head to contrive and a hand to execute the means of perfecting it. And I do this with greater pleaſure, as it affords me the opportunity of renewing that antient correſpondence and acquaintance with you, which to me was always ſo pleaſing and ſo inſtructive.

You may poſſibly remember, that in or about the year 1758, you made for me a ſet of artificial magnets, ſix in number, each five and a half inches long, half an inch broad, and one eighth of an inch thick. Theſe, with two pieces

pieces of ſoft iron, which together equalled one of the magnets, were incloſed in a little box of mahogany wood, the grain of which ran with, and not acroſs, the length of the box; and the box was cloſed by a little ſhutter of the ſame wood, the grain of which ran acroſs the box; and the ends of this ſhutting piece were bevelled ſo as to fit and ſlide in a kind of dovetail groove when the box was to be ſhut or opened.

I had been of opinion that good mahogany wood was not affected by moiſture ſo as to change its dimenſions, and that it was always to be found as the tools of the workman left it. Indeed the difference at different times in the ſame country, is ſo ſmall as to be ſcarcely in a common way obſervable. Hence the box which was made ſo as to allow ſufficient room for the magnets to ſlide out and in freely, and, when in, afforded them ſo much play that by ſhaking the box one could make them ſtrike the oppoſite ſides alternately, continued in the ſame ſtate all the time I remained in England, which was four years, without any apparent alteration. I left England in Auguſt 1762, and arrived at Philadelphia in October the ſame year. In a few weeks after my arrival, being deſirous of ſhowing your magnets to a philoſophical friend, I found them ſo tight in the box, that it was with difficulty I got them out; and conſtantly during the two years I remained there, viz. till November 1764, this difficulty of getting them out and in continued. The little ſhutter too, as wood does not ſhrink length ways of the grain, was found too long to enter its grooves, and not being uſed, was miſlaid and loſt; and I afterwards had another made that fitted.

In December 1764 I returned to England, and after ſome time I obſerved that my box was become full big enough for my magnets, and too wide for my new ſhutter; which was ſo much too ſhort for its grooves, that it was apt to fall

fall out; and to make it keep in, I lengthened it by adding to each end a little coat of ſealing-wax.

I continued in England more than ten years, and during all that time after the firſt change, I perceived no alteration. The magnets had the ſame freedom in their box, and the little ſhutter continued with the added ſealing-wax to fit its grooves, till ſome weeks after my ſecond return to America.

As I could not imagine any other cauſe for this change of dimenſions in the box, when in the different countries, I concluded, firſt generally that the air of England was moiſter than that of America. And this I ſuppoſed an effect of its being an iſland, where every wind that blew muſt neceſſarily paſs over ſome ſea before it arrived, and of courſe lick up ſome vapour. I afterwards indeed doubted whether it might be juſt only ſo far as related to the city of London, where I reſided; becauſe there are many cauſes of moiſture in the city air, which do not exiſt to the ſame degree in the country; ſuch as the brewers and dyers boiling caldrons, and the great number of pots and teakettles continually on the fire, ſending fourth abundance of vapour; and alſo the number of animals who by their breath continually increaſe it; to which may be added, that even the vaſt quantity of ſea coals burnt there, do in kindling diſcharge a great deal of moiſture.

When I was in England, the laſt time, you alſo made for me a little achromatic pocket teleſcope, the body was braſs, and it had a round caſe, (I think of thin wood) covered with ſhagrin. All the while I remained in England, though poſſibly there might be ſome ſmall changes in the dimenſions of this caſe, I neither perceived nor ſuſpected any. There was always comfortable room for the teleſcope to ſlip in and out. But ſoon after I arrived in America, which was in May 1775, the caſe became too ſmall for the inſtrument, it was with much difficulty and various contrivances that I got it out, and I could never after get

get it in again, during my ſtay there, which was eighteen months. I brought it with me to Europe, but left the caſe as uſeleſs, imagining that I ſhould find the continental air of France as dry as that of Pennſylvania, where my magnet box had alſo returned a ſecond time to its narrowneſs, and pinched the pieces, as heretofore, obliging me too, to ſcrape the ſealing-wax off the ends of the ſhutter.

I had not been long in France, before I was ſurpriſed to find, that my box was become as large as it had always been in England, the magnets entered and came out with the ſame freedom, and, when in, I could rattle them againſt its ſides; this has continued to be the caſe without ſenſible variation. My habitation is out of Paris diſtant almoſt a league, ſo that the moiſt air of the city cannot be ſuppoſed to have much effect upon the box. I am on a high dry hill in a free air as likely to be dry as any air in France. Whence it ſeems probable that the air of England in general may as well as that of London, be moiſter than the air of America, ſince that of France is ſo, and in a part ſo diſtant from the ſea.

The greater dryneſs of the air in America appears from ſome other obſervations. The cabinet work formerly ſent us from London, which conſiſted in thin plates of fine wood glued upon fir, never would ſtand with us, the vaneering, as thoſe plates are called, would get looſe and come off; both woods ſhrinking, and their grains often croſſing, they were forever cracking and flying. And in my electrical experiments there, it was remarkable, that a mahogany table on which my jars ſtood under the prime conductor to be charged, would often be ſo dry, particularly when the wind had been ſome time at north-weſt which with us is a very drying wind, as to iſolate the jars, and prevent their being charged till I had formed a communication between their coatings and the earth. I had a like table in London which I uſed for the ſame purpoſe all the time I reſided there; but it was never ſo dry as to refuſe conducting the electricity.

Now

Now what I would beg leave to recommend to you, is, that you would recollect, if you can, the ſpecies of mahogany of which you made my box, for you know there is a good deal of difference in woods that go under that name; or if that cannot be, that you would take a number of pieces of the cloſeſt and fineſt grained mahogany that you can meet with, plane them to the thinneſs of about a line, and the width of about two inches acroſs the grain, and fix each of the pieces in ſome inſtrument that you can contrive, which will permit them to contract and dilate, and will ſhow, in ſenſible degrees, by a moveable hand upon a marked ſcale, the otherwiſe leſs ſenſible quantities of ſuch contraction and dilatation. If theſe inſtruments are all kept in the ſame place while making, and are graduated together while ſubject to the ſame degrees of moiſture or dryneſs, I apprehend you will have ſo many comparable hygrometers, which being ſent into different countries, and continued there for ſome time, will find and ſhow there the mean of the different dryneſs and moiſture of the air of thoſe countries, and that with much leſs trouble than by any hygrometer hitherto in uſe.

With great eſteem,

I am, dear ſir,

Your moſt obedient,

And moſt humble ſervant,

B. FRANKLIN.

N° VI.

Deſcription of a new Stove *for burning of Pitcoal, and conſuming all its Smoke.*

BY DR. B. FRANKLIN.

Read January 28, 1786. TOWARDS the end of the laſt century an ingenious French philoſopher, whoſe name I am ſorry I cannot recollect, exhibited an experiment to ſhow that very offenſive things might be burnt in the middle of a chamber, ſuch as woollen rags, feathers, &c. without creating the leaſt ſmoke or ſmell. The machine in which the experiment was made, if I remember right, was of this form, made of plate iron. Some clear burning charcoals were put into the opening of the ſhort tube A, and ſupported there by the grate B. The air as ſoon as the tubes grew warm would aſcend in the longer leg C and go out at D, conſequently air muſt enter at A deſcending to B. In this courſe it muſt be heated by the burning coals through which it paſſed, and riſe more forcibly in the longer tube in proportion to its degree of heat or rarefaction, and length of that tube. For ſuch a machine is a kind of inverted ſyphon; and as the greater weight of water in the longer leg of a common ſyphon in deſcending is accompanied by an aſcent of the ſame fluid in the ſhorter; ſo, in this inverted ſyphon, the greater quantity of levity of air in the longer leg, in riſing is accompanied by the deſcent of air in the ſhorter. The things to be burned being laid on the hot coals at A, the ſmoke muſt deſcend through thoſe coals, be converted into flame, which, after deſtroying the offenſive ſmell, came out at the end of the longer tube as mere heated air.

Plate II. Figure 1.

Whoever would repeat this experiment with ſucceſs, muſt take care that the part A, B, of the ſhort tube be quite full of burning coals, ſo that no part of the ſmoke may deſcend and paſs by them without going through them, and being converted into flame; and that the longer tube be ſo heated as that the current of aſcending hot air is eſtabliſhed in it before the things to be burnt are laid on the coals; otherwiſe there will be a diſappointment.

It does not appear either in the Memoirs of the Academy of Sciences, or Philoſophical Tranſactions of the Engliſh Royal Society, that any improvement was ever made of this ingenious experiment, by applying it to uſeful purpoſes. But there is a German book, entitled *Vulcanus Famulans*, by Joh. George Leutmann, P. D. printed at Wirtemberg in 1723, which deſcribes, among a great variety of other ſtoves for warming rooms, one which ſeems to have been formed on the ſame principle, and probably from the hint thereby given, though the French experiment is not mentioned. This book being ſcarce, I have tranſlated the chapter deſcribing the ſtove, viz.

" Vulcanus Famulans, by John George Leutmann, P. D.
" Wirtemberg, 1723.

" CHAP. VII.

" On a ſtove, which draws downwards.

" Here follows the deſcription of a ſort of ſtove, which " can eaſily be removed and again replaced at pleaſure. " This drives the fire down under itſelf, and gives no " ſmoke, but however a very unwholeſome vapour.

" In the figure, A is an iron veſſel like a fun- " nel, in diameter at the top about twelve inches, " at the bottom near the grate about five inches; " its height twelve inches. This is ſet on the barrel C, " which is ten inches diameter and two feet long, cloſed

Plate II. Figure 20.

" at

"at each end E E. From one end riſes a pipe or flue "about four inches diameter, on which other pieces of pipe "are ſet, which are gradually contracted to D, where the "opening is but about two inches. Thoſe pipes muſt together be at leaſt four feet high. B is an iron grate. "F F are iron handles guarded with wood, by which the "ſtove is to be lifted and moved. It ſtands on three legs. "Care muſt be taken to ſtop well all the joints, that no "ſmoke may leak through.

"When this ſtove is to be uſed, it muſt firſt be carried "into the kitchen and placed in the chimney near the fire. "There burning wood muſt be laid and left upon its grate "till the barrel C is warm, and the ſmoke no longer riſes "at A, but deſcends towards C. Then it is to be carried "into the room which it is to warm. When once the "barrel C is warm, freſh wood may be thrown into the "veſſel A as often as one pleaſes, the flame deſcends and "without ſmoke, which is ſo conſumed that only a vapour paſſes out at D.

"As this vapour is unwholeſome, and affects the head, "one may be freed from it, by fixing in the wall of the "room an inverted funnel, ſuch as people uſe to hang over "lamps, through which their ſmoke goes out as through "a chimney. This funnel carries out all the vapour cleverly, ſo that one finds no inconvenience from it, even "though the opening D be placed a ſpan below the mouth "of the ſaid funnel G. The neck of the funnel is better "when made gradually bending, than if turned in a right "angle.

"The cauſe of the draft downwards in the ſtove is the "preſſure of the outward air, which falling into the veſſel "A in a column of twelve inches diameter, finds only "a reſiſting paſſage at the grate B, of five inches, and "one at D, of two inches, which are much too weak "to drive it back again; beſides, A ſtands much higher "than B, and ſo the preſſure on it is greater and more

 "forcible,

"forcible, and beats down the flame to that part where "it finds the leaſt reſiſtance. Carrying the machine firſt "to the kitchen fire for preparation, is on this account, "that in the beginning the fire and ſmoke naturally aſcend, "till the air in the cloſe barrel C is made thinner by the "warmth. When that veſſel is heated, the air in it is "rarefied, and then all the ſmoke and fire deſcends "under it.

"The wood ſhould be throughly dry, and cut into "pieces five or ſix inches long, to fit it for being thrown "into the funnel A." Thus far the German book.

It appears to me by Mr. Leutmann's explanation of the operation of this machine, that he did not underſtand the principles of it, whence I conclude he was not the inventor of it; and by the deſcription of it, wherein the opening at A is made ſo large, and the pipe E, D, ſo ſhort, I am perſuaded he never made nor ſaw the experiment, for the firſt ought to be much ſmaller and the laſt much higher, or it hardly will ſucceed. The carrying it in the kitchen, too, every time the fire ſhould happen to be out, muſt be ſo troubleſome, that it is not likely ever to have been in practice, and probably has never been ſhown but as a philoſophical experiment. The funnel for conveying the vapour out of the room, would beſides have been uncertain in its operation, as a wind blowing againſt its mouth would drive the vapour back.

The ſtove I am about to deſcribe, was alſo formed on the idea given by the French experiment, and completely carried into execution before I had any knowledge of the German invention; which I wonder ſhould remain ſo many years in a country where men are ſo ingenious in the management of fire, without receiving long ſince the improvements I have given it.

Description of the Parts.

A, the bottom plate which lies flat upon the hearth, with its partitions 1, 2, 3, 4, 5, 6, that are caſt with it, and a groove Z Z, in which are to ſlide, the bottom edges of the ſmall plates Y, Y, figure 12; which plates meeting at X cloſe the front. Plate II. Figure 2.

B 1, figure 3, is the cover plate ſhowing its under ſide, with the grooves 1, 2, 3, 4, 5, 6, to receive the top edges of the partitions that are fixed to the bottom plate. It ſhows alſo the grate W W, the bars of which are caſt in the plate, and a groove V V, which comes right over the groove Z Z, figure 2, receiving the upper edges of the ſmall ſliding plates Y Y, figure 12.

B 2, figure 4, ſhows the upper ſide of the ſame plate, with a ſquare impreſſion or groove for receiving the bottom mouldings T T T T of the three ſided box C, figure 5, which is caſt in one piece.

D, figure 6, its cover, ſhowing its under ſide with grooves to receive the upper edges S S S of the ſides of C, figure 5, alſo a groove R, R, which when the cover is put on comes right over another Q Q in C, figure 5, between which it is to ſlide.

E, figure 7, the front plate of the box.

P, a hole three inches diameter through the cover D, figure 6, over which hole ſtands the vaſe F, figure 8, which has a correſponding hole two inches diameter through its bottom.

The top of the vaſe opens at O, O, O, figure 8, and turns back upon a hinge behind when coals are to be put in; the vaſe has a grate within at N N of caſt iron H, figure 9, and a hole in the top one and a half inches diameter to admit air, and to receive the ornamental braſs guilt flame M, figure 10, which ſtands in that hole and, being itſelf hollow and open, ſuffers air to paſs through it to the fire.

G, figure 11, is a drawer of plate iron, that ſlips in between in the partitions 2 and 3, figure 2, to receive the falling

falling aſhes. It is concealed when the ſmall ſliding plates Y Y, figure 12, are ſhut together.

I, I, I, I, figure 8, is a niche built of brick in the chimney and plaſtered. It cloſes the chimney over the vaſe, but leaves two funnels one in each corner communicating with the bottom box K K, figure 2.

DIMENSIONS of the PARTS.

	Feet.	In.
Front of the bottom box, - -	2	0
Height of its partitions, - -	0	$4\frac{1}{4}$
Length of N° 1, 2, 3 and 4, each, -	1	3
Length of N° 5 and 6, each - -	0	$8\frac{1}{4}$
Breadth of the paſſage between N° 2 and 3,	0	6
Breadth of the other paſſages each, -	0	$3\frac{1}{2}$
Breadth of the grate, - -	0	$6\frac{1}{2}$
Length of ditto, - - -	0	8
Bottom moulding of box C, ſquare, -	1	0
Height of the ſides of ditto, - -	0	4
Length of the back ſide, - -	0	10
Length of the right and left ſides, each, -	0	$9\frac{1}{2}$
Length of the front plate E, where longeſt,	0	11
The cover D, ſquare, - -	0	12
Hole in ditto, diameter, - -	0	3
Sliding plates Y Y their length, each, -	1	0
——————their breadth, each, -	0	$4\frac{1}{2}$
Drawer G, its length, - - -	1	0
————breadth, - - -	0	$5\frac{3}{4}$
————depth, - - -	0	4
————depth of its further end, only,	0	1
Grate H in the vaſe, its diameter to the extremity of its knobs, - - -	0	$5\frac{3}{4}$
Thickneſs of the bars at top, - -	0	$0\frac{1}{4}$
————————at bottom, leſs, -	0	0
Depth of the bars at the top, - -	0	$0\frac{3}{4}$
Height of the vaſe, - - -	1	6
Diameter of the opening O, O, in the clear,	0	8

Diameter

	Feet.	In.
Diameter of the air-hole at top, - -	0	1 ½
———of the flame hole at bottom, -	0	2

To fix this Machine.

Spread mortar on the hearth to bed the bottom plate A, then lay that plate, level, equally diſtant from each jamb, and projecting out as far as you think proper. Then puting ſome Windſor loam in the grooves of the cover B, lay that on: Trying the ſliding plates Y Y, to ſee if they move freely in the groves Z Z, V V, deſigned for them.

Then begin to build the niche, obſerving to leave the ſquare corners of the chimney unfilled; for they are to be funnels. And obſerve alſo to leave a free open communication between the paſſages at K K, and the bottom of thoſe funnels, and mind to cloſe the chimney above the top of the niche, that no air may paſs up that way. The concave back of the niche will reſt on the circular iron partition 1 A 4, figure 2, then with a little loam put on the box C over the grate, the open ſide of the box in front.

Then, with loam in three of its grooves, the groove R R being left clean, and brought directly over the groove Q Q in the box, put on the cover D, trying the front plate E, to ſee if it ſlides freely in thoſe grooves.

Laſtly, ſet on the vaſe, which has ſmall holes in the moulding of its bottom to receive two iron pins that riſe out of the plate D at I I, for the better keeping it ſteady.

Then putting in the grate H, which reſts on its three knobs H H H againſt the inſide of the vaſe, and ſlipping the drawer into its place; the machine is fit for uſe.

To uſe it.

Let the firſt fire be made after eight in the evening or before eight in the morning, for at thoſe times and between thoſe hours all night, there is uſually a draft up a chimney, though it has long been without fire; but between thoſe hours in the day there is often in a cold chimney.

ney a draft downwards, when if you attempt to kindle a fire, the ſmoke will come into the room.

But to be certain of your proper time, hold a flame over the air-hole at the top. If the flame is drawn ſtrongly down for a continuance, without whiffling, you may begin to kindle a fire.

Firſt put in a few charcoals on the grate H.

Lay ſome ſmall ſticks on the charcoals,

Lay ſome pieces of paper on the ſticks,

Kindle the paper with a candle,

Then ſhut down the top, and the air will paſs down through the air-hole, blow the flame of the paper down through the ſticks, kindle them, and their flame paſſing lower, kindles the charcoal.

When the charcoal is well kindled, lay on it the ſea-coals, obſerving not to choak the fire by putting on too much at firſt.

The flame deſcending through the hole in the bottom of the vaſe, and that in plate D into the box C paſſes down farther through the grate W W in plate B 1, then paſſes horizontally towards the back of the chimney; there dividing, and turning to the right and left, one part of it paſſes round the far end of the partition 2, then coming forward it turns round the near end of partition 1, then moving backward it arrives at the opening into the bottom of one of the upright corner funnels behind the niche, through which it aſcends into the chimney, thus heating that half of the box and that ſide of the niche. The other part of the divided flame paſſes round the far end of partition 3, round the near end of partition 4, and ſo into and up the other corner funnel, thus heating the other half of the box, and the other ſide of the niche. The vaſe itſelf, and the box C will alſo be very hot, and the air ſurrounding them being heated, and riſing, as it cannot get into the chimney, it ſpreads in the room, colder air ſucceeding

ſucceeding is warmed in its turn, riſes and ſpreads, till by the continual circulation the whole is warmed.

If you ſhould have occaſion to make your firſt fire at hours not ſo convenient as thoſe above mentioned, and when the chimney does not draw, do not begin it in the vaſe, but in one or more of the paſſages of the lower plate, firſt covering the mouth of the vaſe. After the chimney has drawn a while with the fire thus low, and begins to be a little warm, you may cloſe thoſe paſſages and kindle another fire in the box C, leaving its ſliding ſhutter a little open; and when you find after ſome time that the chimney being warmed draws forcibly, you may ſhut that paſſage, open your vaſe, and kindle your fire there, as above directed. The chimney well warmed by the firſt day's fire will continue to draw conſtantly all winter, if fires are made daily.

You will, in the management of your fire, have need of the following implements:

A pair of ſmall light tongs, twelve or fifteen inches long, plate II, figure 13.

A light poker about the ſame length with a flat broad point, figure 14.

A rake to draw aſhes out of the paſſages of the lower plate, where the lighter kind eſcaping the aſh-box will gather by degrees, and perhaps once in a week or ten days require being removed, figure 15.

And a fork with its prongs wide enough to ſlip on the neck of the vaſe cover, in order to raiſe and open it when hot, to put in freſh coals, figure 16.

In the management of this ſtove there are certain precautions to be obſerved, at firſt with attention, till they become habitual. To avoid the inconvenience of ſmoke, ſee that the grate H be clear before you begin to light a freſh fire. If you find it clogged with cinders and aſhes, turn it up with your tongs and let them fall upon the grate below; the aſhes will go through it, and the cinders may be raked off and returned into the vafe when you would burn them. Then fee that all the fliding plates are in their places and clofe fhut, that no air may enter the ftove but through the round opening at the top of the vafe. And to avoid the inconvenience of duft from the afhes, let the afh-drawer be taken out of the room to be emptied; and when you rake the paffages, do it when the draft of the air is ftrong inwards, and put the afhes carefully into the afh-box, that remaining in its place.

If being about to go abroad, you would prevent your fire burning in your abfence, you may do it by taking the brafs flame from the top of the vafe, and covering the paffage with a round tin plate, which will prevent the entry of more air than barely fufficient to keep a few of the coals alive. When you return, though fome hours abfent, by taking off the tin plate and admitting the air, your fire will foon be recovered.

The effect of this machine, well managed, is to burn not only the coals, but all the fmoke of the coals, fo that while the fire is burning, if you go out and obferve the top of your chimney, you will fee no fmoke iffuing, nor any thing but clear warm air, which as ufual makes the bodies feen through it appear waving.

But let none imagine from this, that it may be a cure for bad or fmoky chimneys, much lefs, that as it burns the fmoke it may be ufed in a room that has no chimney. 'Tis by the help of a good chimney, the higher the better, that it produces its effect; and though a flue of plate iron fufficiently high might be raifed in a very lofty room, the management to prevent all difagreeable vapour would be too nice for common practice, and fmall errors would have unpleafing confequences.

It is certain that clean iron yields no offenfive fmell when heated. Whatever of that kind you perceive, where there are iron ftoves, proceeds therefore from fome foulnefs burning or fuming on their furface. They fhould

therefore

therefore never be ſpit upon, or greaſed, nor ſhould any duſt be ſuffered to lie upon them. But as the greateſt care will not always prevent theſe things, it is well once a week to waſh the ſtove with ſoap lees and a bruſh, rinſing it with clean water.

The Advantages of this Stove.

1. The chimney does not grow foul, nor ever need ſweeping; for as no ſmoke enters it, no ſoot can form in it.

2. The air heated over common fires inſtantly quits the room and goes up the chimney with the ſmoke; but in the ſtove, it is obliged to deſcend in flame and paſs through the long winding horizontal paſſages, communicating its heat to a body of iron plate, which having thus time to receive the heat, communicates the ſame to the air of the room, and thereby warms it to a greater degree.

3. The whole of the fuel is conſume d by beingturned into flame, and you have the benefit of its heat, whereas in common chimneys a great part goes away in ſmoke which you ſee as it riſes, but it affords you no rays of warmth. One may obtain ſome notion of the quantity of fuel thus waſted in ſmoke, by reflecting on the quantity of ſoot that a few weeks firing will lodge againſt the ſides of the chimney, and yet this is formed only of thoſe particles of the column of ſmoke that happen to touch the ſides in its aſcent. How much more muſt have paſſed off in the air? And we know that this ſoot is ſtill fuel; for it will burn and flame as ſuch, and when hard caked together is indeed very like and almoſt as ſolid as the coal it proceeds from. The deſtruction of your fuel goes on nearly in the ſame quantity whether in ſmoke or in flame: but there is no compariſon in the difference of heat given. Obſerve when freſh coals are firſt put on your fire, what a body of ſmoke ariſes. This ſmoke is for a long time too cold to take flame. If you then plunge a burning candle into it, the candle inſtead of inflaming the ſmoke will in-

ſtantly be itſelf extinguiſhed. Smoke muſt have a certain degree of heat to be inflammable. As ſoon as it has acquired that degree, the approach of a candle will inflame the whole body, and you will be very ſenſible of the difference of the heat it gives. A ſtill eaſier experiment may be made with the candle itſelf. Hold your hand near the ſide of its flame, and obſerve the heat it gives; then blow it out, the hand remaining in the ſame place, and obſerve what heat may be given by the ſmoke that riſes from the ſtill burning ſnuff. You will find it very little. And yet that ſmoke has in it the ſubſtance of ſo much flame, and will inſtantly produce it, if you hold another candle above it ſo as to kindle it. Now the ſmoke from the freſh coals laid on this ſtove, inſtead of aſcending and leaving the fire while too cold to burn, being obliged to deſcend through the burning coals, receives among them that degree of heat which converts it into flame, and the heat of that flame is communicated to the air of the room, as above explained.

4. The flame from the freſh coals laid on in this ſtove, deſcending through the coals already ignited, preſerves them long from conſuming, and continues them in the ſtate of red coals as long as the flame continues that ſurrounds them, by which means the fires made in this ſtove are of much longer duration than in any other, and fewer coals are therefore neceſſary for a day. This is a very material advantage indeed. That flame ſhould be a kind of pickle, to preſerve burning coals from conſuming, may ſeem a paradox to many, and very unlikely to be true, as it appeared to me the firſt time I obſerved the fact. I muſt therefore relate the circumſtances, and ſhall mention an eaſy experiment, by which my reader may be in poſſeſſion of every thing neceſſary to the underſtanding of it. In the firſt trial I made of this kind of ſtove, which was conſtructed of thin plate iron, I had inſtead of the vaſe a kind of inverted pyramid like a mill-hopper; and fearing at firſt

firft that the fmall grate contained in it might be clogged by cynders, and the paffage of the flame fometimes obftructed, I ordered a little door near the grate, by means of which I might on occafion clear it. Though after the ftove was made, and before I tried it, I began to think this precaution fuperfluous, from an imagination, that the flame being contracted in the narrow part where the grate was placed, would be more powerful in confuming what it fhould there meet with, and that any cynders between or near the bars would be prefently deftroyed and the paffage opened. After the ftove was fixed and in action, I had a pleafure now and then in opening that door a little, to fee through the crevice how the flame defcended among the red coals, and obferving once a fingle coal lodged on the bars in the middle of the focus, a fancy took me to obferve by my watch in how fhort a time it would be confumed. I looked at it long without perceiving it to be at all diminifhed, which furprifed me greatly. At length it occurred to me, that I and many others had feen the fame thing thoufands of times, in the confervation of the red coal formed in the fnuff of a burning candle, which while envelloped in flame, and thereby prevented from the contact of paffing air, is long continued and augments inftead of diminifhing, fo that we are often obliged to remove it by the fnuffers, or bend it out of the flame into the air, where it confumes prefently to afhes. I then fuppofed that to confume a body by fire, paffing air was neceffary to receive and carry off the feparated particles of the body; and that the air paffing in the flame of my ftove, and in the flame of a candle, being already faturated with fuch particles, could not receive more, and therefore left the coal undiminifhed as long as the outward air was prevented from coming to it by the furrounding flame, which kept it in a fituation fomewhat like that of charcoal in a well luted crucible, which, though long kept in a ftrong fire, comes out unconfumed.

An

An eaſy experiment will ſatisfy any one of this conſerving power of flame envelloping red coal. Take a ſmall ſtick of deal or other wood the ſize of a gooſe quill, and hold it horizontally and ſteadily in the flame of the candle above the wick, without touching it, but in the body of the flame. The wood will firſt be inflamed, and burn beyond the edge of the flame of the candle, perhaps a quarter of an inch. When the flame of the wood goes out, it will leave a red coal at the end of the ſtick, part of which will be in the flame of the candle and part out in the air. In a minute or two you will perceive the coal in the air diminiſh gradually, ſo as to form a neck; while the part in the flame continues of its firſt ſize, and at length the neck being quite conſumed it drops off; and by rolling it between your fingers when extinguiſhed you will find it ſtill a ſolid coal.

However, as one cannot be always putting on freſh fuel in this ſtove to furniſh a continual flame as is done in a candle, the air in the intervals of time gets at the red coals and conſumes them. Yet the conſervation while it laſted, ſo much delayed the conſumption of the coals, that two fires, one made in the morning, and the other in the afternoon, each made by only a hatfull of coals, were ſufficient to keep my writing room, about ſixteen feet ſquare and ten high, warm a whole day. The fire kindled at ſeven in the morning would burn till noon; and all the iron of the machine with the walls of the niche being thereby heated, the room kept warm till evening, when another ſmaller fire kindled kept it warm till midnight.

Inſtead of the ſliding plate E, which ſhuts the front of the box C, I ſometimes uſed another which had a pane of glaſs, or, which is better, of Muſcovy talc, that the flame might be ſeen deſcending from the bottom of the vaſe and paſſing in a column through the box C, into the cavities of the bottom plate, like water falling from a funnel, admirable to ſuch as are not acquainted with the nature of the machine, and in itſelf a pleaſing ſpectacle.

Every

Every utenſil, however properly contrived to ſerve its purpoſe, requires ſome practice before it can be uſed adroitly. Put into the hands of a man for the firſt time, a gimblet or a hammer, (very ſimple inſtruments) and tell him the uſe of them, he ſhall neither bore a hole or drive a nail with the dexterity or ſucceſs of another who has been a little accuſtomed to handle them. The beginner therefore in the uſe of this machine, will do well not to be diſcouraged with little accidents that may ariſe at firſt from his want of experience. Being ſomewhat complex, it requires as already ſaid a variety of attentions; habit will render them unneceſſary. And the ſtudious man who is much in his chamber, and has a pleaſure in managing his own fire, will ſoon find this a machine moſt comfortable and delightful. To others who leave their fires to the care of ignorant ſervants, I do not recommend it. They will with difficulty acquire the knowledge neceſſary, and will make frequent blunders that will fill your room with ſmoke. It is therefore by no means fit for common uſe in families. It may be adviſeable to begin with the flaming kind of ſtone coal, which is large, and, not caking together, is not ſo apt to clog the grate. After ſome experience, any kind of coal may be uſed, and with this advantage, that no ſmell, even from the moſt ſulphurous kind can come into your room, the current of air being conſtantly into the vaſe, where too that ſmell is all conſumed.

The vaſe form was choſen as being elegant in itſelf, and very proper for burning of coals: Where wood is the uſual fuel, and muſt be burnt in pieces of ſome length, a long ſquare cheſt may be ſubſtituted, in which A is the cover opening by a hinge behind, B the grate, C the hearth box with its diviſions as in the other, D the plan of the cheſt, E the long narrow grate. This I have not tried, but the vaſe machine was compleated in 1771, and uſed by me in London three winters, and one afterwards in America, much to my ſatisfaction; and I have not yet thought

Plate 2. Figure 17.

thought of any improvement it may be capable of, though ſuch may occur to others. For common uſe, while in France, I have contrived another grate for coals, which has in part the ſame property of burning the ſmoke and preſerving the red coals longer by the flame, though not ſo completely, as in the vaſe, yet ſufficiently to be very uſeful, which I ſhall now deſcribe as follows.

A, is a round grate, one foot (French) in diameter, and eight inches deep between the bars and the back; the ſides and back of plate iron; the ſides having holes of half an inch diameter diſtant 3 or 4 inches from each other, to let in air for enlivening the fire. The back without holes. The ſides do not meet at top nor at bottom by eight inches: that ſquare is filled by grates of ſmall bars croſſing front to back to let in air below, and let out the ſmoke or flame above. The three middle bars of the front grate are fixed, the upper and lower may be taken out and put in at pleaſure, when hot, with a pair of pincers. This round grate turns upon an axis, ſupported by the crotchet B, the ſtem of which is an inverted conical tube five inches deep, which comes on as many inches upon a pin that fits it, and which is fixed upright in a caſt iron plate D, that lies upon the hearth; in the middle of the top and bottom grates are fixed ſmall upright pieces E E about an inch high, which as the whole is turned on its axis ſtop it when the grate is perpendicular. Figure 19 is another view of the ſame machine. Plate 2. Figure 18.

In making the firſt fire in a morning with this grate, there is nothing particular to be obſerved. It is made as in other grates, the coals being put in above, after taking out the upper bar, and replacing it when they are in. The round figure of the fire when thoroughly kindled is agreeable, it repreſents the great giver of warmth to our ſyſtem. As it burns down and leaves a vacancy above, which you would fill with freſh coals, the upper bar is to be taken out, and afterwards replaced. The freſh coals while the

grate

grate continues in the ſame poſition, will throw up as uſual a body of thick ſmoke. But every one accuſtomed to coal fires in common grates, muſt have obſerved that pieces of freſh coal ſtuck in below among the red coals have their ſmoke ſo heated as that it becomes flame as faſt as it is produced, which flame riſes among the coals and enlivens the appearance of the fire. Here then is the uſe of this ſwivel grate. By a puſh with your tongs or poker, you turn it on its pin till it faces the back of the chimney, then turn it over on its axis gently till it again faces the room, whereby all the freſh coals will be found under the live coals, and the greater part of the ſmoke ariſing from the freſh coals will in its paſſage through the live ones be heated ſo as to be converted into flame: Whence you have much more heat from them, and your red coals are longer preſerved from conſuming. I conceive this conſtruction, though not ſo complete a conſumer of all the ſmoke as the vaſe, yet to be fitter for common uſe, and very advantageous. It gives too a full ſight of the fire, always a pleaſing object, which we have not in the other. It may with a touch be turned more or leſs from any one of the company that deſires to have leſs of its heat, or preſented full to one juſt come out of the cold. And ſupported in a horizontal poſition, a tea-kettle may be boiled on it.

The author's deſcription of his Pennſylvania fire-place, firſt publiſhed in 1744, having fallen into the hands of workmen in Europe, who did not, it ſeems, well comprehend the principles of that machine, it was much diſfigured in their imitations of it; and one of its main intentions, that of admitting a ſufficient quantity of freſh air warmed in entering through the air-box, nearly defeated, by a pretended improvement, in leſſening its paſſages to make more room for coals in a grate. On pretence of ſuch improvements, they obtained patents for the invention, and for a while made great profit by the ſale, till the public became ſenſible of that defect, in the ex-

K pected

pected operation. If the same thing should be attempted with this vase stove, it will be well for the buyer to examine thoroughly such pretended improvements, lest, being the mere productions of ignorance, they diminish or defeat the advantages of the machine, and produce inconvenience and disappointment.

The method of burning smoke, by obliging it to descend through hot coals, may be of great use in heating the walls of a hot-house. In the common way, the horizontal passages or flues that are made to go and return in those walls, lose a great deal of their effect when they come to be foul with soot; for a thick blanket-like lining of soot prevents much of the hot air from touching and heating the brick work in its passage, so that more fire must be made as the flue grows fouler: But by burning the smoke they are kept always clean. The same method may also be of great advantage to those businesses in which large coppers or caldrons are to be heated.

Written at Sea, 1785.

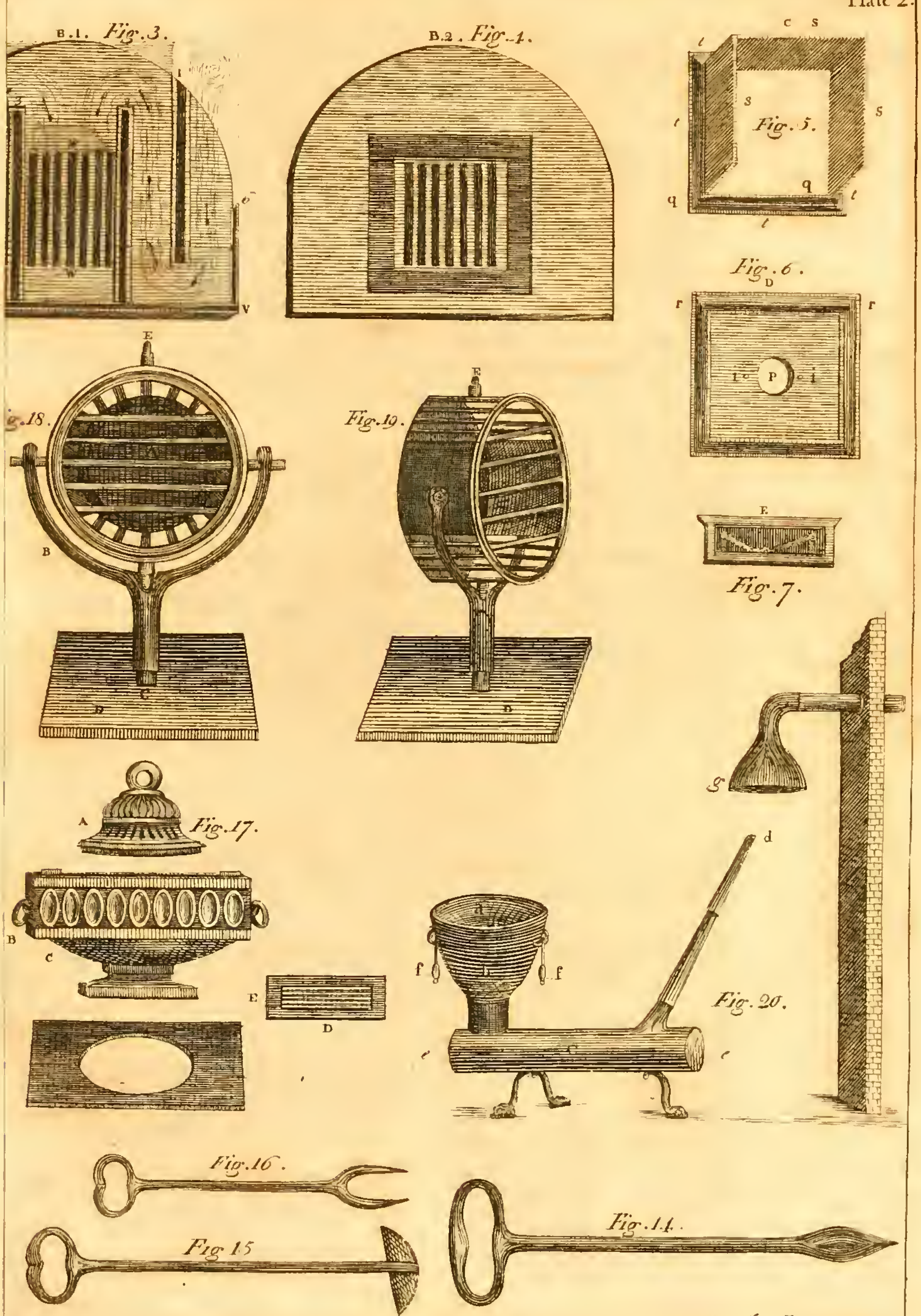
N° VII.

A Theory of Lightening and Thunder Storms, by ANDREW OLIVER, *Esq. of Salem in the State of Massachusetts.*

Read January, 1774.

IT has been generally, and, considering the phenomena themselves, very naturally supposed, that the electric charges which are exhibited in repeated flashes of lightening during a thunder storm, are previously accumulated in the vapors which constitute the cloud; and that these vapors, when by any means they become either over-charged with electric matter, or are deprived of their natural

Plate 2.



Jn.ᵒ Taylor sculp.ᵗ


Plate 2.
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.
Fig. 14.
Fig. 15
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.

natural quantities of it*, difcharge their furplufage to, or receive the neceffary fupplies from, either the earth or the neighbouring clouds, in fucceffive explofions, till an equilibrium is reftored between them. But I fhall endeavour in the following pages to prove, that thefe charges refide, not in the cloud or vapors of which it confifts, but in the air which fuftains them; and that, previous to the formation of the cloud, or even the afcent of the vapors of which it is formed. But, in order to convey my ideas upon this fubject with perfpicuity, I find it neceffary to introduce them with a quotation from doctor *Franklin*'s letters on electricity, in which the doctor compares water, whether in its natural ftate, or rarefied into vapors, to a fponge; and the electric fluid, in connection with it, to water applied to the fponge.

" When a fponge (fays he) is fomewhat condenfed by " being fqueezed between the fingers, it will not receive " and retain fo much water as when it is in its more loofe " and open ftate. If more fqueezed and condenfed, fome " of the water will come out of its inner parts, and flow " on the furface. If the preffure of the fingers be intirely removed, the fponge will not only refume what was " lately forced out, but attract an additional quantity. As " the fponge in its rarer ftate will *naturally* attract and " abforb *more* water; and in its denfer ftate will *naturally* " attract and abforb *lefs* water; we may call the quantity " it abforbs in either ftate, its *natural quantity*, the ftate " being confidered."

The doctor then fuppofes, " that what the fponge is to " water, the fame is water to the electric fluid;—that " when a portion of water is in its common denfe ftate, " it can hold no more electric fluid than it has; if any be " added it fpreads upon the furface." He adds, " when " the fame portion of water is rarefied into vapor and forms

 " a cloud,

* A body is faid to be electrically charged, whenever it has either *more* or *lefs* than its natural quantity of electric matter.

"a cloud, it is then capable of receiving and abſorbing a "much greater quantity, as there is room for each parti- "cle to have an electric atmoſphere. Thus water in its "rarefied ſtate, or in the form of a cloud, will be in a "negative ſtate of electricity; it will have leſs than its "*natural quantity*, that is, leſs than it is naturally capable "of attracting and abſorbing in that ſtate*."

The foregoing paſſages I have copied *verbatim* from that celebrated electrician, as I purpoſe in the courſe of this eſſay to avail myſelf of his idea of the ſponge, in order to illuſtrate a different theory of thunder clouds, which I now beg leave, though with diffidence of my own judgment, and with all due deference to that of ſo great a man, to ſubſtitute in the room of the foregoing; which I muſt confeſs at firſt ſight carries great appearance of probability with it, and is highly corroborated by the curious and beautiful experiment the doctor made with the ſilver cann, braſs chain, and lock of cotton†.

But in reading doctor *Prieſtley*'s hiſtory of electricity, ſome thoughts of ſignior *Beccaria* occurred, which ſatisfied me that this hypotheſis, however ingenious and plauſible, was inſufficient for the purpoſe of accounting for the riſe and phenomena of thunder ſtorms, the frequent extent and violence of which ſeem to require a more general cauſe than that hinted above, to ſupply them with ſufficient quantities of electric matter.

"Conſidering the vaſt quantity of electric fire that ap- "pears in the moſt ſimple thunder ſtorms (ſays doctor "Prieſtly‡) ſignior *Beccaria* thinks it impoſſible that any "cloud, or number of clouds, ſhould ever contain it all, "ſo as either to diſcharge or receive it. Beſides, during "the progreſs and increaſe of the ſtorm, though the light- "ening frequently ſtruck to the earth, the ſame clouds "were

* Franklin's Letters, page 119.
† Page 121.
‡ Prieſtley's Hiſtory of Electricity, page 325.

" were the next moment ready to make a ſtill greater diſ-
" charge, and his apparatus continued to be as much af-
" fected as ever. The clouds muſt conſequently have *re-*
" *ceived* at one place the moment that a *diſcharge* was
" made from them in another."

Signior *Beccaria* accounts for this vaſt exhibition of electric fire from a thunder cloud, by ſuppoſing that ſome parts of the earth may become more highly charged with the electric fluid than others, and that great quantities of it do ſometimes ruſh out of particular parts, and riſe through the air into the higher regions of the atmoſphere; other parts of the earth becoming caſually deſtitute of their natural quantity of the fluid at the ſame time, and ready to receive it: That a chain of clouds nearly contiguous, or a ſingle cloud extending from one of theſe regions to another, in an oppoſite ſtate, might ſerve as a conductor or conductors to reſtore the electric equilibrium between them, which would equally cauſe thunder and lightening in both regions, and throughout the intermediate clouds*. Here doctor *Prieſtley* juſtly obſerves, that " the greateſt difficul-
" ty attending this theory of the origin of thunder ſtorms
" relates to the *collection* and *inſulation* of electric matter
" within the body of the earth." With regard to the *collection*, the doctor obſerves that his author " has nothing
" particularly to ſay :" Nor indeed without a previous *inſulation* of thoſe parts of the earth which may be concerned in the production of the phenomena, can any ſuch *collection* take place. Now if we conſider that in order to have two regions of the earth thus inſulated, and of ſufficient dimenſions, one to ſupply, and the other to receive the quantities of electric fire diſcharged during one thunder ſtorm of any extent and continuance, the parts inſulated muſt be not ſuperficial regions, but muſt reach to a conſiderable depth; and we muſt ſuppoſe, with doctor Prieſtley, " that the electric matter which forms and ani-
" mates the thunder cloud, iſſues from places far below
" the

* Ibid.

" the ſurface of the earth, and that it buries itſelf there*." But, with deference to the judgment of that unwearied friend to ſcience, I apprehend that ſuch an inſulation is hardly conſiſtent with that diſtribution of conductors, eſpecially of water, which provident nature has made through all parts of our globe; the higheſt mountains being furniſhed with *internal* ſprings and fountains, and watered *externally* by rivulets, which derive their origin from condenſing miſts or melting ſnows upon their ſummits: While the ſurface of the earth in general, not excepting the moſt ſandy deſerts, affords ſupplies of water to thoſe who will be at the pains of digging for it. If then the vapors which conſtitute the cloud are, of themſelves, incapable of furniſhing ſuch quantities of electric matter as are neceſſary for the repeated diſcharges in a ſevere thunder ſtorm, as ſignior *Beccaria* thinks they are, and as ſeems to me indubitable; and if the inſulations of large portions of the ſurface or exterior parts of the earth, which are abſolutely neceſſary to ſupport *Beccaria*'s hypotheſis, cannot take place; which, how they can in our terraqueous maſs, is difficult to conceive, conſiſtently with the *hitherto* diſcovered properties of the electric fluid: We muſt ſeek for ſome other ſubſtance in nature which may be capable of affording thoſe reiterated ſupplies, of that powerful element which are uſually exhibited in a thunder ſtorm. This I preſume, we ſhall find in the atmoſphere over our heads; not in the *vapors* which float therein, but in the *air* itſelf which ſuſtains them.

Air is by electricians juſtly claſſed with *electric* ſubſtances, as it poſſeſſes the ſame general properties in common with others of that denomination, particular inſtances of which may occur in the following pages; wherein I ſhall endeavour to prove,

I. That the *electric capacity* of air is leſſened by condenſation.

II. That *this* capacity is increaſed by heat.

Premiſing

* Prieſtley, page 335.

Premiſing that by *air* I here intend *that* fluid in its common compreſſed ſtate with us near the ſurface of the earth; and by its *electric capacity*, that ſtate of it which diſpoſes it, under any circumſtances whatever, "to attract, abſorb and retain," what doctor *Franklin* calls its *natural quantity*, or the quantity which is *natural* to it in that ſtate.

I. I ſhall endeavour to prove that the *electric capacity* of air is leſſened by condenſation.

That a change of denſity in air produces alſo a change in its electric capacity (as above defined), follows from ſome experiments of monſieur de *Faye* and doctor *Prieſtley*, the former of whom found, upon repeated trials, that no electricity could be excited by the friction of a glaſs tube in which the air was condenſed*. The doctor, repeating the experiments with ſome variation, found, that when one additional atmoſphere was forced into the tube, the electricity excited by rubbing it was ſcarcely diſcernable. Now, though the effect was a ſuſpenſion of the operation of the excited tube *without*, the cauſe was evidently the condenſed ſtate of the air *within*; which may be accounted for if we conſider, that although it is certain from many experiments that glaſs is abſolutely impermeable to the electric fluid, inſomuch that it cannot force its way through a pane of glaſs, or the ſides of a phial, without breaking the glaſs, as was the caſe in thoſe ſpontaneous diſcharges of ſeveral of the jars in the electrical battery mentioned by doctor *Prieſtley*†; yet it is as certain, that this impermeability of the glaſs to the fluid itſelf, is no obſtruction to the operation of that repellent power upon which the viſible effects of this element ſeem principally to depend; which power undeniably acts from one ſide of the glaſs, through the very ſubſtance of it, upon the ſame fluid on the other ſide, provided there be any other ſubſtance on that ſide capable of receiving it when thus repelled.

This is the caſe in the *Leyden* experiment in every form in which it can be made; the charge given to one ſide of the

* Page 50. † Page 489.

the glaſs, repelling and throwing off an equal quantity of the electric fluid from the oppoſite ſurface, through the non-electric coating in contact with it; nor can any charge be given to either ſide without a proportional diſcharge from the other. In like manner, when an uncoated tube is excited by friction, a quantity of the fluid, equal to that which is excited and condenſed upon the outer ſurface, is thrown out from the inner, provided there is any ſubſtance within in a capacity to receive and abſorb it, without which no excitation can take place. "A glaſs tube, "out of which the air is exhauſted, diſcovers no ſigns of "electricity outwards*," there being no ſubſtance within capable of receiving and abſorbing the fluid from the inner ſurface, which though repelled from it inwards during the operation, yet returns to it again inſtantly upon a ceſſation of the action of the rubber without. But upon a readmiſſion of air the excitation is eaſy, and is attended with the uſual effects. Air then, which is the only ſubſtance admitted (excepting perhaps a few ſtraggling vapors which float in it) receives and abſorbs a ſufficient quantity of the electric fluid from the inner ſurface to permit an excitation of the tube which contains it. But as we have ſeen that air, when condenſed within, prevents the viſible effects of an excitation, equally with a total vacuity, we may adopt the idea of doctor *Franklin*, *mutatis mutandis*, and conlcude that "what the ſponge is to water "the ſame is *air* to the electric fluid:" At leaſt that this capacity of air if leſſened by condenſation in a manner, not indeed perfectly ſimilar, but, ſomewhat analogous to that in which the capacity of a ſponge to receive and retain water is leſſened by compreſſion. Agreeably to which idea, the condenſed air within the tube, having its electric capacity filled and even crowded with the electric matter, will receive none from the inner ſurface, which, on the contrary, is thereby prevented from being forced out of it, without which

* Prieſtley's hiſtory of electricity, page 550.

which none can be forced into or condenſed upon the outer ſurface, ſo as to exhibit any ſigns of electricity; as obſerved before,

II. I ſhall endeavour to prove that the electric capacity of air is increaſed by heat.

This alſo appears probable, at leaſt, from the above cited experiments of doctor *Prieſtley*; for after the air in his tube had had this capacity ſo far diminiſhed by condenſation as not to permit an excitation without, that capacity, together with the conſequent excitability of the tube, was reſtored by the action of heat upon the included air. "Repeating my attempts (ſays he) to excite the tube above "mentioned, I found that, after very hard rubbing, it be-"gan to act a little, and that its virtue increaſed with the "labour. Thinking it might be the warmth which pro-"duced this effect, I held the tube to the fire and found "that when it was pretty hot, it would act almoſt as well "as when it contained no more than its uſual quantity "of air*."

In page 553, doctor *Prieſtley* tells us that ſome of his electrical friends were of opinion, "that the reaſon why "a tube with condenſed air in it cannot be excited is, that "the denſe air within prevents the electric fluid from be-"ing forced out of the inſide of the tube, without which "none can be forced into the outſide; and that heating "the tube makes the air within leſs electrical." That is, as I conceive their meaning, puts it in a capacity to receive and abſorb more of the electric fluid than it could otherwiſe do in that condenſed ſtate. The doctor indeed queries by way of objection to the foregoing ſolution,— "How upon this principle can a ſolid ſtick of glaſs be ex-"cited?" To which I would anſwer, that poſſibly, when a ſolid ſtick of glaſs is excited, as much of the electric fluid may be drawn out of one ſide of it as is thrown into, or condenſed upon the other; if ſo, although it may ſhew equal ſigns of electricity on both ſides, yet one ſide will be in a

L poſitive

* Page 551.

positive, the other in a negative state; when it will exactly resemble the curious stone called the *tourmalin*, by some *lapis electricus*, which doctor *Priestley* says * "has "always, *at the same time*, a positive and a negative elec- "tricity; one of its sides being in one state, and the other "in the opposite;" which does not depend upon the external form "of the stone." But the truth of this solution must be determined by future experiments.

That the electrical state of the air is liable to be affected by heat, is further evident from a course of experiments which were made by the abbé *Mazeas*, with an apparatus that was constructed solely with a view of determining the electricity of the atmosphere, anno 1753 †. With this apparatus the abbé observed, that from the 17th of June, when he began his experiments, the electricity of the air was sensibly felt every day, *from sun rise till seven or eight o'clock in the evening*, when the weather was *dry;* but that in the driest *nights* of that summer he could discover no signs of electricity in the air, nor till the morning, when the sun began to appear above the horizon, and that "they vanished again in the evening, about half an "hour after sun set;" and further, "that the *strongest* "common electricity of the atmosphere, during the sum- "mer, was perceived in the month of *July* on a *very dry* "day, the heavens being very clear, and the sun *extreme- "ly hot*."

Now, as this electricity of the air was sensible only during *day light*, no electricity being discoverable therein even in the *driest nights*, and as the air exhibited the *strongest* signs of electricity when the sun shone *extremely hot;* is not the conclusion unavoidable, that heat somehow affects the electric capacity of air, either enlarging it, and thereby disposing the air to attract, receive and absorb greater quantities of electric matter than it is capable of absorbing in its natural state; or superadding to its *natural quantity* more than it can absorb, and thereby disposing it to throw off

* Page 299. † Page 342.

off the redundancy upon any objects which may be in a ſituation to receive it? One or the other ſeems neceſſarily to follow, but the former is moſt agreeable to doctor *Prieſtley*'s experiment of the condenſed air in the tube above mentioned, and is perfectly conſonant with the obſervations of doctor *Franklin*, Mr. *Kinnerſley* and others, that thunder clouds are generally in the *negative* ſtate of electricity*. But more upon this head hereafter. I would however obſerve here, that many, and perhaps all other electric ſubſtances, even the moſt firm and ſolid, as well as air, are liable to have their electric capacities thus diverſified by heat, more particularly the tourmalin above mentioned. But as, in treating of the properties of this ſtone, doctor *Prieſtley* has thought it deſerving of a diſtinct ſection in his electric hiſtory, to that I ſhall refer the reader for a particular account of them †; wherein he will find a diſcovery made by Meſſrs *Canton* and *Wilſon*, that theſe properties are not peculiar to the tourmalin, but that many gems have a natural diſpoſition to afford the ſame appearances; from whence we may conclude as above, by analogy, that all electric ſubſtances are, more or leſs, affected in like manner, by the ſame cauſe. But to return to the ſubject.

If from the foregoing conſiderations the reader ſhould be ſatisfied, that the *electric capacity* of air, in its condenſed ſtate in the lower regions of the atmoſphere, is liable to be diminiſhed by a further condenſation, and that, *cæteris paribus*, it is increaſed by heat *et vice verſa*; the ſolution of the phenomena of thunder and lightening, to his ſatisfaction, upon electrical principles, will perhaps be no difficult taſk.

For let us conceive a region of the atmoſphere, extending over a large tract of country, to be rarefied and heat-

* Epitome of Phil. Tranſ. Gent. Mag. Sept. 1773, page 447. Mr. Henley thinks cold electrifies the atmoſphere poſitively, and thence conjectures that heat electrifies it negatively. His concluſions are founded upon a courſe of experiments.

† Page 297.

ed during a hot ſummer's day, not only by the paſſage of the ſun's direct rays through it, and by the reflectinoof thoſe rays from the ſurface of the earth into it; but chiefly, by the communication of the heat acquired by that ſurface to it: The *electric capacity* of that region of air would be increaſed, both on account of the heat it undergoes, and of the rarefaction conſequent upon that heat: It will then have leſs than its *natural quantity*, or the quantity it is *naturally* diſpoſed to receive and abſorb in that ſtate; it will conſequently be, in the language of electricians, *negatively* electriſed, or in a craving ſtate, requiring and forcing ſupplies from all ſubſtances capable of affording them, provided it be itſelf in a condition to receive them. But, however craving, it cannot receive thoſe ſupplies from the neighbouring regions of the atmoſphere, while thoſe regions ſeverally remain in the ſtate of pure air, even ſuppoſing the latter to poſſeſs more than their *natural quantities*, and thereby as much diſpoſed to impart, as the former is to receive them, without the intervention of non-electric conductors; and that, owing to the impermeability of air, as ſuch, to the electric fluid. This I ſhall endeavour, 1. To illuſtrate by experiments made with glaſs. 2. To prove by experiments made upon air itſelf.

1. If a pane of glaſs be coated on both ſides, by the application of plates of tin to them, the glaſs may be charged in the ſame manner as the *Leyden* phial; when, after the removal of the plates, no diſcharge having previouſly taken place, both ſides of the glaſs will remain charged, one poſitively, the other negatively; the former having more than its *natural quantity*, the latter being proportionably deficient, and in a craving ſtate. Theſe ſtates both ſurfaces will obſtinately maintain for a long time: Nor do I know of any method of reſtoring the electric equilibrium between them, but, either to immerſe the pane in water or ſome other non-electric fluid, which will do it inſtantly, and ſilently; or to reapply the metalline coatings to both ſides

ſides as they were placed at firſt, with a good conductor introduced between them, which will anſwer the ſame purpoſe, and be attended with an exploſion, or ſmart ſpark and ſnap; or laſtly, to place it in a ſituation where it may be expoſed to air replete with moiſt vapors, where, after ſome time, the vapors will, by condenſing upon each ſide, furniſh it with a moiſture equivalent to a non-electric coating, while the vapors which remain in the ſurrounding air will, by continually impinging upon and receding from the two ſurfaces, at length reſtore both to their natural ſtate.

It is evident from the foregoing experiment, *Firſt*, That the charges reſide in the glaſs itſelf, as they remain after the coatings are removed. *Secondly*, That the oppoſite ſides have a very ſtrong propenſity, one to give, the other to receive the fluid, and thereby to reſtore the electric equilibrium between themſelves; which is done with violence, as obſerved above, when they are put in a condition of doing it by the reapplication of the metalline coatings, with a conductor between them, and *Laſtly*, That notwithſtanding the violent propenſity in the ſides of the glaſs, to reſtore themſelves and each other to their natural electric ſtates, and the ſmall diſtance between them, they can never effect it, without the intervention of non-electric conductors.

2. I ſhall now ſhew by other experiments, that different regions or ſtrata of air *may* become charged, both poſitively and negatively, in the ſame manner as the ſides of the pane of glaſs were in the foregoing; and that the effects of ſuch charges are preciſely the ſame.

Meſſrs *Wilkie* and *Æpinus* at *Berlin*, having the hint naturally ſuggeſted to them by a previous courſe of experiments, endeavoured to give the electrical ſhock by means of *air*, in the ſame manner in which it may be given by *glaſs*; "in which after making ſeveral attempts (ſays doc- "tor Prieſtley*) they at length ſucceeded, by ſuſpending "large

* Page 242.

" large boards of wood covered with tin, with the flat ſides " towards one another, and at ſome inches aſunder. For " they found, that upon electrifying one of the boards " poſitively, the other was always negative. But the diſ- " covery was made complete and indiſputable by a perſon's " touching one of the plates with one hand, and bringing " his other hand to the other plate; for he then received " a ſhock through his body exactly like that of the *Ley-* " *den* experiment. With this plate of air, as we may call " it, they made a variety of experiments. The two me- " tal plates, being in oppoſite ſtates, ſtrongly attracted one " another, and would have ruſhed together if they had " not been kept aſunder by the ſtrings. Sometimes the " electricity of both would be diſcharged by a ſtrong ſpark " between them, as when a pane of glaſs burſts with too " great a charge. A finger put between them promoted " the diſcharge, and felt the ſhock. If an eminence was " made on either of the plates the ſelf-diſcharge would al- " ways be made through it, and a pointed body fixed up- " on either of them prevented their being charged at all."

To the foregoing relation of the experiments themſelves, I ſhall ſubjoin the concluſions drawn from them by the curious electricians who made them, in the words of doctor *Prieſtley*, viz. " The ſtate of theſe two plates, they " (*Wilkie* and *Æpinus*) " excellently obſerve, juſtly repreſents the " ſtate of the clouds and the earth" (and perhaps of different clouds at various heights one over another) " dur- " ing a thunder ſtorm; the clouds being always in one " ſtate, and the earth in the oppoſite; while the body of " air between them anſwers the ſame purpoſe as the ſmall " plate of air between the boards, or the plate of glaſs be- " tween the two metal coatings in the *Leyden* experiment. " The phenomenon of lightening is the burſting of the " plate of air by a ſpontaneous diſcharge, which is always " made through eminencies, and the bodies through which " the diſcharge is made are violently ſhocked."

As

As in the former experiment made with the pane of glaſs, the charges, both poſitive and negative, reſide in the glaſs itſelf, and not in the coatings, thoſe remaining after theſe are removed; ſo in the latter, which is completely analogous to it, the charges are accumulated and reſide in the air ſituated between the boards, and not in their tin linings, which ſerve only as conductors, to diſtribute the fluid equally over, or to convey it equally from, the whole ſurface of air which is limited by, and in contact with them, on either ſide; whereby the whole of each ſurface may be equally charged at the ſame time, or diſcharged by the ſame exploſion.

If two or more regions of the atmoſphere, when free from vapors, become thus differently electrical in their ſtate and capacities, which, that they may, from the heat and conſequent rarefaction in a ſummer's day, we have already ſeen, and perhaps from a variety of other cauſes to us unknown; and if from the contrary currents of air which frequently take place at different heights, they ſhould perchance become ſituated one over or adjacent to another, like ſtrata of minerals within the bowels of the earth; what the metalline coating is to the pane of glaſs, or the tinned boards to the plate of air in the laſt experiment, the ſame would clouds, formed and floating therein, be to theſe regions of air; the electric equilibrium between which might be reſtored through their intervention, either by ſpontaneous diſcharges through the pure air between them in ſevere flaſhes of lightening or through the falling drops of rain, which in their ſucceſſive deſcent form a chain of natural conductors between one region of the air and another, and betwixt each of them and the earth; the paſſage of the electric fluid through which would alſo be attended with lightening and thunder, but not ſo ſevere as when the diſcharge is made through the pure air; the moſt fatal lightening uſually preceding the fall of the rain.

It

It is not uncommon, during the riſe and progreſs of a thunder ſtorm, to ſee different ſets of clouds, at various heights in the atmoſphere, moving promiſcuouſly in all directions, as though they were impelled hither and thither by contending winds; when probably the whole phenomenon ariſes from the different electrical ſtates of the regions of the air in which they float; as they approach one or other of which, they are attracted or repelled, and move accordingly, communicating, receiving, or tranſmitting the electric fluid, to or from them reſpectively, as they may be either deficient of their natural quantity, or poſſeſs a redundancy of this fluid. And as in the experiment of Meſſrs *Wilkie* and *Æpinus* mentioned above, the two tin plates with the boards they covered, would have ruſhed together had they not been kept aſunder by the ſtrings, ſo theſe clouds floating freely in air, and being at liberty to act upon every impulſe, gradually coaleſce, reſtoring the electric equilibrium to the neighbouring atmoſphere by repeated diſcharges as they unite*; till at length they form one denſe maſs of humid vapors, which precipitating in a heavy ſhower of rain, refreſh the thirſty ſoil, leaving the atmoſphere above in a homogenous electric ſtate, calm and ſerene.

How theſe clouds are generated, formed, and adapted to thoſe grand purpoſes in the œconomy of nature, is next to be conſidered: In proſecution of which inquiries I ſhall ſubmit the following obſervations to the candor of the reader.

Whatever the immediate cauſe of evaporation may be, it is certain that the ſuperficial moiſture of all bodies is perpetually exhaling in vapors, which aſcend into the higher regions of the atmoſphere, where they gather and are formed into clouds, and at length recondenſe, deſcending

* It is certain that in moſt thunder ſtorms the flaſhes of lightening are chiefly diſcharged from cloud to cloud, very few, and frequently none at all taking place between the cloud and the earth.

ing in dew, mift or rain upon the furface of the earth from whence they fprang.

Thefe vapors are either detached in ftreams from the humid ground by the influence of the fun, or thrown off by the perfpirations of thofe infinite multitudes of animals and plants which cover the face of the earth*, or fupplied by evaporation, from the ocean, or other grand collections of water.

Ignorant as we are of the nature of thefe operations, and of the manner in which they are performed, it is natural to fuppofe, that the vapors themfelves afcend in the fame electric ftate, whether pofitive, neutral or negative, with the fubftances from which they arife. Accordingly fignior *Beccaria*, in making fome of his experiments, obferved, that " fteam rifing from an electrified eolipile diffufes it-" felf with the fame uniformity with which thunder clouds " fpread themfelves and fwell into arches, extending itfelf " towards any conducting fubftance†." This ftream then was electrified as well as the eolipile from whence it proceeded. The fea muft neceffarily be fuppofed, in common with the whole terraqueous mafs, to contain juft its natural quantity of the electric fluid, and no more: We may therefore conclude that both the vapors which arife immediately from it, and the air which fuftains them, and from its fituation enjoys a more equable temperature, than that over the land, are in the fame electrical ftate with the fea itfelf, containing neither *more* nor *lefs* than their *natural quantity*.

Confidering the vaft extent of the ocean, and the comparatively fmall degree of moifture of which the dry land is fufceptible, we may conclude, that a very fmall proportion of the clouds which are formed in the atmofphere are exhaled from the latter, and that the ocean is the grand fource from whence they principally derive their origin.

M Our

* See *Hales's* vegetable ftatics, and Chambers's *cycloped*, under the word, Perfpiration.

† Prieftley's Hiftory, page 327.

Our ſenſes accordingly convince us that the ſea-air is always replete with moiſt vapors, even when its natural tranſparency is not in the leaſt interrupted by them. Hence in a hot ſummer's day, when the wind ſuddenly ſhifts from weſt to eaſt, we immediately perceive a chill from the ſea-breeze; and ſometimes long before the thermometer indicates a change in the temperature of the atmoſphere. Theſe vapors, when they firſt ariſe from the ſea, are generally ſo nearly of the ſame denſity with the ſurrounding and contiguous air, that the rays of light in paſſing through them, undergo no ſenſible change in their refraction; they are therefore at firſt generally inviſible, but when the weather is extreamly cold, and the air of conſequence uncommonly denſe, they are always viſible, and appear like a ſteam ariſing from boiling water*. Not that vapors aſcend moſt copiouſly in the coldeſt ſeaſons, which ſeems contrary both to reaſon and experience; but that the different denſities of the air next the ſurface of the water, and of the vapors which aſcend in it, render the latter viſible, by the irregular refractions of the rays of light in paſſing through them. For the ſame reaſon our breath is viſible in the winter, but not in warm weather.

Let us now ſuppoſe the atmoſphere, on a ſummer's morning, to be all around in a homogenous ſtate, as indicated by a cloudleſs ſky and a dead calm. As the ſun riſes on the eaſtern coaſts of America, and warms and rarefies the atmoſphere eaſtward, the rarefied air naturally aſcends, and a current of air as naturally flows thither from the oppoſite quarter, which is but juſt emerging from the cool ſhades of night, to ſupply its place. The conſequence of which is a light weſterly breeze. As the ſun aſcends higher, the air over the land becomes heated and rarefied, both by the paſſage of the ſun's direct and reflected rays through it, and by the reverberation of the heat acquired from

* This is always the appearance in a clear, ſtill morning, when the mercury in Farenheit's thermometer is at 0, or below it.

from them by the ſurface of the earth; till at length that whole region of the atmoſphere has its electrical capacity enlarged, thereby becoming negatively electriſed, or in a craving ſtate, as obſerved before. On the contrary the ſun's rays which fall upon the ſurface of the ſea, eſpecially when ruffled by wind, chiefly enter that tranſparent medium, in which they are refracted and irrecoverably abſorbed; very few, comparatively, being reflected; whence very little heat can be reverberated from that element to warm the incumbent air, which is ſenſibly affected only by the paſſage of the ſun's direct rays through it, unleſs the weather be calm and the ſurface very ſmooth*. Beſides, it is colder at ſea than aſhore in the ſummer ſeaſon, when, and when only thunder ſhowers are frequent, and indeed warmer in the winter, for the following reaſon, viz. as the ſea is every moment changing its ſurface, neither heat nor cold can affect it ſo ſoon as they do the ſurface of the earth, which continues the ſame.

The air over the land, when thoroughly heated and rarefied, naturally aſcends into the higher regions, while the denſer air from the ſea neceſſarily flows in and takes its place. Hence, probably, the eaſterly winds which uſually ſpring up near the middle of the day, after a ſultry morning.

This body of warm air aſcends till it arrives at that region of the atmoſphere in which thunder clouds are formed; while the vapors which are wafted to the continent by the eaſtern current, being attracted by this now ſuperior air which demands a ſupply of the electric fluid, continually

* In a perfect calm the ſurface of the ſea acts like a mirror upon the ſun's rays, ſtrongly reverberating them back into the atmoſphere, *when* the heat is as ſenſible upon water as upon the dry land. But whenever that ſurface becomes agitated and broken by the force of wind acting upon it, thoſe rays, by perpetually impinging upon an infinite variety of new formed, fluctuating ſurfaces undergo innumerable refractions, in all directions, whereby they are abſorbed and loſt within the fluid maſs in ſome proportion to the violence of the agitation. Accordingly when the weather is ſerene and calm, the ſurface like a looking-glaſs reflects the phenomena of the ſky over head; upon the firſt ſpringing up of a breeze it changes to a light blue, which deepens to a fine ſky-blue as the wind riſes, to a deeper ſea-green in a briſk gale, and to a ſullen blackneſs in a ſtorm, excepting where the waves are interſperſed with white heads of foam, which, by contraſt, only render the ſcene more gloomy.

tinually aſcend till they arrive at it, leaving the denſer air, with which they were firſt connected, behind. As theſe vapors move freely through and mix with air, they eaſily inſinuate themſelves between the particles of that fluid, and unite with it, whereby every particle of air which, from the cauſes aforeſaid, is become in any degree deſtitute of the quantity of electric matter which is natural to it in its preſent ſtate, may and will attract and attach to itſelf one or more particles of this vapor, and thereby furniſh itſelf with a non-electric coating, and thus become qualified to receive from any neighbouring object ſuch a ſupply of the electric fluid as its ſtate may demand.

Thus provided, this body of air, together with the vapors which are more or leſs attached to every particle of it, will conſtitute a denſe cloud; and as the air itſelf was before (by ſuppoſition) in a craving or negative ſtate of electricity; and as the vapors are preſumed to have ariſen from the ocean in their natural or neutral ſtate, the whole body of a cloud formed by them will ſtill be in a negative ſtate, and thereby conſtitute a complete thunder cloud; which when formed, if uniform in denſity and contexture, ſhould it be attracted within the *ſtriking diſtance* from any object ſtanding upon the earth, would have its electric equilibrium reſtored at once by a flaſh of lightening darting from the earth: Or ſhould it paſs near another cloud in a different ſtate, the flaſh would reſtore an equilibrium between the two clouds.

That a body of air, either in a poſitive or negativo ſtate of electricity, while pure, ſhould be incapable of communicating its ſurpluſage of the electric element to, or receiving ſupplies from the neighbouring regions, though in a contrary ſtate; and that the ſame air, when replete with watery vapors, may be reſtored to an equilibrium throughout its whole extent by an inſtantaneous diſcharge, may yet require ſome further evidence before it be admitted.

But,

But, as the particles both of air and vapor are ſeverally too minute to fall under our notice, I ſhall endeavour to illuſtrate by analogy what cannot be directly demonſtrated by experiment. In order to this, I ſhall firſt give a general deſcription of, and then ſubjoin ſome obſervations upon doctor *Prieſtley*'s electrical battery.

This battery conſiſted of ſixty four cylindrical glaſs jars fixed in a ſquare box; the jars were coated within and without with tin foil, and the floor of the box was covered with the ſame, whereby the outſides of all the jars formed but one continued electrical ſurface. In like manner, by means of ſmall braſs bars extending over the mouths of the jars in their ſeveral ranges, and by wires which connected the ſeveral bars, together with others which deſcended from them, communicating with the inner coating of each jar, their interior ſurfaces were ſo connected as to form, in the ſame ſenſe, but one ſurface. Thus conſtructed, the whole battery is capable of being equally charged in every part at the ſame time, and of being diſcharged throughout by the ſame exploſion.

Here I would obſerve, that if, inſtead of the metalline coatings, the jars were filled with water to the ſame height with them, and were immerſed in the ſame order in a ſquare veſſel of water to an equal depth, the bars and wire remaining as before, the ſucceſs of all the experiments made with them would be the ſame as above. Let then a battery be conſtructed and charged in this form; after which let the bars and wires aforeſaid be removed, and the water contained in the jars be decanted off by glaſs ſyphons, and let the water be drawn off from the veſſel in which they ſtand. It is evident from the experiment of the charged pane of glaſs already mentioned, and other experiments recited in doctor Franklin's letters, that theſe jars will remain *ſeverally* charged, as they were *jointly* before. They may now, when dry, be taken out and handled at pleaſure with ſafety; nor can they be eaſily re-

ſtored

ſtored to their natural ſtates, but either by immerſing them ſingly under water, or by replacing the whole apparatus and filling both the jars, and the box which contains them, with water as at firſt, and introducing a metalline conductor betwixt the water without the jars and any one of the wires which connect their inſides; then the whole will be inſtantly diſcharged with an exploſion*.

To apply theſe obſervations to the preſent ſubject, we may regard every particle of a body of pure†, but incidentally electrified air, in the ſame light with one of the jars in the battery aforeſaid, which, after having been charged, is deprived of its adventitious coatings: Each particle, like one of thoſe jars, will retain the ſtate it may happen to be in, ſo long as it remains deſtitute of a conducting appendage. But when, and by what means ſoever, a ſufficiency of moiſt vapors ſhall become interſperſed amongſt theſe particles of air to furniſh them ſeverally with non-electric coatings, and by the nearneſs or contiguity of theſe vapors to form a communication from one to another throughout the whole, they will then be in the ſame connected ſtate with the jars in the battery, when complete in every part, and charged; and like thoſe jars be the particles ever ſo numerous, they will be in a capacity of jointly receiving or communicating the electric fire. And as, by the addition of jars in the conſtruction of the battery, the exploſion at the diſcharge may be increaſed indefinitely, ſo will the violence of the exploſion from a thunder cloud be increaſed in proportion to its extent, and to the multitude of aerial particles together with tbeir appendant vapors of which it conſiſts, and which are ſo connected as to be capable of uniting in the ſame diſcharge. But as a thunder cloud is not uſually formed at once, but by degrees, ſmaller clouds generally forming themſelves in

* Theſe experiments I never ſaw particularly made, but the concluſions neceſſarily follow from ſome which I have ſeen, as well as from thoſe pointed out above.

† Pure as to the purpoſes of electricity, or free from conducting vapors; perhaps pure elementary air is not to be found in our atmoſphere.

in ſeparate parties before they join the main body; and as the electrical ſtates of theſe clouds may be very different from each other, from the different electrical ſtates of thoſe parts of the atmoſphere in which they gather; the general equilibrium of the atmoſphere over a country cannot be reſtored by a ſingle diſcharge, but ſucceſſive flaſhes will dart from cloud to cloud, and betwixt theſe and the earth, till at length the whole collected maſs of vapor is ſpent and diſſolved in rain.

Here a common obſervation naturally occurs, viz. that frequently after a flaſh of lightening a ſudden ſhower deſcends in large drops. The mutual attraction between the vapors and the air, when in this electrical ſtate, is ſufficient to ſuſtain the former, notwithſtanding that they are by this attraction greatly condenſed, being as it were forced into a phyſical contact, both with the particles of air, and with each other*. But as ſoon as the air is reſtored to its natural electric ſtate by a flaſh of lightening, this attraction ceaſes, and the vapors precipitate by their own ſpecific gravity in a heavy ſhower.

Long and extenſive calms, in certain latitudes and ſeaſons, take place upon the ocean, during the continuance of which, the heat is ſcarcely tolerable. (See note, page 91.) Where theſe take place the air will naturally undergo the ſame changes, in its denſity and electric capacity, as the air over the land does in the ſummer ſeaſon, and, when ſufficiently

* A gentleman of my acquaintance, who is both intelligent and curious, informed me ſome years ſince, that he was once upon the top of a mountain in Spain, upon which a thunder cloud gathered; that as ſoon as the cloud became inſulated from the mountain it diſcharged a violent tempeſt of thunder and lightening upon the plains below; that he never was ſo thoroughly ſoaked in the moſt violent ſhower as when in the body of this cloud, though without a drop of rain, feeling as if he had been immerſed in a river. This idea is further juſtified by the ſolid appearance of the clouds that riſe in the weſt on a hot ſummer's day, compared with thoſe, which float in the atmoſphere at other ſeaſons; which ſhews a manifeſt difference in their denſity and contexture: And when we obſerve attentively the ſeveral parts of a thunder cloud, the diſtinctneſs of their borders and their ſwelling furbeloes; how ſtrongly they reflect the rays of the ſun, thereby exhibiting the moſt vivid lights and deep contraſting ſhades; and on the other hand obſerve the beautiful effects of their refractive power in the intenſe golden ſkirts which adorn the riſing cloud with a ſetting ſun behind it; we muſt neceſſarily conclude, that, although the vapors of which ſuch clouds conſiſt are collected and condenſed in higher regions of the atmoſphere than are thoſe which uſually form clouds at other ſeaſons, yet their denſity and ſpecific gravity is much greater; and they derive their ſupport from the electric principle.

ſufficiently heated and rarefied, will in like manner aſcend, its place being ſupplied by the denſer air from all quarters without the limits of the calm. This heated and conſequently (granting the principles of the preſent theory) electrical air, when raiſed to a certain height in the atmoſphere, may become as well adapted to the formation of a thunder cloud, from the vapors which are perpetually exhaling from the ſea, as the air over the land under the like circumſtances. Wherefore, in ſome latitudes in all ſeaſons, and perhaps in all latitudes in different ſeaſons of the year, thunder ſtorms may as well happen at ſea, even at remote diſtances from land, as aſhore.

I now proceed to conſider an objection which may be raiſed againſt the foregoing theory, which I ſhall firſt ſtate in its full force, and then endeavour to give a ſatisfactory anſwer to it.

Objection. If the electrification of that body of air in which a thunder cloud is formed depends upon the *heat* it has previouſly acquired, whence is it that thunder ſtorms are frequently attended with ſhowers of hail, which hail is ſometimes ſo large as to indicate its deſcent from the coldeſt regions of the atmoſphere?

Anſwer. Sir *Iſaac Newton* aſſerts from experiments of his own, that " the denſity of the air in the atmoſphere of " the earth is as the weight of the whole *incumbent* air." Conſequently the air gradually decreaſes in denſity from the ſurface of the earth to the top of the atmoſphere. The body of air which is ſuppoſed in this *theory* to be qualified by the action of heat upon it, to become a proper *ſubſtratum* for the formation and ſupport of a thunder cloud, is thereby expanded and rarefied, and thence becomes ſpecifically higher than it was before: It therefore aſcends till it arrives at that height in the atmoſphere at which the air is naturally, from its ſituation, of the ſame rarety with itſelf; and there it reſts in equilibrio. This region is extreamly cold at all ſeaſons, as appears from the teſtimonies of of travellers who have viſited the tops of very high mountains, even under the line. The greater the heat which this body of air acquires below, the greater degree of rarefaction it undergoes, and the higher, of conſequence, it aſcends in the atmoſphere, where the cold is proportionably more ſevere than is uſual near the ſurface of the earth. But though it was the heat which it acquired below that firſt rarefied and expanded it, it will by no means be proportionably recondenſed by the cold which it meets with in its aſcent; for as the heat which occaſioned its rarefaction decreaſes upon that account, the preſſure of the incumbent atmoſphere upon it decreaſes as it riſes, whereby its denſity may, upon the whole, remain nearly the ſame; if ſo, may we not ſuppoſe its electrical ſtate alſo, previous to the formation of the cloud, to continue nearly the ſame? For ſhould this warm air aſcend all together as in a body, without intermixing with the denſer ſurrounding air through which it riſes, as a bubble of air does in any other fluid, and as *this* air probably would in a calm ſeaſon, the denſer parts of the atmoſphere eaſily giving way to it, till it arrives at that region the denſity of which is equal to its own, where it would be at reſt; ſhould this, I ſay, be the caſe, it would not, even in that cold region, cool ſo ſuddenly as to undergo any immediate change in its electrical ſtate, from the natural coldneſs of the region; neither would it be from condenſation, its denſity remaining nearly the ſame, as obſerved above.

But when the cloud is formed, or rather when a number of clouds are forming in the neighbourhood of each other, and joining their forces preparatory to the tempeſt, a general confuſion takes place in the atmoſphere; various and even contrary currents of air flowing promiſcuouſly hither and thither, as is evident from the viſible irregular motions of detached parts of the clouds. In this general effort of nature to reſtore an equilibrium, ſome of theſe aerial currents will probably introduce air, which having been till now at a diſtance from the ſcene of action, has ſuffered no material change in its *natural* electric ſtate*; and is on the contrary fraught with all the cold which is natural to the region of the atmoſphere from whence it came. In falling through this adventitious current of air, the drops of rain, precipitating from the body of clouds above, are congealed into ice, and deſcend in hail, which as it falls collects other ſnowy or icy particles round it; a hail-ſtone when it comes to the ground reſembling denſe ſnow with a nucleus or kernel of ſolid ice in the middle.

That the air which this hail-ſtone falls through is colder than the region from whence it deſcends, may be thus proved, viz. If the freezing took place where, and as ſoon as the vapors were firſt ſet at liberty by a flaſh of lightening, it would be impoſſible for them ever to unite into drops, but they muſt deſcend in the fineſt chryſtals, an aſſemblage of which conſtitutes a flake of ſnow; the nucleus, or proper hail-ſtone then muſt have been firſt a fluid drop, and afterwards congealed in its fall through a colder region than that in which it was formed.

It may be further objected, that a thunder cloud, in the eaſtern parts of America, always makes its firſt appearance in the weſt, over the land, its progreſs being *towards* the ſea; which ſeems to contradict the ſuppoſition in the theory, that the vapors of which it conſiſts are chiefly ſupplied *from* the ſea.

To which I anſwer, 1. That a thunder cloud is with us very rarely, indeed ſcarcely ever formed in the weſt, without a ſea-breeze ſpringing up previouſly from the eaſt, 2. That the ſea air, as obſerved before, always abounds with vapors, although from the cauſes already aſſigned, they are uſually, at their firſt riſing, inviſible. 3. That the firſt appearance of a cloud will always be where the vapors are

* This ſuppoſition will be juſtified by conſidering, that ſuch is frequently the ſtate of the atmoſphere, that the thunder clouds which are formed in it are but of ſmall extent; notwithſtanding which, the change in the ſtate of the air occaſioned by them is perceived to the diſtance of many leagues round.

are first collected into a body and condensed, and thereby rendered visible, which in a thunder cloud will be in the west, notwithstanding the vapors of which it consists may chiefly have arisen from the sea. 4. That when a thunder cloud is once formed it will be in a state of attraction with the earth in general, and more especially so with all substances which are natural conductors of the electric fluid, such as the water contained in rivers, bays, arms of the sea, &c. and by these the course of a thunder cloud is known to be very sensibly affected.

But the ocean is the grand object towards which its course will be directed; accordingly the progress of the clouds is from the western horizon, eastward, be the weather below what it may, not excepting the most violent easterly storms, which are sometimes, though but rarely, accompanied with thunder and lightening.

To the foregoing observations I would add, 5. That when an extensive thunder cloud is forming in the atmosphere by means of the mutual attraction of the condensing vapors, and the body of electrised air which sustains and condenses them, the increasing density of the whole compound mass of air and vapor will, by degrees, occasion its redescent towards the earth, from the law of gravity; it will also be attracted by, and move towards the ocean, upon the principles of electricity; the cloud will then descend obliquely, in a diagonal between the directions of these two powers; and both, continually acting upon it, will jointly accelerate its motion. Such a cloud, if dense and large, would end in a perfect tornado, either upon the land or water, as thunder showers frequently do; smaller clouds being also, usually, accompanied with gusts or flurries of wind.

I shall here add one observation more which I have frequently made, and which may tend to confirm the foregoing theory, viz. That as the general course of the eastern coast of north America is from north-east to south-west;

the courſe of a thunder cloud is uſually from the north-weſt, with the wind at ſouth-eaſt, perpendicular to the direction of the coaſt, and contrary to each other.

Inland ſeas and great lakes, ſuch as are thoſe in North-America, may anſwer the ſame purpoſes in the interior parts of the country, as the ocean does near the limits of the continent; both by affording the neceſſary ſupplies of vapors for the formation of the clouds, and by their attractive influence upon thoſe clouds when formed.

I now conclude with a few hints, which I ſhall throw into the form of queries.

1. Whatever the primary cauſe of evaporation may be, does not the formation of vapors into diſtinct clouds depend upon the electrical ſtate of the atmoſphere?

2. Were the atmoſphere always uniformly electrical could we have any rain*; in that caſe, if evaporation be performed independent of electricity, ſhould we not be invelloped in everlaſting fogs?

3. Mr. *Canton* ſuppoſes that the *aurora borealis* may be "the flaſhing of electric fire from poſitive towards negative clouds, throughout the upper part of the atmoſphere." But as the air is uſually charged more or leſs with vapors, even when perfectly pellucid; and as the moſt remarkable *auroræ* frequently appear without a cloud in the hemiſphere, may not this phenomenon be rather occaſioned by the "flaſhing of electric fire," from one region or body of air to another in a different ſtate of electricity, through the intervening vapors?

4. May not the reaſon of its uſual appearance in the north and of its flaſhing ſouthward be, that, in every northern latitude, the air to the ſouthward is at all ſeaſons of the year, *cæteris paribus*, more affected by the heat of the ſun than the air northward of the ſame latitude; and does not this occaſion an electrical current to flow from north to ſouth,

* Signior *Beccaria* concludes from experiments, that gentle rains are the effects of a moderate, as thunder ſhowers are of a more plentiful, electricity.

ſouth, ſo often as the above mentioned circumſtances concur, though with ſome interruption from the irregular diſpoſition of the conducting vapors; and may not this occaſion thoſe gleams and ſtreams with which this phenomenon is uſually attended?

N° VIII.

Theory of Water Spouts, by ANDREW OLIVER, *Eſquire, of Salem in the State of Maſſachuſetts.*

IN my laſt I took the liberty to communicate to the Philoſophical Society a Theory of *Lightening* and *Thunder Storms*, which was ſuggeſted to my mind upon the peruſal of doctor *Prieſtley's hiſtory of electricity.* In the inveſtigation of which theory, while I was endeavouring to account for the exhibitions of thoſe phenomena upon the ocean, at great diſtances from the land, ſome thoughts naturally occurred relative to the *water ſpout;* a phenomenon as curious perhaps as any one in nature, and which can rarely take place but at ſea.

WATER SPOUTS have by ſome been ſuppoſed to be merely electrical in their origin; particularly by ſignior *Beccaria*, (*Prieſtley*'s hiſt. of elect. p. 355, 356) who ſeems to have ſupported his hypotheſis by ſome experiments. But as ſeveral ſucceſſive phenomena are neceſſary to conſtitute a complete water ſpout, (ſome of which undoubtedly depend upon the electric principle) if we attend to the moſt authentic deſcriptions of theſe ſpouts, through their various ſtages, from their firſt exhibition to their total diſſipation, we ſhall be obliged to have recourſe to ſome other principle, in order to obtain a complete ſolution. I ſhall therefore, *firſt*, deſcribe theſe phenomena according to the beſt obſervations I have met with; and *then*, endeavour to give

give a general philosophical solution of them. But I must here observe, that the following descriptions are all taken from the accounts of mariners, who are indeed the only persons that have opportunities of viewing them; but, unfortunately for the cause of philosophy, do not usually observe them with that circumstantial accuracy, respecting the previous and subsequent states of the atmosphere, which may be necessary to found a complete physical solution upon; nor with any view to that end, as it is foreign to their main business, trade and commerce. But as such accounts are the best I have met with even in the Transactions of the Royal Society down to 1744, lower than which I have not seen them; from such I shall endeavour to draw the best conclusion which the nature of the evidence will justify.

The most intelligent and beautiful account of a water spout that I ever met with, is in the abridgment of the Phil. Trans. vol. VIII, by *Martin*, pa. 655, as it was observed by Mr. *Joseph Harris*, May 21, 1732, about sunset, lat. 32° 30′ N. long. 9° E. from cape Florida; which I shall here transcribe.

" When first we saw the spout (says he) it was whole " and entire, and much of the shape and proportion of a " speaking trumpet; the small end being downwards, and " reaching to the sea, and the big end terminated in a black " thick cloud. The spout itself was very black, and the " more so the higher up. It seemed to be exactly perpendicular to the horizon, and its sides perfectly smooth, " without the least ruggedness. Where it fell the spray " of the sea rose to a considerable height, which made " somewhat the appearance of a great smoke. From the " first time we saw it, it continued whole about a minute, " and till it was quite dissipated about three minutes. It " began to waste from below, and so gradually up, while " the upper part remained entire, without any visible alteration, till at last it ended in the black cloud above.

" Upon

"Upon which there ſeemed to fall a very heavy rain in "that neighbourhood. There was but little wind, and "the ſky elſewhere was pretty ſerene——"

In other accounts contained in the Philoſophical Tranſactions, theſe phenomena are deſcribed as having the appearance of a ſword pointing downwards, ſometimes perpendicularly ſometimes obliquely towards a column of water or froth, which ſeems to riſe out of the ſea to meet it, attended with a violent ebullition or perturbation at the ſurface. Again in others the appearance is compared to ſmoke aſcending viſibly as through the funnel of a chimney, either directly, or with a ſpiral motion, which according to the fancies of ſome reſembles the aſcent of water in the ſcrew of *Archimedes;* by ſuppoſing ſomething ſimilar to which in the atmoſphere, they have endeavoured to account for the riſe of the water from the ſea in a water-ſpout. To which I would add, that, from the relations of ſome perſons who uſe the ſea, with whom I have converſed upon the ſubject, I find that it is no uncommon thing, during a calm below, and a ſerene ſky above, to obſerve at the diſtance of two or three leagues a ſmall cloud hovering in the air, from whence the commencing ſpout ſeems to dart downward to the ſea, upon which the uſual phenomena take place in their order. I have alſo been informed (and to information I muſt truſt, having never been at ſea) that it is common during theſe appearances for ſhips to ſail, even within hail of each other, with different winds; and within the limits of the ſame viſible horizon, with contrary winds: And laſtly, that the riſe and progreſs of this phenomenon is ſometimes ſo rapid, that, even in a ſerene ſky, a few minutes will be ſufficient to generate a cloud from one of theſe ſpouts, and to diſcharge from thence a heavy ſhower of rain.

Before I proceed to attempt a philoſophical ſolution of theſe curious productions of nature, in which the two principal fluids of our globe, air and water, are largely concerned;

concerned; it may be neceſſary to make ſome obſervations upon the nature and properties of fluids in general, as ſuch.

1. No fluid can be at reſt unleſs every part of it reſpectively be acted upon by an equal force or preſſure in every direction, till when its ſeveral parts will neceſſarily recede from the greater preſſure towards the leſſer, nor can an equilibrium take place.

2. If two or more fluids of different natures and denſities come together, ſuch as quickſilver, water, oil and air, which will not mix; they will take their places according to their ſpecific gravities, the denſeſt remaining at the bottom.

3. If a veſſel be filled with either of theſe fluids, and a denſer be admitted into it, the latter will expel, and take place of the former.

4. If an empty cylindrical ſpace be ſurrounded on all ſides by a fluid, which is excluded by ſome reſiſting ſurface terminating that ſpace, the fluid will neceſſarily, upon the ſudden removal of the obſtacle, immediately flow in from every ſide towards the center of the void; and as it flows inwards the parts next ſurrounding this ſpace will thereby be crowded together, and force each other upwards, till at length when cloſed, the fluid will by its aſcent have formed a column directly over the middle of the ſpace, to a height proportionable to the united force of the converging currents. This muſt be the caſe with every fluid thus flowing into a vacuum; and in a leſſer degree when a denſer fluid in a ſimilar ſituation ſupplants a rarer: And the greater the difference of the denſities of the two fluids might be, the more conſpicuous would be the effect.

This reaſoning may be illuſtrated, and the concluſions exemplified by facts which muſt have occurred to the obſervation of every one. Do we not obſerve when a ſhower of hail, or rain in large drops, falls upon the ſurface of ſtagnant water, that the water riſes wherever they fall, like ſo many little inverted icicles, which again

inſtantly

inſtantly ſubſide ? The cauſe of which undoubtedly is, that theſe drops, or hail-ſtones, deſcending from a great height in the atmoſphere, acquire ſeverally ſuch a momentum in their fall as to plunge through the ſurface to a proportional depth, driving the ſuperficial water back on every ſide, and leaving a momentary vacuum behind them; not indeed a pure vacuum, but ſuch, relative to the ſurrounding fluid, which immediately returns to fill up the chaſm, and, as it cloſes, gathers and riſes in the little columns above deſcribed. When a large round ſtone, or any other heavy body plunges, the effect is proportionably greater.

5. Let us, for argument's ſake, ſuppoſe the atmoſphere over any certain circular tract of ocean of ſome miles in diameter, to be for a moment annihilated, the ſpace it occupied before being reduced to a pure vacuum: The ſurrounding atmoſphere, when at liberty, would ruſh in from every quarter towards the centre, where the converging currents would immenſely croud each other, and force up a vaſt quantity of air through a very narrow funnel, contracted below by the united preſſure of thoſe currents from all ſides, into the higher regions; which funnel, as the denſity of the air leſſens according to its height, and the ſurrounding preſſure which contracts it muſt decreaſe nearly in the ſame proportion, would more and more diverge and expand the higher it roſe above the ſurface of the ſea. This would be attended with a moſt furious blaſt of wind up to, and far above the top of the atmoſphere. In like manner,

6. If inſtead of a pure vacuum, or a total annihilation of ſuch part of the atmoſphere, we ſuppoſe the ſame to become, by any means whatever, ſpecifically lihgter than the ſurrounding regions, the effect would be the ſame as above, in kind, though not in degree; the denſer air flowing in, but with leſs rapidity, from all quarters without, expelling the lighter and ſupplying its place, as in article four; upon which alſo a large quantity of this confluent air, for the

ſame reaſon, would be driven up with violence through a like narrow vent, yet not with the ſame impetuoſity, nor to the ſame height as if forced through this funnel into a pure vacuum.

That the atmoſphere over large tracts of ſea or land may thus become ſpecifically lighter than that over the ſurrounding regions, will be evident, if we conſider, 1. That heat has a natural tendency to rarefy and expand the air upon which it acts. 2. That the atmoſphere over our heads does not conſiſt of mere elementary air, but is an univerſal receptacle of all the heterogeneous vapors and effluvia that are perpetually exhaling from every ſubſtance that exiſts upon the face of the earth, whether animal, vegetable or mineral. 3. That, by the caſual diſpoſition of theſe vapors and effluvia in the atmoſphere, the air, which is, of itſelf, naturally enough diſpoſed to acquire heat from the paſſage of the ſun's rays through it, may become more diſpoſed to imbibe and retain that heat, in one region, than in another in its neighbourhood; which, from the intervention of clouds, or from its purity and freedom from thoſe ſteams and vapors with which the former is charged, may, in a great degree, retain its natural coolneſs and denſity, while the other becomes heated, rarefied and expanded, and is thereby rendered ſpecifically lighter.

That theſe different affections of the atmoſphere *actually* take place, and diſpoſe the air, at one time and in one place, even in the ſame ſeaſons of the year, to imbibe and retain the heat excited by the ſun's rays, more than at another, is not a matter of mere conjecture; but, whatever the cauſe may be, is notorious to all perſons of obſervation.

Theſe things being premiſed, I beg leave to obſerve further, that ſome parts of the ocean are liable to long and extenſive calms, during the continuance of which the heat is ſcarcely tolerable. Where theſe take place the air muſt neceſſarily undergo proportional changes in its denſity and

electric

*electric capacity** ; and when heated and rarefied to ſome certain degree will give way, as obſerved above, to the denſer air, now proportionably diſpoſed to flow in from all quarters without the limits of the calm.

When once this ſtagnated air, eſpecially if of any great extent, becomes ſpecifically lighter than the ſurrounding air, and ſufficiently rare to be ſupplanted by it ; the latter will, of courſe, ſet it from every ſide in horizontal currents; which will flow, either directly, or obliquely, towards one point, in or near the centre of the becalmed region aforeſaid ; the obliquities of which currents will depend upon the directions and velocities of the winds, or currents of air which might previouſly have taken place in the ſurrounding regions. When theſe currents arrive at the centre of their mutual convergency, all the ſtagnated and rarefied air which was before incumbent upon the calm ſurface of the ſea, will have been expelled and forced higher up into the atmoſphere; upon which theſe currents, by their mutual concourſe in one place, will exceſſively croud each other, as obſerved above, wherever it happens, driving the central air upwards with a violent blaſt ; which, ſhould the currents ſet in obliquely, and ſo converge with a ſpiral motion towards the centre of their mutual concourſe, would aſcend as through the ſcrew of Archimedes, or the worm of a cork-ſcrew, to both of which navigators have likened theſe ſpouts : Otherwiſe it would riſe through a ſtrait, narrow funnel, as in articles five and ſix above ; which if filled with any opaque matter would become viſible, and at a diſtance would reſemble a ſpeaking trumpet with the ſmall end downwards, in which form the water ſpout frequently appears. In the former caſe a whirlwind round about the centre would undoubtedly be the conſequence ; and in either, a water ſpout would probably be produced†.

O 2 For

* See Theory of Lightening, &c. page 81.

† We ſhall in the ſequel ſee abundant reaſon to conclude with doctor *Franklin* and others, that water ſpouts at ſea and whirlwinds on the land (ſome ſpecies of them at leaſt) are produced by the ſame cauſes.

For the preſſure of the atmoſphere is taken off from that part of the ſurface of the ſea, which is directly under the funnel through which the air is driven up; whereas the ſurrounding ſurface is at the ſame time uncommonly preſſed, from the confluence of the currents from all quarters*, whereby the water muſt neceſſarily be forced up to a certain height, proportional to the ſurrounding preſſure, through the ſame funnel with the air itſelf, nor is this all, for in their aſcent the air and water become confuſedly mixed together, whereby the latter is broken and attenuated into the fineſt globules and particles, as when one forcibly blows water out of his mouth; and from this mixture of the two fluids doubtleſs ariſes that opacity which renders the ſpout viſible.

This opaque column of air and water, together with the paſſage through which it aſcends, will expand as it riſes, in proportion as the compreſſure diminiſhes; and, to ſpectators at too great a diſtance to diſcern the narrow ſtem next the water, will reſemble a ſword, or acute cone pointing downwards from a ſmall cloud; to which they are frequently likened. But that they do at the ſame time communicate with the ſea is evident from the perturbation of the water directly under them, which ſometimes boils and foams at a great rate. This is uſually the firſt appearance of one of theſe ſpouts, the duration of which is either longer or ſhorter, and the ſubſequent phenomena more or leſs conſiderable, according to the extent of the cauſe, and the mode of its operation.

The water being thus raiſed from the ſea, and forced irreſiſtably upwards in the fineſt globules by the protruding air, arrives at length at the warm electrical air † lately expelled,

* In the abridgment of Philoſophical Tranſactions, vol. II. (by Eames and Martin) page 61, at the bottom, it appears, that the meeting of two contrary currents of air or contrary winds, raiſes the mercury in the barometer near the place where it happens, which indicates an increaſe of the preſſure of the atmoſphere upon the ſurface of the earth or ſea. How much more then muſt that preſſure be increaſed, from a general confluence of the air from all quarters towards one ſpot?

† See Theory of Lightening, &c. page 90.

expelled, which was previoufly incumbent upon the calm furface beneath; the electric attraction of which probably affifts the further afcent of thefe particles after the firft fury of the blaft is fpent. There it undergoes another operation being converted into vapor, whereby it is wholly difcharged of the marine falts it carried up with it*; which are now left to fhift for themfelves, together with innumerable other heterogeneous corpufcles which fucceffively float in the atmofphere, and which in due time, become feverally fubfervient to many wife purpofes in the œconomy of nature. Thefe vapors will then be greedily attached by the craving particles of this air, now deficient of its natural quantity of electric matter†, and form a denfe cloud, in like manner as thunder clouds are formed over the land; but with much greater expedition, as the fupply of vapors is more fudden. This cloud will then be ready in a fhort time to difcharge a fhower of frefh water upon the fea from whence it rofe, and may be attended with thunder and lightening, or not, as the air in which the cloud was formed was more or lefs electrical, or the cloud extenfive.

A previous calm may not be *neceffary* to the production of thefe phenomena, and indeed they frequently happen without one: But, upon the fame principle, if it be calmer where they are produced, or the ftate of the atmofphere there be fuch as to difpofe it to acquire and retain the heat acquired from the fun's rays, more than in the furrounding regions, which, as we have feen above, may be the cafe, the effects may be the fame in kind, though perhaps not in degree; the moft perfect water fpouts probably rifing from whence there has previoufly been a dead calm, or nearly fuch, for the foregoing reafons.

If

* The water carried up in one of thefe fpouts is undoubtedly falt when it firft rifes from the fea, as it afcends in great quantities, and in a very denfe column; but it is always frefh when it defcends again in a fhower: It muft therefore in the mean time have gone through a compleat natural diftillation.

† Theory of Lightening, &c. page 92.

If there be any wind at the time of the phenomenon, the aerial funnel through which the water afcends, inftead of being perpendicular to the horizon, as it would be in a calm, might incline more or lefs to it, in proportion to the ftrength or weaknefs of the prevailing current of air: Or, inftead of continuing in one fpot, it might have a progreffive motion over the furface of the fea, in the direction of the general current; both of which circumftances frequently take place. In either cafe it is natural to fuppofe, that both air and water would afcend fpirally, as through the worm of a fcrew, every current which fets in towards the centre receiving an oblique bias from the prevailing current.

It fometimes happens, that after the fubfiding of a fpout, it is fucceeded by a fecond, and that by a third, either in the fame place, or at no great diftance from it. But this alfo is analogous to what we obferve upon the plunging of heavy bodies out of air into water. For, after the firft fubfiding of the fmall column of water which is occafioned by it, and is above refembled to an icicle, the water again rifes and fubfides as at firft, though not in the fame degree, as may be concluded from thofe fainter concentric circles which expand from the fame centre after the fubfidence of the firft column. The fame thing which here takes place in water, may alfo take place in air, under fimilar circumftances.

Since writing the foregoing, while I was endeavouring to contrive fome experiment to illuftrate the fubject, a very fimple one was fuggefted to my mind, the fuccefs of which I think demonftrates the truth of the hypothefis introduced above to account for the firft afcent of the water in the fpout; the event being precifely the fame as was expected before hand, and as ought to have taken place upon the principles above advanced.

EXPERIMENT.

In a ftiff paper card I made a hole juft big enough to infert a goofe quill fo as that it might be fixed perpendicularly

cularly to the plane of the card: After cutting the quill off ſquare at both ends and fixing it, I laid the card upon the mouth of a wine glaſs, filled with water to within one fifth or ſixth part of an inch from the lower orifice of the quill; then applying my mouth to the upper part, I drew out the air in the quill by a ſtrong ſuction, and in one draught of my breath drew in about a ſpoonful of the water; this by ſtronger ſuctions I was able to repeat again and again, the quill remaining as before. The water, as I expected, did not aſcend to the mouth in a ſtream, as it would have done had the quill reached below the ſurface; but broken and confuſedly mixed with the air which aſcended with it; as is above ſuppoſed to be the caſe in the aſcent of water in a ſpout at ſea.

In this experiment the ſuction occaſioned a vacuum, or at leaſt a great rarefaction of the air, within and directly under the quill; the ſurrounding air of courſe flowed in from every quarter to ſupply it, ruſhing up into the quill, and through it to the mouth; the preſſure of the atmoſphere being thereby taken off from the ſurface of the water immediately under the orifice, while the preſſure upon the ſurrounding ſurface remained, and was probably increaſed, the water was forced up together with the air as above notwithſtanding the quill had no manner of communication with the water. If the ſuction be made very ſtrong, and the quill be fixed at the diſtance of a quarter of an inch or more from the water, a conſiderable agitation and ebullition takes place in the water under it, ſimilar to that obſerved in moſt natural water ſpouts, and the paſſage of the water from the ſurface to the quill becomes very viſible.

It was hinted in a preceding note, that water ſpouts at ſea and whirlwinds at land, ſome ſpecies of them at leaſt, ariſe from the ſame cauſe, how different ſoever their apparent effects may be. This I think is made ſufficiently evident from the obſervations of a couple of land ſpouts at *Hatfield*

Hatfield in *Yorkshire*, by Mr. *Abr. de la Pryme**, whose accounts of them I shall here transcribe, as the Transactions of the Royal Society are in the hands of but few among us, and as the facts related by him tend strongly to confirm the present theory, however his conclusions from them may differ from it.

" On the 15th of August, 1687, (says he) appeared a " spout in the air at *Hatfield* in *Yorkshire*; it was about a " mile off coming directly to the place where I was; I " took my prospective glasses to observe it as well as I " could.

" The season was very dry, the weather *extreme hot*, " and the air very cloudy; the wind aloft, and pretty " strong, and (which is remarkable) blowing out of seve- " ral quarters at the same time, and filling the air here- " abouts with mighty thick and black clouds, layer upon " layer; the wind thus blowing soon created a great *vor-* " *tex*, *gyration* and *whirling* among the clouds; the cen- " tre of which every now and then dropt down in the " shape of a thick, long, black pipe, commonly called a " spout; in which I could distinctly view a motion like " that of a screw, continually drawing upwards, and screw- " ing up (as it were) whatever it touched. In its progress " it moved slowly over a hedge-row and grove of young " trees which it made to bend like hazle wands, in a cir- " cular motion; then going forward to a great barn it " twitched off in a minute all the thatch, and filled the " whole air therewith. Coming to a very great oak tree, " it made it bend like the foregoing trees, and broke off " one of the greatest and strongest branches that would " not yield to its fury, and twisting it about, flung it to a " very considerable distance off; then coming to the place " where I stood, within three hundred yards of me, I be- " held this odd phenomenon, and found that it proceeded " from nothing but a *gyration of the clouds by contrary* *winds*

* Abridgment of Philosophical Transactions, vol. IV. by Jones, page 106, 107.

" *winds meeting in a point or centre;* and where the great-
" eſt condenſation and gravitation was, falling down into
" a pipe or great tube (ſomething like the *cochlea Archi-*
" *medis)* and that in its working or whirling motion, ei-
" ther ſucks up water, or deſtroys ſhips, &c. Having tra-
" velled about a quarter of a mile farther, it diſſolved by
" the prevalency of the wind that came out of the eaſt."

The account of the other is as follows, viz. " I have
" ſeen another ſpout in the ſame place, which very much
" confirms me in my notion of the origin and nature of
" them.—The 21ſt of June, 1702, was *pretty warm;* on
" the afternoon of which day, about two of the clock, *no*
" *wind ſtirring below* though it was ſomewhat great in the
" air, the clouds began to be mightily agitated and driven
" together; whereupon they became very black, and were
" (moſt viſibly) *hurried round*, from whence proceeded a
" moſt audible whirling noiſe, like that commonly heard
" in a mill. After a while, a long tube or ſpout came
" down from the centre of the congregated clouds, in
" which was a ſwift *ſpiral motion* like that of a ſcrew, or
" the *cochlea Archimedis* when it is in motion, by which
" ſpiral nature and ſwift turning, water aſcends up into
" the one as well as into the other. It travelled ſlowly
" from weſt to north-eaſt, broke down a great oak tree or
" two, frighted ſome out of the fields, and made others
" lie down flat upon their bellies, to ſave being whirled
" about and killed by it, as they ſaw many jackdaws to
" be, that were ſuddenly caught up, carried out of ſight,
" and then caſt a great way amongſt the corn; at laſt it
" paſſed over the town of *Hatfield*, to the great terror of
" the inhabitants, filling the whole air with the thatch that
" it plucked off from ſome of the houſes; then touching
" upon a corner of the church, it tore up ſeveral ſheets of
" lead, and rolled them ſtrangely together; ſoon after
" which it diſſolved and vaniſhed without doing any fur-
" ther miſchief.

" By all the obfervations that I could make of this, and " the former, I found that had they been at fea and joined " to the furface thereof, they would have carried a vaft " quantity of water up into the clouds, and the tubes would " then have become much more ftrong and opaque than " they were, and have continued much longer.

" It is commonly faid that at fea the water collects and " bubbles up a foot or two high under thefe fpouts before " that they be joined: But the miftake lies in the pellu- " cidity and finenefs of thofe pipes, which do moft certain- " ly touch the furface of the fea before that any confider- " able motion be made in it, and that, when the pipe be- " gins to fill with water, it then becomes opaque and " vifible."

I fhall here make a remark or two upon the above cited author's mode of expreffion in the foregoing accounts, which is evidently adapted to a preconceived idea of the *cochlea Archimedis*, by fuppofing fomething fimilar to which, as taking place in our atmofphere, he is not alone in endeavouring to account for thefe phenomena. In conformity to this idea he fpeaks of the fpout as *drawing upwards*, and *fcrewing up* whatever it touched; and fuppofes that by its *fpiral motion* and *fwift turning*, water afcends in it as in the *fcrew of Archimedes*. But this hypothefis, however fpecious, has been long fince exploded as unphilofophical.

Mr. *de la Pryme* mentions the appearance of a long black pipe which now and then dropped down from the centre of the gyrating clouds; in which pipe he diftinctly viewed a motion like that of a fcrew; and as fuch he feems to have fuppofed it acted, viz. either in the manner of a corkfcrew upon folids; or as the *cochlea Archimedis* upon fluids, drawing them up into the atmofphere. But as he himfelf afterwards, when applying his obfervations to a fpout at fea, very juftly concludes that the pellucidity and finenefs of thefe pipes over the water render them invifible below,

" notwithftanding

" notwithſtanding (as he conceives) that the pipes do moſt " certainly touch the ſurface of the ſea before any conſider- " able motion be made in it, and that they are then rendered " opaque and viſible when they begin to fill with water;" might he not with equal reaſon have ſuppoſed that thoſe aerial pipes which he obſerved over the land were alſo continued from the clouds down to the ſurface of the earth, as from their effects below, one would naturally conclude they were, and that they were pellucid and inviſible ſo long as they contained nothing but air; but that " every " now and then," when they met with any ſubſtances which might perchance paſs within the compaſs of their gyration, or which they could eaſily carry up; ſuch as detached parts of the broken clouds; water from ſtagnant ponds, brooks and rivers, hay, ſtubble, thatch, duſt, &c. they then become opaque and viſible, and that they appeared to dart downwards by a kind of optical deception? For upon the foregoing principles theſe pipes of air muſt neceſſarily be broadeſt above, as we have already ſeen, and terminate in a narrow ſtem below, the broadeſt part being, at a diſtance, firſt viſible, and the ſhank ſeemingly tapering downwards to a point. It is however certain from the effects of the above mentioned ſpouts, that, whatever the appearances were *aloft*, they were all occaſioned by the ruſhing of the air upwards through a narrow paſſage, that was contracted *below*, by the concourſe and preſſure of the oppoſite currents of that fluid, and dilated above from the diminution of that preſſure.

I have reſerved for this place an account of a curious ſpout which made its appearance *anno* 1694, not at ſea, but in the harbour of *Topſham**, and at low water; which paſſed with a ſlow progreſſive motion over both land and water; acting as a complete water ſpout over the latter, and as a whirlwind upon the former: For when it paſſed over the channel of the river, it threw up the water in a denſe

 ſtream,

* Lowthorp's Abridgm. Phil. Tranſ. vol. II. page 104.

ſtream, as if it had been impelled through the hoſe of a fire engine, and the ſtream accordingly ended in a thick miſt, reſembling a dark ſmoke; the ſurface of the water, round about the ſpot from whence it roſe, being greatly agitated, as is uſual in thoſe phenomena. In its courſe it met with the hull of a new ſhip of about one hundred tons, which was much ſhaken by it, but received no hurt. In paſſing over the flats it took hold of a boat which was faſtened to an anchor, whirled both boat and anchor to ſome height in the air, and rent the boat "from the *head* "to the *keel*." When it reached the ſhore it lifted up another boat about ſix feet from the ground, letting it fall again upſidedown; and had a ſtrange effect upon a parcel of planks, ſome of which were raiſed up perpendicularly, and ſtood upon their ends while it paſſed along; and in its further progreſs it was attended with the uſual effects of a whirlwind, ſuch as ſtripping off, not only thatch, but ſheets of lead from the tops of houſes, and tearing off the limbs of trees. This account may tend to confirm the theory here offered, as it proves to a demonſtration, that the water ſpout therein deſcribed, was occaſioned by a previous whirlwind in the atmoſphere; which whirlwind was alſo occaſioned by the ruſhing of a large quantity of air, upwards, from all quarters near the ſurface of the earth, through a very contracted aerial paſſage, towards the top of the atmoſphere; the narrowneſs of which paſſage, as determinable from the effects obſerved in its progreſs, ſhews it to have been compreſſed upon all ſides by a general conflux of oppoſite currents of air; as the ruſhing of the air through it with ſuch violence from beneath, does, that the denſity of the fluid and the compreſſive force of the currents were greateſt there. The aſcending air carried up the water with it through the ſame paſſage; not by any mechanical operation upon it, like the action of a ſcrew of any kind; but, merely, by taking off the preſſure of the atmoſphere from the ſurface of the water directly under it;

it; whence the water muſt neceſſarily aſcend, as in any common hydraulic machine; and that with a force proportional to the preſſure of the atmoſphere upon the ſurrounding ſurface, now greatly increaſed by the confluence of thoſe currents.

Before I cloſe this ſubject, I ſhall juſt mention, without making any remarks, the effects which a whirlwind had amongſt a number of ſhocks of corn at *Warrington* in *Northamptonſhire*, Auguſt 1ſt, 1694; out of which from eighty to a hundred ſhocks were carried up into the air, a great part of them out of ſight; theſe when the fury of the blaſt was ſpent, fell down again at the diſtance of ſome miles from their own field. The account of this whirlwind immediately precedes the article laſt quoted from the *Philoſophical Tranſactions*. Should the foregoing theory be adjudged tenable, it will render very credible thoſe ſtrange accounts which we have ſometimes had, of its raining tadpoles and frogs, which have been found upon the tops of houſes after a ſhower; and even ſmall fiſhes, a ſhower of which fell at *Cranſtead* near *Wrotham* in *Kent*, *anno* 1696, on the Wedneſday before Eaſter (Lowthorp's abridgement of Philoſophical Tranſactions, vol. II. page 144.) For ſhould one of thoſe aerial pipes paſs over a frog pond, or the ſhallow parts of a fiſh pond, the ſame natural cauſe which in a ſpout at ſea, would carry up the water from the ocean, would alſo carry up the water from the ponds aforeſaid, together with the contents; whether tadpoles, frogs or fiſhes: Theſe muſt deſcend again ſomewhere; and whereever they fell, a ſhower of fiſhes, frogs or tadpoles, would be the conſequence.

Experiments

N° IX.

Experiments on Evaporation, and Meteorological Obſervations made at Bradford in New-England, in 1772, *by the Rev.* SAMUEL WILLIAMS, A. M.

IN making experiments on the quantity of water that evaporated in the year 1771, the method I uſed, was to fill the veſſel the beginning of every month: In the courſe of theſe experiments, I obſerved that in the beginning of the month when the tube was newly filled, it exhauſted much faſter than towards the latter end, when one or two inches of the water was evaporated; and that the quantity of evaporation meaſured this way, came out leſs than the quantity of rain that fell in the courſe of the year. The beginning of the year 1772, I attempted to examine this matter more carefully. With this view I made the following experiments.

EXPERIMENT I.

I procured two cylindrical veſſels of three inches diameter, and ſix deep, as much alike as they could be made: One, I filled with water as I had done in 1771, once a month; the other, with the ſame kind of water, once a week; and placed them about ſix inches apart, in ſuch a manner as to be expoſed to the wind, and ſun, but covered from the rain. The reſult was, that which was filled once a week, exhauſted about one third more than the other. In *January* and *February*, the difference was a little leſs; in *March* and *April*, it was a little more. In *May*, the laſt month in which I compared them, the evaporation from the former was 6.35 inches; from the other 4.10. By this experiment

experiment I was convinced that it never could be known with much accuracy by either of theſe methods, what quantity of water does really evaporate from the ſurface of ſeas, lakes and rivers. For in the one caſe, after about an inch is exhauſted the ſurface of the water is too much ſheltered from the wind, which greatly retards the evaporation. In the other, as the water has all the advantage of the wind, and is heated by the ſun, and atmoſphere, to a conſiderable greater degree than the water in ſeas, lakes and rivers, the quantity of evaporation comes out too much. And therefore nothing certain as to the real quantity of evaporation from watery fluids, can be determined by ſuch experiments, however carefully they may be made.

EXPERIMENT II.

To meaſure with more certainty the real quantity of evaporation, I attempted in the next place to examine what it was in fact from the ſurface of a river. This experiment was made in the following manner: I filled one of the veſſels with river water, and placed it as before. The other I fixed in the centre of a circular board of three feet diameter. This inſtrument, by means of a line faſtened to a tree on a ſmall iſland, was placed ſo as to float near the middle of *Menimack* river. To defend the tube againſt the dews and rain, a circular piece of glaſs, fifteen inches diameter, was ſupported by wires fixed to the board, eight inches above the tube; and the whole was ſo balanced by weights as to leave half an inch of the tube above the ſurface of the water. When thus afloat I filled the tube with water, propoſing to let it remain in this ſituation a week, to ſee how much would evaporate in that ſpace of time. After repeated diſappointments by the rain, wind and waves, for three months, I at laſt ſucceeded in trying the experiment from *Auguſt* 26th, to *September* 2d. During that time there was little wind,

ſtill

ſtill water, no rain, nor any thing to diſturb the experiment. The event was, that at the end of the ſeventh day, the tube was exhauſted 1.15 inch. And that no water had got into the tube in that time, I was certain from this circumſtance; all that part of the ſurface of the board which was within half a foot of the tube was dry every morning and evening. In the other tube, the evaporation in the ſame time was 1.50 inch; which gives 35 decimal parts of an inch difference between the real evaporation from the ſurface of the river, and that of the water when ſuſpended in the air, as in the other veſſel. All the evaporations therefore meaſured the latter of theſe ways, ought to be diminiſhed in this proportion, to have the true quantity ſuch as it is in nature.

EXPERIMENT III.

Theſe experiments on watery fluids put me upon enquiring what the evaporation was from the ſurface of the earth. To determine this, *Sept.* 14, two days after there had been any rain, I ſunk one of the veſſels into the earth in a light ſoil, ſo as to take up all the earth contained in a ſpace equal to the contents of the veſſel. Having carefully weighed the veſſel with the earth it contained, I fixed it in the ground in a plain open field, where it was expoſed to the ſun and wind, but defended from the dew and rain, as in the former experiment. At the end of *ſeven days* I took it up, and weighing it again found it had loſt 783 grains, *troy*. The diameter of the veſſel being three inches, its ſurface expreſſed in whole numbers was equal to nine ſquare inches. Dividing the number of grains that evaporated, 783, by the number of ſquare inches contained in the ſurface of the veſſel, 9, we ſhall have 87 grains for the evaporation from one ſquare inch; and this, (aſſuming 254 grains as the weight of a cubic inch of water) will give $\frac{34}{100}$ parts of an inch, as the depth of water that paſſed off by evaporation. In the other veſſel filled with water,

water, and placed as before, the evaporation in the ſame time was exactly one inch. If this experiment may be ſuppoſed to repreſent the operations of nature, the concluſion will be, that the evaporation from the ſurface of the earth, is but little more than *one third* of what the evaporation is from the ſurface of water.

EXPERIMENT IV.

Another thing I had in view was to know what the evaporation was from plants and trees. In order to make an eſtimate of this, *Auguſt* 20, I took up four different ſorts of plants, with as much of the earth adjoining to each as wholly covered their roots. Each plant, with the earth thus about it, being ſix inches ſquare, I put into a wooden box of the ſame form and ſize. The boxes were covered with thin lead, well cemented at the joints, that nothing might evaporate that way; and had two apertures at the top; one, to admit the ſtem of the plant, the other, that the plant might be ſupplied with water, but which was kept ſtopped when not in uſe. Having taken the weight of each, I placed them in the ground that they might have the ſame degree of heat as before; leaving as much of the plant above the ſurface of the earth, as when it was in its natural ſtate. In this ſituation I added known quantities of water, aiming to put in from time to time as much as I thought they would throw off. At the end of *thirty days* I took them up, taking an account of their weight as before, and alſo that of each plant. The reſult is expreſſed in the following particulars:

The ſeveral ſorts of plants.	*Weight of the plants.* Grains.	*Water evaporated in 30 days.* Grains.
Apple tree,	23	1271
Alder tree,	30	2593
Spear mint,	22	5186
Clover,	43	1894

Q In

In this experiment, the evaporation from theſe four very ſmall plants was 10944 grains; amounting to about 43 cubic inches of water, in thirty days. The evaporation in the ſame time from the veſſel ſuſpended in the air, was 4.25 inches in depth: The quantity therefore thrown off by the plants, was more than what the evaporation would have been from a watery ſurface, of ten inches ſquare. If this way of reaſoning may be applied to fields covered with trees, graſs, and other vegetables, the inference will be, that the evaporation for ſeveral months is greater from them, than it is from equal areas of the ſurface of water.

METEOROLOGICAL OBSERVATIONS made at BRADFORD *in* 1772.

THE inſtruments uſed in the following obſervations, the times at which they were taken, and the method in which they are ſet down, were deſcribed in the paper ſent to the Society laſt year. With regard to thoſe of the preſent year the following things are to be obſerved: The *barometrical* obſervations till Nov. 6, muſt be viewed as imperfect, being taken with a barometer of too ſmall a bore. From the 6th of November to the end of the year, they are very exact; being taken by a very good barometer made by *Nairne.* In meaſuring the quantity of evaporation, I uſed a tube three inches diameter and ſix deep; which was filled once a week. The rain was meaſured by a tube of the ſame form and ſize. In all other reſpects, the ſame method was obſerved as in the meteorological obſervations of 1771.

The obſervations taken by the *barometer* and *thermometer* are ſet down in three columns; the firſt column contains the obſervations taken uſually about 6^h A. M. the ſecond at noon, and the third at 9^h P. M. The other columns give the general ſtate of the winds and weather of the day.

JANU-

JANUARY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	30,0½	30,1	30,1	18½	25	26½	W. to S. W. little.	Fair day; ſnow in the night.
2	30,1½	30,1½	30,1	27	32	30	W. little.	Fine pleaſant weather.
3	29,7½	29,5	29,4½	18	23	33	S.	Cloudy day; rain in the night.
4	29,4	29,6	29,7½	37½	40	41	S. W.	Fine pleaſant weather.
5	29,7¾	29,7½	29,8	26	24	18	N. W. ſtrong.	Clear bright day.
6	29,9½	29,9½	30,0½	13	15½	20	N. W.	Ditto. *Aurora Borealis* in the evening.
7	30,1	30,1	30,1	15½	18½	22½	W. to S.	Cloudy, dull weather.
8	30,1	30,1	30,1½	23	30	29½	W. little.	Fine pleaſant day.
9	30,1½	30,1	30,1	27	31½	34	Ditto.	Ditto.
10	30,1½	30,1	30,1¼	28½	30	26½	Ditto.	Ditto.
11	30,1¼	30,1	30,0½	21	25	26	Ditto.	Ditto.
12	29,8	29,6½	29,4¼	26	27	32	E. freſh.	Cloudy day; ſnow in the night.
13	29,6	29,6	29,6	35	40½	34½	S. W. to W.	Flying clouds with bright intervals.
14	29,7½	29,7½	29,8	28	31½	30	W. little.	Mild pleaſant day.
15	29,8	29,7¼	29,6	23	25	25	Ditto.	Ditto.
16	29,2	29,2	29,0½	23	27	28	S. E. to W.	Snow A. M. clear P. M.
17	29,4½	29,5	29,5	21	21	20	W.	Clear bright day.
18	29,3¼	29,2	29,2	10	14	18	N.	Cloudy, with ſome ſnow.
19	29,1½	29,1	29,1	18	21	27	N. to S. W.	Cloudy dull weather.
20	29,0	28,8½	28,6½	27½	26½	20	N. to N. W.	Storm of ſnow, all day; bright evening.
21	28,9	29,1½	29,2	11½	20	19	N. W.	Fair weather.
22	29,2	29,1½	29,1	18	29½	28½	S. E.	Cloudy dull day.
23	29,0	29,0	29,0	28½	34	36½	S. little.	Cloudy, with bright intervals.
24	29,0½	29,0¼	29,0½	37	39½	40	Ditto.	Dull miſling day.
25	29,3¼	29,3	29,3	38	35	30	W. little.	Fine pleaſant day.
26	29,6	29,6	29,6¼	23	26	21½	N. W.	Ditto.
27	29,7	29,5½	29,5	9	12	10½	N.	Cloudy dull weather.
28	29,2	29,2	29,1½	7	11	17	Ditto.	Ditto, with rain.
29	29,3	29,3	29,3½	16½	23	26	W. little.	Fair and clear.
30	29,0	28,7½	28,6	20	32	32	N. E. freſh.	Cloudy day; ſtorm of ſnow in the night.
31	28,7	28,9	28,9	26	37	34	N. E. to S.	Cloudy dull weather.

Quantity of rain in January, 2,00 } Inches.
Quantity of water evaporated, 1,55 }

FEBRU-

FEBRUARY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,0½	29,1	29,1½	31	37	35	S.	Fair A. M; cloudy P. M.
2	29,3½	29,4	29,6⅓	29	35	32	W. little.	Fine bright day.
3	29,2	29,1¼	29,0	20	23	26½	E. fresh.	Clo. day; hea. stor. of sn. & hail in the nig.
4	28,9	29,2½	29,4½	26	32	31½	S. to N. W. fresh.	Dull cloudy day.
5	29,7½	29,7½	29,9¼	21½	23½	21½	N. W. fresh.	Flying clouds, with bright intervals.
6	30,1	30,1¼	30,1¼	14½	20½	14	N. W. little.	Fine bright day. *Aur. Bor.* in the night.
7	30,0⅙	29,9⅔	30,0¼	23	31	32½	S. W. to W.	Fine pleasant weather.
8	30,2¼	30,2½	30,0	23	25	31½	N. to E. S. E.	Cloudy dull day.
9	29,4¼	29,1	29,2⅔	37	40	38	S. to W. fresh.	Rainy A. M; cloudy P. M. bright even.
10	29,7½	29,9	30,0	22	25½	26	N. W. fresh.	Fair and clear.
11	30,0	29,6½	29,7	25	30	34½	S. W. to W. fresh.	Ditto.
12	30,2⅔	30,2	30,3¼	20	19½	14	N. W.	Fair day.
13	30,4	30,4	30,4	3	11	11	N. W. to N. E.	Fair day; cloudy evening.
14	30,2	29,9	29,9½	11½	12½	15	N. to W.	Snow A. M; clear P. M.
15	30,1½	30,1	29,2	7½	14	32	W. to E.	Clo. A. M; sn. with ra. P. M. & in the nig.
16	29,2¾	29,1	29,1¼	27	32	29½	S. W. little.	Fair pleasant day.
17	29,6	29,6	29,6	16½	26	24	W. to S. W. fresh.	Flying clouds, with bright intervals.
18	29,6	29,6	29,6	25	34	39	S. W. little.	Fine pleasant day.
19	29,6½	29,6½	29,6½	36½	39	41½	S. W.	Ditto.
20	29,6½	29,6½	29,6½	40½	44½	49½	Ditto.	Cloudy day; rain in the night.
21	29,2¾	29,4	29,6	49½	51	48	W. S. W.	Fine pleasant day.
22	29,5⅔	29,2½	29,5	44	47½	49	Ditto. fresh.	Ditto.
23	29,6	29,6	29,8	42	43	42½	W.	Ditto.
24	29,8½	29,8½	29,8½	38	40	39½	S. W. to E.	Ditto.
25	29,3½	29,0	28,4½	35	34	35	N. E. to S. strong.	Heavy storm of snow and rain all day.
26	28,6	28,6½	29,1	31	32	28½	W. to N. strong.	Flying clouds, with bright intervals.
27	29,3½	29,3½	29,3½	25	33	35	W.	Fine pleasant day. *Aur. Bor.* in the nig.
28	29,3½	29,4	29,2½	31	37	40	S. W. little.	Ditto.
29	29,2	29,2	29,3½	37	39	32	Ditto. fresh.	Ditto.

Quantity of rain in February, 2,40 } Inches.
Quantity of water evaporated, 1,65 }

MARCH,

MARCH, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,0½	29,5	29,4½	28	31	30	N. W. to S.	Fair weather.
2	29,0	29,3½	29,4	29	36½	31	S. W. little.	Fine bright day.
3	29,0	29,9	29,9½	27	31	33	N. E.	Cloudy dull weather.
4	30,0½	30,0	30,0	30	34	32	N. to W. little.	Fine pleaſant day.
5	29,8	29,4	28,8	27½	33	33	N. E. ſtrong.	Heavy ſtorm of ſnow.
6	28,8	29,1	29,4	31	34	27	W. freſh.	Clear pleaſant weather.
7	29,8	29,8	29,8	16	25	25	W. little.	Ditto.
8	29,9	29,9	29,9½	23	37	34	Ditto.	Ditto.
9	30,2	30,1½	30,0½	26	23½	20½	N. freſh.	Cloudy with ſome ſnow.
10	30,0	30,0	30,0	17½	31	25	E. little.	Fine pleaſant day.
11	29,7½	29,5	28,8	17	27½	31	N. N. E.	Clo. A. M; heavy ſtorm of ſnow P. M.
12	28,7	29,2½	29,8	30	32	25	W. freſh.	Cloudy morning; clear bright day.
13	29,8	29,8	29,8	16	28	26	N.	Cloudy with ſome ſnow.
14	29,8	29,8	30,0½	22	27	20	W.	Clear bright day.
15	30,2	30,2	30,2	12	23	20	Ditto.	Ditto.
16	30,2	30,1	29,6½	10½	25	29½	W. to S. freſh.	Dull cloudy weather.
17	29,6	29,7	29,8½	28	31	28	W. to S. freſh.	Clear bright day.
18	30,0	30,0	29,6½	20	35	36½	W. to S.	Clear A. M; cloudy P. M.
19	29,6	29,4	29,4½	33	49½	39½	S. W. to S. E.	Ditto Ditto.
20	29,4½	29 2½	29,1	34	32½	31½	N. E. to N.	Clo. mor. ſtor. of ſn. P. M. & in the even.
21	28,9	28,9	29,0¾	33	38	38	W. S. W. freſh.	Cloudy weather.
22	29,3	29,3½	29,5½	37	40	42	Ditto.	Flying clouds, with bright intervals.
23	29,6	29,6½	29,6	40½	47	46	S. little.	Fine pleaſant day.
24	29,6	29,6	29,6	39	45	42	S. to S. E.	Cloudy weather.
25	29,6	29,6	29,6	39	46½	42½	S. to E. little.	Fine pleaſant day.
26	29,6	29,6	29,6	36	39	39½	N. to S. E. little.	Dull miſling weather.
27	29,6	29,6	29,6	38	47½	42	S. to E. little.	Fine pleaſant weather.
28	29,7½	29,7½	29,7½	36	40	37½	E. to N. E. freſh.	Cloudy day; rainy evening and night.
29	29,3	29,2½	29,2	36	39	36	E. freſh	Heavy ſtorm of ſnow & rain, all day.
30	29,3	29,4	29,5½	36	36½	35½	N. N. E.	Rainy weather.
31	29,8	29,9	29,8½	36	44	30½	N. to E. little.	Fine pleaſant day.

Quantity of rain in March, 4,95 } Inches.
Quantity of water evaporated, 1,45 }

APRIL, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,8	29,8	29,8	35	46½	47	S. little.	Fine pleafant weather.
2	29,7¾	29,7½	29,6	37	38½	35	N. E. frefh.	Cloudy dull day.
3	29,3½	29,3½	29,4	32½	35	36	N. ftrong.	Ditto, with fome fnow.
4	29,6	29,6	29,7	33	39½	39½	N. to W. little.	Cloudy A. M.—fair and pleafant P. M.
5	29,8	29,8	29,8	33½	44	45	W. to S. W. little.	Fine pleafant weather.
6	29,5	29,4	29,4	41½	50½	45	E. little.	Ditto.
7	29,5½	29,5½	29,5½	39	54	48	S. E. little.	Ditto.
8	29,5½	29,4	29,4	41½	50	48	E.	Clear A. M. clou. P. M. and in the night.
9	29,2¼	29,0	29,1	42	39½	38	N.E. to N. ftrong.	Storm of rain all day.
10	29,2¼	29,2½	29,2½	40	50	51	N. W. to S. W.	Fine pleafant day.
11	29,2½	29,2½	29,1½	46	57	54½	S. W. to S. little.	Fair day; clo. evening; fmall fhowers in the night.
12	29,0	29,0	29,1½	47	48	46	S. W. to W. frefh.	Fair weather. *Aur. Bor.* in the night.
13	29,2	29,2	29,2	41	46	41½	Variable.	Cloudy dull day.
14	29,2	29,2	29,3½	40	48½	47½	S. W. frefh.	Flying clouds, with bright intervals.
15	29,6	29,6	29,8½	41½	49	48	W. by S.	Fine bright day; rain in the night.
16	29,8	29,5	29,5½	44	47	53	S. to S. E.	Rainy all day.
17	29,4	29,5½	29,8	48½	52	50½	S. E. to W. little.	Fair pleafant weather.
18	29,9½	30,0	30,0½	43	53	49	W. little.	Ditto.
19	30,1	30,1	30,0½	42	48	44	N. E.	Fair A. M. cloudy P. M.
20	29,8	29,1½	29,2	36	38	39½	NE to N W ftrong.	Storm of rain 'till 4 P. M. clou. evening.
21	29,4½	29,5	29,6¾	38½	44	47	N. W. frefh.	Fair A. M. fhowers P. M. & in the even.
22	29,7	29,7	29,4½	43½	53½	55	S. W.	Fine pleafant day.
23	29,3	29,1	29,1¼	51	58	53	S. to W. frefh.	Fair A. M. fhowers P. M. and in the even.
24	29,2	29,2	29,4	45	49	47	Variable.	Dull cloudy day.
25	29,5	29,5	29,6	41	48½	45½	N. E. to W.	Ditto.
26	29,6½	29,6	29,6	39½	55	53	W. to S. S. E.	Fine pleafant day.
27	29,7½	29,7½	29,5⅓	50	64	64½	S. little.	Ditto.
28	29,6	29,6	29,6	57½	59½	62	Ditto.	Ditto.
29	29,8	29,8½	29,8	54	60	58½	E. frefh.	Ditto.
30	29,8	29,6	29,5¼	53	65½	69	S. W.	Ditto.

Quantity of rain in April, 2,70 } Inches.
Quantity of water evaporated, 3,45 }

MAY,

MAY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,5½	29,3	64	75½	77½	S. W.	Fair weather.
2	29,3	29,3	29,3	71	77	75	Ditto frefh.	Do. Lightn. in the night, with thund.
3	29,8	29,9	30,0	58½	55	54	N. W.	Cloudy, with bright intervals.
4	30,2	30,2	30,1⅔	42½	57	59	E.	Clear A.M. clou. P.M. & in the evening.
5	30,1½	30,1½	29,9½	46	48	51	S. E.	Dull cloudy weather.
6	29,9½	29,8½	29,9½	49	55	50½	Variable.	Fine pleafant day.
7	30,0	30,0	30,0	47½	58½	55	E.	Ditto
8	30,0	29,9	29,5	49	51	52	N. E. to N. N. W.	Cloudy A.M. rainy P.M. & in the night.
9	29,1	29,1	29,2	51	57	55	S. W. frefh.	Fair A. M. fmall fhowers P. M.
10	29,4½	29,4½	29,7	49	52½	49½	N. W. frefh.	Fair and pleafant.
11	29,8⅔	29,8⅔	29,8½	43	52½	53	W.	Ditto.
12	29,8	29,7	29,6½	50	59½	62	Variable.	Cloudy A. M. rainy P. M.
13	29,7½	29,7½	29,8	52	58	56	W.	Clear and pleafant.
14	29,8½	29,8	29,7⅓	49	60½	56	Ditto.	Ditto.
15	29,6⅔	29,5⅔	29,4	52½	63	63	S. W. little.	Ditto.
16	29,3½	29,3½	29,3½	52½	67	64	Ditto.	Ditto.
17	29,3	29,3	29,3	52	60	56	Variable.	Ditto.
18	29,5	29,5	29,4¾	54	59	53½	E.	Cloudy A. M. rainy P. M.
19	29,4⅔	29,5	29,5	53	60	57	N. E.	Fair day; cloudy evening.
20	29,3½	29,3½	29,4½	54	59½	57	S. W. to N. W.	Small fhowers A. M. cloudy P. M.
21	29,5	29,6½	29,6½	49½	59½	59½	N. W.	Fine pleafant day.
22	29,5	29,5	29,6	56	70½	65	S. W. frefh.	Ditto.
23	29,7	29,5	29,4	57	72	71	Ditto.	Ditto.
24	29,3½	29,2	29,3½	65	77½	64	S. W. to N. E.	Fair till 3 P. M. cloudy evening & night.
25	29,3½	29,5	29,8	53	57	56	N. to N. E.	Dull cloudy day.
26							Ditto.	Ditto. } Small fhowers.
27							Ditto.	Ditto.
28							N. E. to S. W.	Ditto.
29	29,7	29,7	29,7½	55	59	56½	S. W. to N.	Ditto.
30	29,7	29,6½	29,6½	55	61	66	N. W. to S.	Fair and pleafant.
31	29,5½	29,4	29,3	63	70	75	S. W. frefh.	Ditto.

Quantity of rain in May, 1,00 } Inches.
Quantity of water evaporated, 6,35 }

JUNE,

JUNE, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,4	29,4	29,4	65	77	70½	S. W. fresh.	Fair day; thun. & lighten. in the night.
2	29,5	29,5½	29,6½	64	60	60	N. E.	Dull cloudy weather.
3	29,7½	29,8	29,9¼	55	59½	54	Ditto.	Fine pleasant day.
4	30,0	30,1	30,0½	51	64	61	N. W.	Ditto.
5	30,1	30,1	30,0⅔	55½	73	67	W. to S. S. W.	Ditto.
6	30,0½	29,7¾	29,6½	61	78	72½	S. W.	Ditto.
7	29,6½	29,5	29,6	63½	72½	65	S. W. to N. N. E.	Ditto.
8	29,5⅓	29,5½	29,6	62	71½	64	S. S. W.	Ditto.
9	29,8¼	29,8	29,8½	55½	62	54	W.	Ditto.
10	29,8½	29,8½	29,8½	55	62	57	W. to N. E.	Fair A. M; cloudy P. M.
11	29,7⅓	29,7	29,6	55	58	58	N. E. to S. S. E.	Cloudy day; showers in the night.
12	29,3	29,4½	29,6	57	64	62	W. fresh.	Fine bright day.
13	29,6	29,6½	29,6½	54½	67	67	Ditto.	Ditto.
14	29,6	29,6	29,6	65	77	77	S. W.	Ditto. lightening in the evening.
15	29,6	29,5	29,4	73	83	75½	S. W. fresh.	Ditto.
16	29,3½	29,3½	29,5	69½	73	69	S. W. to W. fresh.	Cloudy A. M; rainy P. M.
17	29,5	29,5	29,5	62	66	64	N.	Cloudy dull day.
18	29,5	29,4½	29,5	60	61½	60½	Ditto.	Rainy day.
19	29,4⅔	29,4	29,4	58½	63½	60½	Ditto.	Ditto.
20	29,3½	29,4	29,3½	57	62	64	Ditto.	Ditto. Fair evening.
21	29,4	29,3½	29,4½	59	67½	60	N. to E.	Cloudy dull day.
22	29,4½	29,4½	29,3½	56	56½	54	N. E.	Cloudy, with showers.
23	29,3½	29,4	29,4	53	61	65	N. to W.	Flying clouds with bright intervals.
24	29,4	29,4	29,4	64	70	69	Ditto.	Ditto.
25	29,4½	29,4⅔	29,4	66½	74	71	Ditto.	Ditto.
26	29,3⅔	29,3⅔	29,3⅔	62½	61½	59	N. W. to E.	Cloudy, with rain.
27	29,4	29,4½	29,6	63	71	67	N. W. fresh.	Flying clouds, with bright intervals.
28	29,7½	29,8	29,8½	70	76	73	W.	Fine pleasant weather.
29	29,8½	29,8	29,7	66½	82	76	S. W.	Ditto.
30	29,8	29,7	29,6	70	83½	79	Ditto.	Ditto.

Quantity of rain in June, 1,25 } Inches.
Quantity of water evaporated, 5,45 }

JULY,

JULY, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,4	29,4	29,4	74	78	77½	S. W.	Small ſhowers A. M. fair & pleaſ. P. M.
2	29,4	29,4	29,4	71	77	75	Ditto.	Cloudy, with ſmall ſhowers.
3	29,4½	29,4½	29,3	74	77	73	N. to E.	Ditto, lightening in the evening.
4	29,3½	29,3½	29,2½	71	78½	79	S. W. to W.	Ditto.
5	29,6	29,6½	29,6	73½	80½	75	W.	Fine pleaſ. day. *Aur. Bor.* in the evening.
6	29,6	29,6	29,6	71½	81	73	S. W.	Fair A. M; cloudy P. M.
7	29,8	29,7½	29,5½	72	79	80	S. W. ſtrong.	Fair till 2 P. M. clou. with th. & ra. nig.
8	29,5¼	29,2	29,4½	77	86	76	S. W. to N. W.	Fair day. *Aur. Bor.* in the night.
9	29,6	29,6	29,6	77	81	72	W.	Fine pleaſant day.
10	29,6	29,6	30,0	66	77	67	Ditto.	Fair A. M. ſhower of rain and hail P. M.
11	30,1	30,1	30,1	62	74	68½	Ditto.	Fine pleaſant day.
12	30,0½	30,0	30,0	78	78	74	Ditto.	Ditto.
13	29,9	29,9½	29,9½	70	75	73	W. to S. little.	Ditto.
14	29,9	29,9	29,9¼	70	78	77½	S. W.	Ditto.
15	29,6	29,6½	29,6½	73	77½	73	Ditto.	Fair A. M. a ſmall ſhower P. M.
16	29,6	29,6½	29,6	71	80	76	Ditto.	Cloudy, dull weather.
17	29,6½	29,6½	29,6	74	78	70	Ditto.	Cloudy, with bright intervals.
18	29,8	29,9	29,9	65	77½	72	Ditto.	Fine pleaſant weather.
19	29,9½	30,0	29,9½	72	81	71	Ditto.	Fair A. M. cloudy P. M.
20	30,1½	30,1½	30,1	72	79	70	Ditto.	Fine pleaſant day.
21	30,1	30,0½	29,9	68	75	70	Ditto.	Fair A.M. cloudy P.M. rain in the night.
22	30,0½	29,7	29,7	66	73	70	Ditto.	Cloudy A. M. clear P. M.
23	29 5	29,5	29,6	65	75	71	N. E. to W.	Ditto, ditto.
24	29,6	29,6	29,6½	68	79	72	Variable.	Cloudy, with thunder and rain.
25	29,8½	29,9	30,0	73	82½	76	W. to N.	Pleaſant day. Rem. *Aur. Bor.* in the nig.
26	30,0½	30,0	29,7	74	84½	80½	S. W.	Fair pleaſant weather.
27	29,8½	29,8	29,6½	76	86½	79	S. to S. W.	Ditto. Lightening in the evening.
28	29,8	29,6	29,4	78	95¾	87	Ditto.	Ditto.
29	29,5	29,3	29,3	81½	*96½	81	Ditto.	Ditto.
30	29,4	29,3	29,3	78	85	78	Ditto.	Fair A. M. thunder and rain P. M.
31	29,8	28,9	28,7	72½	72	71½	Variable.	Dull rainy day.

Quantity of rain in July, 2,95 }
Quantity of water evaporated, 4,60 } Inches.

* This obſervation was taken at 1½ P. M. The thermometer being hung in a ſhade in the open air.

AUGUST, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,5	29,5	29,6	65	70	64½	N. W.	Flying clouds, with bright intervals.
2	29,9½	30,0½	30,1	66	73½	69½	W.	Fine pleafant weather.
3	30,3¼	30,3½	30,3	65	79	69	Ditto, little.	Ditto.
4	30,2	30,2	30,2	64	78	71½	S.	Ditto.
5	30,1¼	29,6	29,2½	67	70	70	N.	Heavy fhowers; clears up in the night.
6	29,7	29,7	29,6½	71	82	77	S. W.	Fair weather.
7	29,7	29,7	29,7	75	83	77½	Ditto.	Ditto.
8	29,7	29,5	29,3	76	86	80	Ditto.	Fair day; terr. thun. & lightg. in the nig.
9	29,4	29,4	29,5	78	75	70	Variable.	Cloudy A. M; fair P. M.
10	29,6	29,6	29,6	65	67	66	N. E. little.	Cloudy day; fmall fhowers in the even.
11	29,5	29,2	28,6½	67	81	75	Variable.	Unfettled weather.
12	28,9	28,9	28,9½	62	79	74	S. W.	Fair and pleafant.
13	29,2	29,2	29,4½	70	80	80	Ditto.	Ditto.
14	29,5	29,5	29,5	74	80½	76	Ditto.	Ditto.
15	29,5	29,4	29,2½	75	84½	78½	S.	Fair A. M. fhowers P. M.
16	29,4½	29 4	29,5	70	72	68	N. E.	Dull cloudy day.
17	29,5	29,5	29,5½	65	66	63½	Ditto.	Cloudy day; rain in the night.
18	29,5	29,4	29,6½	60½	60½	59	Ditto.	Dull rainy weather.
19	29,6	29,6	29,7½	59½	68	64½	S. to N. E.	Unfettled weather.
20	29,8	29,5½	29,6¾	62½	63½	65½	Variable.	Ditto.
21	29,9	29,9½	30,0	63	70	67½	W.	Fine pleafant weather.
22	29,9	29,8	29,6⅔	62½	76	71	S. W.	Ditto.
23	29,5	29,3½	29,2	67	80	74	Ditto.	Fair A. M. fmall fhowers P. M.
24	29,8½	29,9½	30,0½	66	67½	62½	N. E. to E. little.	Fine pleafant day.
25	29,9¾	29,9	28,4½	58	69	67	E.	Cloudy day; heavy fhowers in the night.
26	28,3½	28,2⅔	28,6¼	65	72	75½	S. ftrong.	Flying clouds, with bright intervals.
27	29,2½	29,2	29,2½	72	78	75	Ditto, little.	Fine pleafant day.
28	29,6½	29,7	28,8¼	73	78½	71	W.	Ditto.
29	29,9½	30,0	30,0½	62	72	69	Ditto.	Ditto.
30	30,3	30,3	30,2½	62	74	67½	Ditto.	Ditto.
31	30,1	29,9	29,6½	64	76	76	S. W. little.	Ditto.

Quantity of rain in Auguft, 3,15 }
Quantity of water evaporated, 4,35 } Inches.

SEP-

SEPTEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,6½	29,7	71	75	73	E. little.	Fair pleafant weather.
2	29,7	29,9	29,8½	67	69	67	N. to N. E.	Cloudy day ; ftorm of rain in the night.
3	29,2½	29,2	29,2	67½	68	70	N. frefh.	Rainy till 4 P. M. clears up in the night.
4	29,7½	29,8	30,0½	68½	81½	75	W.	Fine pleafant day.
5	30,1	30,1	30,1	67	70	62½	Ditto.	Ditto.
6	30,1	30,1	30,1	53	61	62½	Ditto.	Ditto.
7	30,1	30,0	29,8½	57	67½	65½	W. to S.	Fair A. M. cloudy P. M.
8	29,8	29,8	29,8	62	64	67	S. to W.	Cloudy day ; fair evening.
9	29,6	29,6	29,7¾	65	73	75	S.	Cloudy dull weather.
10	29,4	29,3½	29,4	58½	57½	55	N.	Rainy day ; fair evening.
11	29,2	29,1½	29,1	52	56	53	N. ftrong.	Storm of rain all day.
12	29,1	29,1½	29,2	50	51	52	N. to W.	Rainy A. M. cloudy P. M. fair evening.
13	29,8½	29,9	30,0	51	58	59	W. little.	Fine pleafant day.
14	30,0	30,0	30,0½	52½	68	63	W. to S. W. little.	Ditto.
15	30,0	29,9	29,9	58	60½	56	W.	Ditto.
16	29,9	29,8	29,8	51	57	55	W. to N.	Fair A. M. cloudy P. M. rain in the night.
17	29,4½	29,3	29,3½	55	57	61	S. to W.	Rainy day ; fair evening.
18	29,5	29,5	29,7½	58	65	62	W. little.	Fair day.
19	29,8	29,8	29,8½	54	66½	65½	Ditto.	Ditto.
20	29,8	29,7⅔	29,7	59	64	66½	S. little.	Ditto.
21	29,8	29,8	29,8	62½	63	63	Ditto.	Ditto.
22	29,8	29,8½	29,9	55	62½	59	S. to E.	Ditto.
23	29,4½	29,4	29,4	62	64½	63	S.	Rainy day.
24	29,6⅔	29,6⅔	29,6⅔	59½	58	57	N.	Cloudy dull weather.
25	29,8	29,8	29,8	57	61	55	Ditto.	Flying clouds, with bright intervals.
26	29,8	29,8½	29,9	49	52	55	Ditto.	Fair weather.
27	29,9	29,9½	29,9½	52	59	62	N. to E.	Ditto.
28	29,9	29,8	29,8	59	65	65	S. to S. W.	Fine bright day.
29	29,7	29,6½	29,7	59	60	57	E.	Dull mifling day.
30	29,7	29,7	29,7	57	59	56	Ditto.	Ditto.

Quantity of rain in September, 2,95 } Inches.
Quantity of water evaporated, 4,50 }

OCTO-

OCTOBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,7	29,7	29,7⅓	56	65	58½	N.	Dull cloudy day.
2	29,7⅓	29,6½	29,6½	56½	64	63	W.	Fine plea. day. *Aur. Bor.* in the night.
3	29,8½	29,8½	29,9	52	55	51	Ditto.	Ditto.
4	29,9½	30,0	30,0	43	54	53	Ditto.	Ditto.
5	30,0	29,9½	29,9	47½	56	57	S. little.	Fair weather.
6	29,9¼	29,9	29,8½	53	58	59	N. E.	Dull cloudy day.
7	29,7½	29,6½	29,5½	57½	56½	55	N.	Storm of rain all day.
8	28,8	28,6	28,5½	53	54	54½	Ditto.	Ditto.
9	28,6	28,8	28,9½	54	56	57	Ditto.	Dull mifling day.
10	29,2	29,2	29,6½	54½	57	57½	Ditto.	Ditto.
11	29,6¼	29,6¼	29,7½	54	57	60	W. little.	Fair pleafant weather,
12	29,8½	29,8½	29,9½	54	62	60	S. W. little.	Ditto.
13	30,0	30,0	30,0	53½	57	53	W. N. W.	Ditto.
14	30,0½	30,0	30,1	46	55	55	Ditto.	Ditto.
15	30,0½	30,0	30,1	50	55½	59	Ditto.	Ditto.
16	30,2	30,2	30,2½	54	62	59	E. to N. W.	Ditto.
17	30,0½	30,0	29,9	55	62	60	W. to S. E.	Ditto.
18	29,7½	29,7½	29,7½	56½	57½	59	Variable.	Cloudy dull day,
19	29,7	29,6½	29,5	60	65	61½	S. W. little.	Fine pleafant day.
20	29,2	29,2	29,2	57	61	59	N. E. little.	Cloudy A.M. mifling P.M. & in the nig.
21	29,3	29,2½	29,2	56½	56	52	N.	Cloudy A. M; rainy P. M.
22	29,2	29,2	29,2	48½	50	48	W. frefh.	Fair weather.
23	29,4	29,4	29,4	42½	50	49	W.	Fine pleafant day.
24	29,4½	29,4	29,3½	42	54	53½	W. to S. W.	Ditto.
25	29,4	29,3	29,3	53	60½	58½	S. W. frefh.	Cloudy A. M; fair P. M.
26	29,3	29,3	29,5½	52½	50½	57½	S. W.	Fine pleafant day.
27	29,6	29,4½	29,2	50	50	48	N. E. frefh.	Clo. day: fto. of ra. P. M. & in the nig.
28	29,2	29,2	29,2	49	49	50	N. E.	Cloudy with fome rain.
29	29,2½	29,2	29,2½	48½	53	52	S. W.	Dull cloudy weather.
30	29,1	29,1	29,1½	47½	51	52	S. E. frefh.	Cloudy A. M; rain P. M. & in the nig.
31	29,4	29,5½	29,6	50	52½	52	N. W. frefh.	Flying clouds with bright intervals.

Quantity of rain in October, 6,70 } Inches.
Quantity of water evaporated, 3,75 }

NOVEM-

NOVEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,6	29,6	29,7	48	53	53	S. W. little.	Fine pleafant day.
2	29,7½	29,7	29,6½	47½	55	53	S. E.	Fair A. M; clo. P. M. fho. in the nig.
3	29,7	29,7	29,7	50	51	49	N. W. frefh.	Fair and pleafant.
4	29,6	29,6	29,5½	42	52	46	S.	Ditto.
5	29,6	29,6	29,6	42	42	36	N. W. frefh.	Ditto.
6	29,9	29,9½	30,0½	35	39	41½	Ditto.	Ditto.
7	30,0½	30,0⅔	30,0⅔	37½	46	45	S. little.	Ditto.
8	30,1½	30,3	30,3½	40	48	47	W. little.	Ditto.
9	30,4⅔	30,5	30,4¾	40	47	53	S. E. little.	Ditto.
10	30,2⅓	30,0	29,8	45	49	53	S. frefh.	Cloudy dull weather.
11	29,8½	29,8⅔	30,0¼	47	48	46	N. W. frefh.	Fair weather.
12	30,0⅓	30,0⅓	30,0	38	42	42	Ditto.	Fine pleafant day.
13	29,9	29,6⅓	29,1	35	38	49	S. E. to E. frefh.	Rainy day.
14	29,5⅔	29,6½	29,8	41½	43	41	N. W.	Fair pleafant weather.
15	29,7½	30,0	30,1½	36½	39	38	S. W. to W.	Ditto.
16	30,3	30,4	30,5	31	35	32	W.	Ditto.
17	30,5⅓	30,5⅔	30,5	27	32	32	Ditto.	Ditto.
18	30,3½	30,2⅔	30,3¼	28	34	37½	S. W. to W.	Ditto.
19	30,4	30,4	30,4	33½	40	39	Ditto.	Ditto.
20	30,3¾	30,3½	30,3½	33	43	40	W.	Ditto.
21	30,4⅔	30,4⅔	30,4⅔	37	40	43	N.	Dull cloudy day.
22	30,3⅔	30,3½	30,3	42	43	45	Variable.	Ditto.
23	30,3	30,4	30,4	42	44	42	W.	Fair pleafant weather.
24	30,4	30,4	30,3	37	38	40½	Variable.	Cloudy dull day.
25	30,2½	30,0	29,9	43	49	48½	S. E.	Dull rainy weather.
26	29,8½	29,7½	29,6	48	51	46	S. E. to E.	Ditto.
27	29,4⅓	29,4⅔	29,6⅓	43	45	44	N. E. to N. W.	Cloudy day; fair evening.
28	29,7½	29,8½	30,0	38	41	41	W. little.	Fine pleafant day.
29	30,0½	30,0	29,7½	42	43	50	E. frefh	Cloudy with rain; ftormy night.
30	29,7	29,6	29,6	50	50½	48	Variable.	Cloudy day; fair evening.

Quantity of rain in November, 3,90 } Inches.
Quantity of water evaporated, 2,45 }

T DECEM-

DECEMBER, 1772.

Days.	Barometer.			Thermometer.			Winds.	Weather.
1	29,8	29,8$\frac{2}{3}$	29,8$\frac{1}{2}$	43	45	45	Variable.	Fair day ; rainy evening.
2	29,8	29,8	29,7	47	47$\frac{1}{2}$	47	S. E.	Rainy weather.
3	29,6	29,6	29,7$\frac{1}{4}$	43	43	40$\frac{1}{2}$	N. W. fresh.	Cloudy A. M ; fair P. M.
4	29,8	29,8$\frac{1}{3}$	29,8$\frac{1}{2}$	38	39$\frac{1}{2}$	35	W. little.	Fine pleasant day.
5	29,9	29,9	29,9	34	36	36	Ditto.	Ditto.
6	30,0	30,0	30,1	29$\frac{1}{2}$	34	32	N. W. to N. E.	Cloudy unpleasant weather.
7	30,1	30,0$\frac{1}{2}$	30 0	25	28	26$\frac{1}{2}$	N. W.	Fair day.
8	29,9	29,9	29,9	26	31	30	Ditto.	Flying clouds, with bright intervals.
9	29,9$\frac{1}{2}$	29,9	29,8$\frac{1}{4}$	29	32	32	W. to S.	Fair A. M. clo. with small showers P. M.
10	29,8	29,8	29,9	32	35	33	S. fresh.	Fine pleasant day.
11	29,9	29,9$\frac{1}{2}$	30,0	32	30	25	N. W. fresh.	Fair and clear.
12	30,0$\frac{1}{2}$	30,1$\frac{1}{4}$	30,1$\frac{3}{4}$	25	30	28	N. W. little.	Ditto.
13	29,9	29,7$\frac{1}{2}$	29,7$\frac{1}{4}$	29	33	39	S. fresh.	Cloudy day ; fair evening.
14	29,8$\frac{1}{2}$	29,8$\frac{1}{4}$	30,0$\frac{1}{2}$	36	39$\frac{1}{2}$	39	W. little.	Fine pleas. day. *Aur. Bor.* in the night.
15	30,3	30 3$\frac{2}{3}$	30.4$\frac{1}{4}$	34$\frac{1}{2}$	36	36	E.	Fair A. M. cloudy P. M.
16	30,4	30,2$\frac{1}{4}$	30,0	33$\frac{1}{2}$	36$\frac{1}{2}$	41	N. E. fresh.	Cloudy A. M ; rainy P. M.
17	30,0	30,0$\frac{1}{2}$	30,1	38	40	40	S. W.	Cloudy unsettled weather.
18	30,2	30,2$\frac{1}{2}$	30,2$\frac{1}{2}$	39	38$\frac{1}{2}$	40$\frac{1}{2}$	Ditto.	Ditto.
19	30,1$\frac{2}{3}$	30,2	30,2$\frac{1}{2}$	39	40$\frac{1}{2}$	41$\frac{1}{2}$	Ditto.	Fine pleasant day.
20	30,2$\frac{1}{4}$	30,2	29,9	25$\frac{1}{2}$	38$\frac{1}{2}$	41$\frac{1}{2}$	N. E.	Cloudy A. M ; rainy P. M.
21	29,8$\frac{1}{2}$	29 9	30,0$\frac{1}{2}$	4[illegible]$\frac{1}{2}$	42	41$\frac{1}{2}$	W. little.	Fine pleasant day.
22	30,1	30 0$\frac{1}{2}$	29,[illegible]$\frac{2}{3}$	38	40$\frac{1}{2}$	44	S. little.	Ditto.
23	29,8	29,9	29,9$\frac{1}{2}$	42	44	43	W.	Ditto.
24	30,0	29,9	29,8$\frac{1}{2}$	40	40	38	N.	Cloudy day ; snow in the evening.
25	29,7$\frac{1}{2}$	29,9	30,0$\frac{1}{2}$	36	3[illegible]$\frac{1}{2}$	35	W.	Fine pleasant day.
26	30,1	30,1	29,9$\frac{1}{2}$	28	32	38	S. W. to E.	Cloudy day ; rain in the night.
27	29,5$\frac{1}{2}$	29,4$\frac{1}{2}$	29,2	42	42	41	Variable.	Dull misling day.
28	29,4	29,6	29,7	41	41	39$\frac{1}{2}$	N. W.	Fine pleasant weather.
29	29,8$\frac{1}{2}$	29,8$\frac{2}{3}$	29,6$\frac{1}{2}$	38	43	45	W by S. little.	Ditto.
30	29,6	29,6$\frac{2}{3}$	29,9	43	44	35	N. W.	Ditto.
31	29,9$\frac{2}{3}$	29,9	29,9	33$\frac{1}{2}$	34	35	S.	Cloudy, dull weather.

Quantity of rain in December, 2,15 } Inches.
Quantity of water evaporated, 3,10 }

The

The greateſt height of the barometer this year, was on the 17th of November: The mercury was then at $30.5\frac{2}{5}$ inches. The leaſt height was $28.2\frac{2}{5}$ inches on Auguſt the 26th. The thermometer on the 29th of July, roſe to $96^{\circ}\frac{1}{2}$; on February the 13th, it ſtood at 3°: The former was the greateſt, the latter its leaſt height. The quantity of rain that fell in the year was 36.30 inches. The quantity of evaporation, meaſured by filling the veſſel once a week, came out 42.65 inches.

It is however to be obſerved that different methods of meaſuring the evaporation, will lead to different concluſions. It was becauſe the tube was of too ſmall a diameter, and not filled often enough, that the quantity of evaporation came out ſo ſmall in 1771. The method of making the experiments being altered, the evaporation turns out very different in 1772. If the experiments could be made on the ſurface of a watery fluid the reſult would determine the quantity of evaporation, with much greater certainty and accuracy, than can ever be done by means of a veſſel ſuſpended in the air.

OBSERVATIONS IN 1773.

The quantity of rain which fell in

January, was	1,95
February,	0,95
March,	1,75
April,	1,90
May,	2,10
June,	1,70
July,	1,00
Auguſt,	4,15
September,	1,05
October,	4,10
November,	1,90
December,	4,00

To

To meaſure the quantity of *rain*, I fixed a tube about three inches diameter, in ſuch a manner as to receive the rain as it fell; which I meaſured as ſoon as it was over, and added up the whole of each month together. In this account the *ſnow* and *hail* are included: Theſe were meaſured by taking up in the tube all the ſnow or hail that fell on a ſpace equal to its ſurface, and then melting it. The method of meaſuring the quantity of *evaporation* by a tube ſuſpended in the air being uncertain and inaccurate, thoſe obſervations are omitted.

The greateſt height of the barometer this year, was on the 22d and 23d of February: The mercury at 12^h on each of theſe days, was at 30.6 inches. The leaſt height was $28.6\frac{1}{3}$ inches, on December 26th at 12^h. The thermometer on the 8th of July, at $12^h\frac{3}{4}$, roſe to $96^\circ\frac{1}{2}$: On February the 22d, at $8\frac{1}{4}$ A. M. it ſtood at $9^\circ\frac{1}{2}$ below o. At both theſe times the thermometer was hung in the open air, in a ſhade: The former was the greateſt, the latter its leaſt height. The quantity of rain this year amounted to 26,55 inches.

On the 17th of July there was an *Aurora*, uncommon in this reſpect, that there were ſeveral appearances of it in the *ſouth*: The firſt of theſe was about 9^h. It began about 20° above the horizon and inſtantly ſpread itſelf in a horizontal direction to the diſtance of 30° each way from the meridian. For the ſpace of one quarter of an hour there were five ſuch appearances, all in the ſouth as before; their duration was not more than half a minute, and their colour a pale light, exactly like that of the Aurora in the north.

There was alſo a remarkable HURRICANE this year, the effects of which were principally felt at *Saliſbury*, *Ameſbury*, and *Haverhill*. Theſe towns lie on Merrimack river, on the north ſide; Saliſbury being the place where the river empties itſelf into the ocean.

The

The hurricane came on *Auguſt* 14th, 1773. Its *riſe* was very ſudden, and without any previous uncommon appearance in the ſky, or other ſymptom of its approach. In the morning there was a light breeze of wind at the eaſt, attended with plentiful ſhowers. At $7^h \frac{3}{4}$ the wind veered about to ſouth-eaſt, where it became a briſk gale. In about two minutes, it got into the ſouth-weſt, and became on a ſudden very violent. From thence in about two minutes more, it ſhifted to weſt-north-weſt, and then ſuddenly died away to a moderate breeze. While the wind was thus changing, it ſeemed to blow in every direction; the guſts became very violent, and formed many little whirlwinds all around, attended with a very heavy ſhower of rain, and an uncommon darkneſs.

At the place where *Salisbury* and *Amesbury* join, the *violence* of the hurricane was very great. Its firſt appearance was on Merrimack river. At the time when the wind was veering to the ſouth-eaſt, its waters ſeemed to be inſtantly thrown into a violent agitation; and came rolling from the eaſt as if they would have overflowed the banks. The hurricane immediately ſtruck the ſhore at Saliſbury-Point and Ameſbury, levelling before it ſeveral well built houſes almoſt new, unroofing, twiſting, and wrecking others; and thus tearing down, or ſhattering moſt of the buildings that were in its way. Several buildings were ſhattered to pieces, and others removed in an inſtant. A ſail-maker's loft in which a man was ſitting, was carried away and diſperſed in a moment; the unhappy man being found ſenſeleſs at the diſtance of 94 feet from the place where the loft ſtood. A large oak poſt 14 feet in length and 11 inches in diameter, was taken up and carried by the wind 138 feet. Two new veſſels of 90 tons burthen, were lifted up from the blocks and carried to the diſtance of 22 feet. And a large bundle of ſhingles was taken up from the earth and thrown near 330 feet, in a direction contrary to that of the poſt and veſſels. The trees around were

were torn up, the fences were thrown down, fcattered or carried off, and the various kinds of lumber that lay dif-perfed on the fhore, were whirled about in different direc-tions, and to different diftances. Some houfes and veffels that feemed the moft expofed to the wind, fuffered nothing at all; and others that feemed to be the leaft expofed, were much damaged or carried off. The number of buildings that fuffered was about 120: And though many perfons were carried to fome diftance, and others much hurt, be-ing covered in the ruins or in the cellars of their houfes, no lives were loft. At the place where this deftruction was done, the buildings were pretty thick, amounting to about 150. The general *direction* of the hurricane was from eaft-fouth-eaft to weft-north-weft. Its *extent*, in width, was about a quarter of a mile; in length, about a mile and three quarters: And its *duration*, not more than four minutes from the time it firft began till all became ftill and quiet again.

At the fame time confiderable damage was done at *Ha-verhill*, ten miles higher up the river. There the hurri-cane came on from the fouth-weft. Its firft effect was the deftruction of a large new barn. The barn containing a large quantity of hay and grain, was crufhed down in an inftant. The hay, boards and fhingles were fcattered round to all points of the compafs, to the diftance of four or five rods from the place where the barn ftood; and fome of them were carried to the diftance of three miles north-eaft. A large dwelling houfe at eight or ten rods diftance, was much damaged; every board and rib was torn from the roof, and the chimney wrecked to the foundation. Five barns were almoft wholly deftroyed, and a number of houfes and other buildings were much damaged. In fome, the windows and doors were drove in; in others, the boards and fhingles were ripped off and fcattered in all directions. The ftone walls in fome places were almoft levelled with the ground; and the trees to the number

number of five hundred were swept off in the space of a mile. The general *direction* of the hurricane was different here from what it was in Salisbury, being from south-west to north-east. Its *extent* was about three miles in length, and half a mile in width: Its *duration* not more than four minutes. The *violence* of the hurricane was probably as great at Haverhill, as it was at Salisbury: But as it passed half a mile above or north-west of the centre of the town, where the buildings were not very thick, the damage done by it was much less.

To what *extent* the disturbance in the atmosphere reached, cannot be exactly determined. Though it did not form whirlwinds of such force as to produce any remarkable effects at any other places but those mentioned above, it evidently extended to all the towns on Merrimack river, from the mouth to some miles above Haverhill. In all the adjacent towns, the sudden change and different directions of the wind, with their effects in twisting the trees, corn, &c. were observed to the distance of six or eight miles on each side of the river. And yet there did not seem to be any very great alterations in the *weight* or *temperature* of the atmosphere, at any considerable distance from the place where the winds were so violent. At *Bradford* opposite to Haverhill, and not more than a mile from the place where the damage was done, the *barometer* at 7^h A. M. stood at 29,8' inches. At the time of the hurricane it suddenly fell to 29,6'; and just after to 29,5', which was its least height that day. By noon it rose to 29,7', and at 9^h P. M. it got up to 29,8'; alterations very common in this part of America. *Farenheit's thermometer* in the morning was at 74°; at noon and 9^h P. M. it was one degree lower: The wind continuing very moderate between west and north-west from 8^h A. M. till night.

From these phenomena we may form some probable conjectures as to the *cause* and *origin* of the hurricane. What occasioned such a violent irregular commotion in the

air,

air, was probably the great *rarefaction* of it. The weather, for a week before, had been uncommonly hot, and the wind conftantly at fouth or fouth-weft. The air next to the furface of the earth muft therefore have been greatly heated and rarefied; and probably was become fpecifically lighter than the air in the higher regions. The confequence would be that the lighter rarefied air would afcend, while the heavier condenfed air would defcend; and in this way the *equilibrium* of the air would be deftroyed: To the place where the equilibrium of the air was thus broken, the adjacent air would inftantly flow on every fide as to a common centre; forming eddies and whirlpools, and thus affuming a *circular motion*. And this it is probable gave rife to thofe fudden changes, different directions, and violent gufts of the wind. The place where this circular motion began, feems to have been in the upper region of the air, at fome diftance from the furface of the earth; for feveral of the effects of the hurricane bear the marks of a *defcent*. At the place where it firft ftruck Merrimack river, it hove up the waters, as if fome great force had been impreffed upon its furface: And at Haverhill, where it was firft felt it crufhed a barn to pieces as if fome immenfe weight had fallen upon it. When the defcending air and whirl came to the earth, being ftopped in its defcent it feems to have inftantly fpread itfelf in to a larger circle or compafs, blowing every way from the centre. And hence the pieces of the barm that was crufhed down by it, became fcattered to the diftance of four or five rods all around. A defcent of the air in one place would be immediately fucceeded with an *afcent* of it in another. And thus the whirlwind, where the air was defcending crufhed down the buildings before it: But where the air was afcending, lifted up, unroofed, or carried them away; fhattering and throwing off the materials thus carried up, as they came to the extremity of the whirl, in tangent lines, to different diftances and in all directions. Befides this circular, the whirlwinds

winds had alſo a *progreſſive* motion. Had the different winds by which the whirlwinds were formed been of equal violence, the whirlwinds would have been ſtationary conſiſting only of a circular motion; but being of unequal violence, the whirlwinds had a progreſſive motion, proceeding in different directions at different places, according to the direction of the ſtrongeſt wind.

The *ſummer* preceding this hurricane had been in ſome reſpects different from what is common in this part of the country. There had been an uncommon drought for two months before, which was no where more ſevere than in the towns upon the river; and in no ſummer for ſeveral years, have we had ſo much hot weather. This circumſtance is agreeable to the preſumption of theory; for, if whirlwinds and hurricanes are derived from the great rarefaction of ſome part of the atmoſphere, it might be expected that the times in which they would happen, would be in the moſt calm, or hot weather.

N° X.

A Letter from J. MADISON, *Eſquire, to* D. RITTENHOUSE, *Eſquire.*

William and Mary College, Virginia, November, 1779.

DEAR SIR,

AGREEABLY to promiſe, I now tranſmit you a ſeries of obſervations upon our climate. They comprehend an entire year, and part of the ſucceeding. I thought once of ſending you only a mean of the obſervations for each month, but as it was a part of our natural hiſtory, which has never yet been made public, I have therefore ſent a copy of the journal. Some ſingular circumſtances too attending the barometer I thought deſerved to be particularly noted, which could not have been done had the firſt idea been adopted. For the obſervations upon the barometer not only ſhew us the different ſtates of the atmoſphere, but, perhaps, may throw farther light upon the true cauſe of the Aurora Borealis. The fact is, that a fall of the barometer always ſucceeds that phenomenon. The frequency of its appearance lately, gave me an opportunity of obſerving this effect at different times. It has for ſome time been ſuppoſed (after Dr. Franklin had firſt given riſe to the opinion) to be an electrical appearance; and I think, the levity of the atmoſphere, as proved by the barometer, adds great weight to that ſuppoſition: ſince it is well known to every electrician, that a rarefaction of the air, in our experiments, will always produce ſimilar appearances. One circumſtance indeed was obſervable, that a change of weather, to wet, generally ſucceeded; but as this effect was not ſo conſtant, it was not much attended to. But the barometer by ſhewing that the atmoſphere is actually lighter, and of conſequence more rarefied at the time of ſuch appearances than at others, evinces at leaſt, that it is in a ſtate the moſt likely to exhibit them; it is to be obſerved alſo, that the greateſt fall of the barometer is not prior to, but always ſucceeds this appearance; ſhewing that the rarefaction firſt begins in the upper parts of the atmoſphere.

It is remarkable that the range of the barometer was not more than one inch and a tenth throughout the whole year, nor do I remember ever to have ſeen a greater difference at any time not included in the journal; whilſt we ſee in other countries, the atmoſphere undergoing changes ſo great as to effect a difference of three or four inches. Whence is it then that we are expoſed to more violent ſtorms of wind and rain? Perhaps indeed the changes here, though not ſo great, may be more ſudden, of which ſome remarkable inſtances may be ſeen in the journal.

Our

Our coldeſt winds, as well as the moſt violent, are the north-weſt. The ſouth and ſouth-weſt winds are the hotteſt, though the ſenſations of heat to which we are expoſed, do not correſpond to the different degrees marked by the thermometer, as they depend much upon a current of air with which we are generally favoured about the hotteſt time of the day, and copious ſweating. I do not recollect ever to have ſeen the thermometer here at more than 95, though Dr. Franklin mentions that in June 1750, it ſtood at 100 in the ſhade at Philadelphia, when he obſerves, "I expected that the natural heat of the body, 96, added "to the heat of the air, 100, ſhould jointly have created "or produced a much greater degree of heat in the body; "but the fact was, that my body never grew ſo hot as the "air that ſurrounded it, or the inanimate bodies immerſ-"ed in the ſame. For I remember well, that the deſk, "when I laid my arm upon it, a chair when I ſat down "in it, all felt exceeding warm to me, as if they had been "warmed before the fire. And I ſuppoſe a dead body "would have acquired the temperature of the air, though "a living one, by continual ſweating, and by the eva-"poration of that ſweat was kept cold." I have been the more particular in tranſcribing this paſſage from the works of this philoſopher, as it certainly ſhews to whom the merit of certain late diſcoveries, which have made ſo much noiſe in the philoſophical world, moſt juſtly belongs; I mean, that power which the human as well as all animate bodies have, of counteracting the heat of an atmoſphere in which they are placed. For what do all the experiments upon heated rooms evince, farther than had before been publiſhed by the doctor? It is thus that Franklin ſetting in his chair, like Newton reaſoning upon the figure of the earth, could ſhew what muſt coſt others infinite labour and fatigue. But, though the effect was obſerved and attributed to evaporation, yet I do not remember that it is any where ſhewn in what manner eva-

 poration

poration produces cold. Hamilton, in his excellent eſſay upon the aſcent of vapors, ſpeaking of the natures of ſolution and evaporation, has theſe words, "how cold is "produced in either caſe, I cannot pretend to ſay." The doctor has given the moſt probable explanation of the manner in which it is produced by ſolution, and I think the following, which is collected from his general doctrine may be applied to evaporation. It is admitted that there is a ſtronger attraction between heat and water, or ſuch like fluid, than between heat and any other body, for on this account it is that bodies are cooled when plunged into water. When ever therefore, water for inſtance, is put upon any part of the human body, its natural heat is more attracted in that part by the water, than by the fleſh, and therefore, the water in going off in the form of vapour carries with it part of the heat, and conſequently leaves that place in a negative ſtate, or with leſs than its natural quantity. It is the ſame with the thermometer. Hence it is, that we are much hotter frequently when the thermometer ſcarcely exceeds 82 or 83, there being no current of air to carry off the moiſture from the ſurface of the body, than when it even ſtands as high as 90 or 95.

I am, with the greateſt reſpect,

Your ſervant and friend,

J. MADISON.

JULY,

JULY, 1777.

The Obſervations upon the Thermometer were made at eight, twelve and four o'clock, in the ſummer. In the winter, the laſt at three, the other obſervations at eight o'clock.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obſervations.
3	N by E	Clear	30 1 3		73	$73\frac{1}{2}$	
4	N b E	Clear	30 1 0	$69\frac{1}{2}$	73	74	
5	N b E	Clear	30 0 0	$70\frac{2}{3}$	74	76,5	
6	N E	Rain	29 7 0	74	77		
7	E b N	Cloudy & rain	30 1 0	73	73	73,5	
8	E b N	Cloudy & rain	30 1 5	72	74	76	
9	S b W	Clear	30 0 8	75	78	79	
10	E b N	Clear	30 0 2	78	83	84	
11	S	Clear	30 0 2	79	82	83	
12	S W	Clear	29 8 4	80	83	85	
13	N b E	Rainy	29 7 2	79	80	81	
14	E b S	Cloudy	29 9 0	75	78	76	
15	E b N	Cloudy	29 9 0	75	78	80	
16	S E	Clear	29 9 0	79	82		
21	S W	Clear	29 9 4	77	81	82	
22	S W	Cloudy	29 8 7	79	82	82	
23	N W	Clear	29 9 2	74	75	75	
24	N b W	Clear	30 0 5	74	75	74	
25	N b E	Cloudy	30 0 5	73	73	74	
26	S W	Clear	29 9 5	74	77		
27	N b E	Clear	29 9 9	77	80	80	
28	S W	Clear	29 9 0	80	82	82	
29	W	Clear	29 3 0	80	81	82	
30	N b W	Clear	30 0 0	78	79	80	
31	N E	Clear	30 1 3	77	78	79	

Note, That the height of the mercury in the barometer is determined by a ſcale divided into inches and tenths with a nonius; ſo that 30.1.3 will be read 30 inches, 1 tenth, and 3 tenths of a tenth.

AUGUST,

AUGUST, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S W	Clear	30 1 3	77	80	83	
2	S W	Clear	30 2 4	79	83	84	
3	S W	Clear	30 1 8	83	85	88	
4	S W	Clear	30 0 8	83	85	86	Much lightening in the evening.
5*	S W	Clear	30 0 5	81	86	87	* Rain with lightening. The barometer was
6	S W	Rainy	30 0 5	82	83	82	obferved to rife before the rain to 30 1 0.
7	S W	Cloudy	30 0 9	80	82	80	
8	S W	Clear	30 2 2	80	82		
9	S W	Clear	30 2 2	80	84		
10	S W	Clear	30 1 7			86	It was hotter according to the fenfation of the
11	S W	Clear	30 1 7	83	86	88	human body this evening than had been obferved
12	S W	Clear	30 1 5	83	87	88	this fummer. There was no wind.
13	S W	Clear	30 1 8	84	86½	88	
14	S W	Clear	30 1 3	84	86	89	
15	S W	Clear	30 0 8	84	86	91	
16	S W	Clear	30 1 0	86	88	87	Much rain and lightening. The point was dri-
17	W	Clear	30 1 4	82	84		ven off the Capitol rod, confiderably fufed.
18	W	Clear	30 1 4	83	85		
19	E	Cloudy	30 1 4		82		
20	W	Clear	30 1 4	79	82	83	Frequent rain.
21	W	Clear	30 1 4		82	85	
23	S W	Clear	30 1 4	82	85	86	
2	S W	Clear	30 1 2	85	86	83	Rain with lightening.
25	S W	Clear	30 1 2	83	85	83	
26	S W	Clear	30 0 6	80	84		Rain.
27	N W	Cloudy	30 0 0	82	79	79	
28	N E	Cloudy	30 1 5	76	76	76	
29	N E	Clear	30 2 6	73	75	75	
30	N E	Clear	30 1 1	73	74		
31	S W	Clear	30 2 0	74	76		

SEP-

SEPTEMBER, 1777.

Days.	Winds.	Weather.	Barometer.			Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S E	Clear	30	1	8	73	76	77	
2	N E	Clear	30	1	8	75	78	78	
3	N E	Cloudy	30	3	0	72	73	72	
4	N E	Cloudy	30	3	3	69	71		
5	S E	Clear	30	2	5	73	75	74	
6	S W	Clear	30	1	9	72	78		
7	S	Clear	30	1	0	78	82	80	Rain.
8	N E	Clear	30	1	7	75	76	75	
9	E	Clear	30	3	0	76	76	75	
10	S W	Cloudy	30	1	7	74	79	79	
11	S E	Clear	30	0	5	79	81		
12	S E	Clear	29	9	4	81	82	80	Rain.
13	N E	Clear	30	1	1	76			
14	S E	Clear	30	2	7	73	73	72	
15	N E	Cloudy	30	2	6	72	74	73	
16	E	Rain	30	1	6	72	71	70	
17	N W	Cloudy	29	9	0	69	69	68	Rain.
18	N E	Cloudy	29	9	0	68	67½	67	Showery.
19	E	Clear	30	0	6	69	70	70	
20	S W	Clear	30	0	6	69	77	77	
21	E	Clear	30	0	0	75	79		
22	N E	Clear	30	0	8	67	69	71	
23	N E	Clear	30	0	8	67	69	70	
24	S W	Clear	30	0	5	69	72		
25	S W	Clear	30	0	0	69	74	71	Rain.
26	N W	Cloudy	30	1	5	60	63	63	
27	N E	Clear	30	2	0	61	63	62	
28	N E	Cloudy	30	1	9	59	60	60	
29	N E	Clear	30	1	6	59	61	61	
30	N E	Cloudy	30	2	6	58	60	60	

OCTO-

OCTOBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	N E	Clear	30 3 0	58	61	62	
2	W	Clear	30 3 0	61	64	65	
3	S W	Clear	30 3 0	62	65	66	
4	S	Clear	30 2 6	67			
5	S	Clear	30 2 7	70	44	75	
6	S E	Clear	30 2 0	70	74	75	
7	N E	Cloudy	30 1 1	72	72	72	
8	N E	Clear	30 2 4	66	68	68	
9	S	Rain	30 0 5	67	72	71	
10	N E	Clear	30 3 0	64	65	65	
11	N	Cloudy	30 3 3	62	62	62	
12	N E	Cloudy	30 1 9	62	62	63	
13	S W	Cloudy	30 1 5	61	63	64	
14	N E	Clear	30 2 1	62	64	65	
15	N E	Clear	30 2 3	61	61	62	
16	N W	Cloudy	30 0 3	58	60	62	
17	N E	Cloudy	29 9 9	59	61	63	
18	N E	Clear	30 0 4	61			
19	S W	Clear	30 1 7	66			
28	N by E	Rain	29 6 8*	56	57		Much rain. * The loweft.
29	S W	Cloudy	29 7 0	48			
30	W b S	Clear	30 0 0	49	59	65	
31	W	Clear	30 2 0	50	60		

NOVEM-

NOVEMBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N E	Clear	30 5 0	56	53	52	
2	N E	Cloudy	30 4 8		58		
10	W	Clear	30 1 3	42	49	50	The ground frozen.
11	S W	Clear	29 7 0	42	52	55	
12	W	Clear	29 9 0	47	55	58	
13	W b N	Clear	30 0 8	41	45	49	
15	N	Cloudy	30 0 8	45	47	49	Froft.
16	N	Cloudy	30 0 9	44	45	47	Wind high and cold.
17	N	Clear	30 0 0	41	52	54	* Remarkable Aurora Borealis at 7 o'clock this evening. It was terminated towards the eaft by the two ftars β and θ in Auriga, and its greateft altitude reached nearly Capella. The ftars in the tail of the Great Bear terminated it to the weft. It is obfervable that the barometer was falling from the 18th, and was never obferved *fo low* but once, little after the vernal equinox, throughout the whole year, as it was fo foon after the Aurora. Its fudden rife was alfo remarkable. It became cloudy about one o'clock.
18	N	Clear	30 3 7	37	42	45	
19	N	Clear	30 2 9	33	39	43	
20	N W	Clear	30 2 4	37	48	50	
21	S W	Clear	30 2 4	39	55	57	
22	W b S	Clear	30 1 0	54	59	61	
23	S W	Clear	30 0 7	55	59	62	
24	S W	Clear	30 0 5	57	59	63	
25	W	Clear	30 0 3	55	56	59	
26	W	Clear	30 0 2	57	59	60	
27*	N W	Clear	30 0 0	36		48	
28	E h S	Cloudy	29 8 4	42	42	41	
29†	N W	Snow	29 3 9	41	39	37	
30	N W	Rain	29 4 4	39	39	40	† Snow fell $7\frac{1}{2}$ inches in 24 hours.

DECEMBER, 1777.

Days.	Winds.	Weather.	Barometer.			Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	W	Clear	29	7	7	35	45	47	
2	W	Clear	30	4	0	37	51		
3	W b S	Clear	30	4	0	37	48	50	
4	W b N	Clear	29	9	6	44	53	55	
5	N	Clear	30	1	0	40			
6	N	Clear	30	3	8	41	49	51	
7	S W	Clear	30	2	3	43	58	60	
8	S W	Clear	30	0	4	54	62	67	
9	N	Rain	29	8	0	52	47	46	Snow in the evening.
10	N b W	Cloudy	30	1	0	41	45	47	
11	N W	Clear	30	2	8	36	45	46	
12	S W	Clear	30	1	2	36	47	50	
13	S W	Clear	30	0	0	40	57	60	
14	N W	Cloudy	30	1	2	45	50	57	
15	N W	Clear	30	1	0	46	58	60	
16	W	Cloudy	30	0	0	46	59	62	
17	S W	Clear	29	9	0	49	51	59	
18	N E	Cloudy	29	9	0	47	51	56	
19	N b W	Clear	29	8	9	48	50	56	
20	N	Clear	30	0	8	46	49	51	
21	W	Clear	30	1	0	36	45	49	
22	S W	Clear	30	0	0	40	47	55	An Aurora.
23	N	Clear	30	0	0	43	44	45	Cloudy evening.
24	N E	Snow	30	0	0	40	41	43	
25	N	Cloudy	30	0	4	35	46	48	
26	S W	Cloudy	30	1	0	41	49	52	
27	S W	Cloudy	29	9	0	50	56	59	
28	N E	Rain	29	9	0	41	44	46	
29	N W	Clear	30	1	0	35	37	39	
30	N	Clear	30	4	6	25	27	28	
31	E by N	Clear	30	4	3	27	32	38	

JANUARY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N E	Clear	30 3 9	30	39	40	
2	S W	Cloudy	30 3 4	40	47	49	
3	S W	Cloudy	30 1 0	50	55	58	
4	W b N	Clear	30 0 0	41	57	59	
5	S W	Clear	30 1 0	50	60	63	
6	S W	Clear	30 2 0	55	62	66	
7	N b W	Clear	30 3 0	50	56		
8	W	Rain	30 1 0	45	52	57	
9	W	Clear	29 6 0	48			
10	S W	Clear	29 9 3	47	58	61	
11	N	Rain	29 9 7	48	47	47	
12	N W	Clear	30 1 3	43	47		
13	N	Clear	30 4 3	37	50	50	
14	W	Clear	30 1 8	47			
15	N	Clear	30 2 9	30	37	39	
16	N	Clear	30 3 5	27	40	43	
18		Clear	29 7 7	41			
19	W	Clear	30 0 1	41	45	47	
20	W	Cloudy	29 9 0	40	43	46	
21	S W	Cloudy	29 8 0	40	51	56	
22	N W	Clear	29 8 1	41	52	55	
23	N E	Cloudy	29 9 3	34	37	40	
34				37			
25	S W	Cloudy	29 8 0	47	50	57	
26	N W	Clear	30 1 0	38	42	44	
27	N W	Clear	30 3 2	32	40	43	
28	S W	Cloudy	30 2 0	38	52	57	
29	S W	Rain	29 9 3	61	62	63	
30	N W	Clear	30 3 2	47	55	56	
31	S W	Rain	30 2 0	48	54		

FEBRU-

FEBRUARY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	S W	Clear	30 3 0	54	59		
2	W	Cloudy	30 4 0	43	48	49	
3	W by S	Clear	30 3 3	40	53	57	
4	S W	Clear	30 3 3	56	60	61	
5	S W	Clear	30 3 0	52	61	62	
6	S E	Clear	30 3 0	57	57	57	
7	N W	Clear	30 1 8	49		55	
8	N E	Clear	30 2 7	45	44	44	
9	N W	Clear	30 2 7	31	40	41	
10	S E	Cloudy	30 1 3	35			Much rain in the evening.
11	W	Rainy	30 1 4	42	43	43	
12	N W	Clear	30 0 7	42	47	47	
13	W b N	Clear	30 0 9	40	47	48	
14	W	Clear	30 1 7	40	45	46	
15	E	Rainy	30 1 6	40	42	43	
16	W	Cloudy	30 1 0	41			
17	N W	Clear	30 3 3	31	40	42	
18	N W	Clear	30 3 4	32	41	42	
19	S W	Cloudy	30 1 0	39			
20	S W	Rain	29 8 3	45	46		
21	N W	Clear	30 1 0	38	45	46	
22	E b S	Cloudy	30 0 5	36	37	37	
23	W	Clear	29 7 7	39	49	47	
24	W b N	Clear	30 0 2	39	54		
25	S W	Cloudy	30 0 2	47	58	60	
26	S W	Cloudy	29 9 0	60	70	70	
27	W	Rain	29 6 6	64	60	59	
28	W b S	Clear	29 9 0	42	51		Wind W at 8.

MARCH,

MARCH, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	W	Clear	29 9 0	36	47	48	
2	N W	Clear	30 0 6	35	41	44	
3	E b N	Cloudy	27 6 0	36	32	31	Snows very faft.
4	N W	Clear	30 0 6	22½	35	39	
5	S W	Clear	30 0 6	23	42	45	
6	W	Clear	30 2 7	42	49	53	
7	S W	Clear	30 2 5	47	57	60	
8	E	Clear	30 2 0	45	46		
9	E	Cloudy	30 1 8	45	48	49	
10	N	Cloudy	30 1 9	45	47	57	
11	S W	Cloudy	30 1 5	47	52	62	
12	S W	Clear	30 0 4	59	69	70	Peach trees in bloffom.
13	S W	Clear	30 0 5	68	71	76	
14	S W	Clear	30 0 6	69	72	79	
15	S W	Cloudy	30 1 7	65	66	65	The wind at N W for part of the day.
16	S W	Cloudy	30 0 8	63	74	76	
17	S W	Clear	29 8 6	67	67	65	
18	N	Clear	30 2 9	51	51	58	
19	S	Cloudy	30 1 9	47	54	55	
20	N W	Clear	29 9 6	50	50	60	
21	N W	Clear	29 9 4	49	51	59	
22	N	Clear	30 2 6	39	47	54	
23	N b E	Cloudy	30 1 0	44	45	56	
24	E	Clear	30 0 6	45	51	53	
25	E	Rain	29 9 6	45	47	48	
26	S E	Rainy	29 6 5	48	57		
27	N	Clear	29 7 1	46			
28	N W	Clear	29 8 0	47	56	57	
29	S E	Rainy	29 3 0	50	54		Winds very high.
30	N W	Cloudy	29 5 0	45	48	49	Ditto.
31	N W	Clear	29 9 0	47	49	54	

APRIL, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N	Clear	29 9 0	48	54	55	
2	S E	Clear	29 7 3	54	57	59	
3	W	Clear	29 7 8	45	54	55	
5	W	Clear	29 7 7	44	57		
6	W	Clear	29 8 0	47	59	60	
7	N W	Cloudy	29 8 0	52	56	58	
8	N W	Clear	30 0 0	52	60	61	
9	E	Clear	30 2 0	51	60	61	
10	S W	Clear	30 2 0	51	63	64	
11	S E	Rainy	30 1 8	59	60	61	
12	E b S	Rainy	30 1 0	58	60	60	
13	S	Clear	30 1 3	59	63	70	
14	S W	Rainy	30 1 0	61	73	71	
15	S W	Rainy	30 0 5	66	67	69	
16	N	Rainy	29 9 0	59	62	65	
17	N W	Clear	29 7 7	58			
18							
26	W	Clear	29 8 6	56	65	61	
27	E	Rainy	29 9 3	58	56	56	
28	E b N	Rainy	29 7 3	57	58	57	
29	E b N	Rainy	29 5 7	57	57	57	
30	N b W	Clear	29 6 5	56	59	59	Lightening and thunder.

MAY,

MAY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	N W	Clear	29 6 8	54	58	59	
2	N E	Clear	29 6 2	55	57	58	
3	N E	Clear	29 7 6	55			
4	N W	Clear	29 6 3	56	62	63	Rain.
5	N W	Clear	29 8 8	61	68	71	
6	S W	Clear	29 8 2	62	72	77	
7	S E	Clear	29 8 0	67	76	79	
8	N	Clear	29 8 6	65	66	69	
9	W	Clear	30 0 4	59	60	66	
10	S	Clear	30 0 3	65			
11	W	Clear	29 8 0	68	75	79	
12	S	Clear	30 0 0	69	76	78	
15	S W	Clear	30 1 7	69	75	77	
16	S W	Clear	30 1 3	75	80	83	Rain.
17	S W	Clear				79	Rain.
18	S W	Clear	30 1 2	78	80	83	
19	S W	Clear	30 0 0	78	82	85	
20	S W	Clear	30 0 0	75	82	85	
21	S W	Clear	30 0 4	71	80		
22	N E	Rain	30 0 3	70	70	71	
23	N by E	Clear	30 1 0	65	74	75	
24	E	Clear	30 0 3	64	72	80	
25	S W	Clear	29 9 0	75	81	85	
26	N	Clear	29 9 4	74	81		
27	W	Clear	29 8 0	75	81	82	Rain.
28	W	Clear	29 9 0	70	72	73	
29	N W	Clear	30 1 7	60	63	65	
30	W	Clear	30 3 0	60	62	63	
31	E by S	Cloudy	30 2 9	60	66	70	

JUNE,

JUNE, 1778.

Days.	Winds.	Weather.	Barometer.			Thermometer. 8 H.	12 H.	3 H.	Obfervations.
1	S E	Rainy	30	1	0	65	69	72	
2	N W	Cloudy	30	0	0	70	75	76	
3	N b E	Cloudy	30	0	0	70	74	75	
4	E	Clear	30	0	2	64	70	71	
5	E	Cloudy	30	0	2	64	70	71	
6	S E	Cloudy	30	0	0	65	75	78	
7	E	Cloudy	29	8	8		75	73	
8	N	Clear	29	8	7	65	74	75	
9	N	Clear	30	0	6		75	76	
11	W b S	Clear	29	9	9	70	80	84	
12	N E	Cloudy	29	8	0	70	73	74	Rain.
13	N E	Clear	29	9	0	68	73	72	
14	N E	Clear	29	9	6	64	65	69	
15	N E	Cloudy	30	0	3	68	70	72	
16	W	Clear	30	1	0	67	75	78	
17	S W	Clear	30	0	0	70	80	84	
18	S W	Clear	29	9	5	78	87	86	
19	E	Rainy	29	9	3	78	79	79	
20	E	Rainy	29	9	0	79	79	79	
21	S E	Cloudy	29	9	7	75	83	86	Rain.
22	S W	Clear	29	9	9	76	83	87	Much Rain.
23	S b E	Cloudy	29	9	5	76	77	77	
24	S b E	Cloudy	29	9	2	75	80	81	
25	S W	Clear	29	9	7	78	84	86	
26	S W	Clear	30	0	0	78	85	87	
27	S W	Clear	30	0	0	80		86	
28	S W	Clear	29	9	5	80	87	86	
29	S W	Clear	29	9	9	75	86	88	
30	S W	Clear	30	0	0	80	86	89	

JULY,

JULY, 1778.

Days.	Winds.	Weather.	Barometer.			Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S W	Clear	29	9	9	80	86	89	
2	W	Clear	29	9	0	70	84	80	
3	W	Clear	29	9	5	77	83	81	Rain.
4	N W	Cloudy	29	8	2	73	75	76	
5	E	Clear	30	0	5	73	76	79	
6	N E	Clear	30	1	6	75	77	81	
7	W	Clear	30	0	7	76	79	88	
8	S W	Clear	30	0	3	78	85	86	
9	S W	Clear	30	0	0	80	86	88	Rain.
10	S W	Clear	30	0	0	80			
11	S W	Clear	30	0	0	80	88	90	
12	S W	Clear	29	9	5	80	87	88	Rain.
13	E	Cloudy	29	9	7	77	80	81	
14	N E	Cloudy	30	0	0	70	73	75	
15	N W	Clear	30	2	0	71	75	77	
16	N E	Clear	30	2	5	73	75	78	
17	E	Cloudy	30	2	2	73	76	77	
21	N E	Cloudy	30	0	0	77	77	78	Rain.
22	E	Rain	29	8	8	75	76	76	Rain.
23	N E	Cloudy	29	9	0	69	73	75	
24	E	Clear	29	9	9	72	77	77	Rain.
25	E	Cloudy						77	Rain.
26	N E	Rain	30	1	7	73	75	77	Rain.
27	N E	Cloudy	30	1	6	73	75	78	Rain.
28	S W	Clear	30	0	7	75	82	85	Rain.
29	S W	Clear	30	0	0	75	83	86	
30	S W	Clear	30	0	0	78	85	87	
31	S W	Clear	30	0	0	78	85	87	

X AUGUST,

AUGUST, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.	12 H.	4 H.	Obfervations.
1	S W	Clear	30 0 0	79	86	90	
2	S W	Clear	30 0 0	80	81	89	
3	S W	Clear	29 9 7	78	84	86	
4	S W	Cloudy	29 9 6	75	77	79	
5	S W	Cloudy	30 0 5	75	75	81	
6	S W	Clear	30 1 3	76	79	82	
7	S W	Clear	30 1 9	76	80	83	
8	S E	Clear	30 2 0			85	
9	E	Cloudy	30 0 0	76		81	
10	S	Clear	30 0 2	77	78	72	
11	E	Rain	29 7 3	77	77	76	Much rain.
12	N W	Cloudy	29 8 0	72			
13				67	67	68	Rain.
14	S E	Cloudy	29 7 8	66	67	67	
15	S	Cloudy	29 9 0	68	70	75	
16	S	Cloudy	29 9 9	75	80	82	
17	S W	Cloudy	30 0 2	75			

Defcription

N° XI.

Defcription of a Machine for Meafuring a Ship's Way through the Sea, *by* F. HOPKINSON, *Efquire.*

Read July 11, 1783.

THE errors and uncertainties incident to the mariner's log, in common ufe, are too obvious not to be univerfally acknowledged. Were it not for the obfervations navigators are accuftomed to make of a fhip's progrefs, by the apparent paffage of the water along fide, and the figns that ufually prefent on approaching the land, the log, alone, would be a very unfafe dependence.

Several attempts have, therefore, been made to improve the log, and render its indications more accurate, but without fuccefs. All the machines of this kind, that I have heard of, were compofed of a number of wheels, which were to be put in motion by the twifting of a line let out aftern, having a drag at the end fo conftructed as to whirl round, fafter or flower, according to the motion of the fhip.

The objections to a machine, fo conftructed, are, *Firft*, If the line is not very long the drag will be confiderably affected by the fhip's wake; and, if it is very long, the twiftings will be irregular, and the line liable to kink. *Secondly*, If the drag is fo heavy as to fink below the bottoms of the waves, when the fhip fails faft, it will be too heavy, and fink too deep when fhe hath but a flow progreffive motion: Or, otherwife, if the drag is of a proper weight when fhe makes but little way, it will fkip from wave to wave through the air when fhe fails with a brifk gale, and fo be of no ufe. And, *Thirdly*, It will be liable to moft of the other irregularities to which the log in common ufe is expofed.

The machine now propoſed will, it is hoped, be free from, at leaſt, ſome of theſe objections. And, although it may not be able to aſcertain a ſhip's way through the ſea to a mathematical preciſion, yet if it ſhould be found to anſwer the purpoſe better than any inſtrument hitherto contrived, it may be admitted as an acquiſition to the art of navigation.

This machine, in its moſt ſimple form, is repreſented by *Fig.* 1, *Plate* 3. Wherein AB is a ſtrong rod of iron moveable on the fulcrum C. D is a thin circular plate of braſs rivetted to the lower extremity of the rod. E an horizontal arm connected at one end with the top of the rod AB by a moveable joint F, and at the other end with the bottom of the index H by a like moveable joint G. H is the index turning on its centre I and travelling over the graduated arch K; and L is a ſtrong ſpring bearing againſt the rod AB and conſtantly counteracting the preſſure upon the palate D. The rod AB ſhould be applied cloſe to one ſide of the cut water or ſtem, and ſhould be of ſuch a length that the palate D may be no higher above the keel than is neceſſary to ſecure it from injury when the veſſel is aground or ſails in ſhoal water. As the bow of the ſhip curves inward towards the keel M, the palate D will be thrown to a diſtance from the bottom of the veſſel, although the perpendicular rod, to which it is annexed, lies cloſe to the bow above; and, therefore, the palate will be more fairly acted upon. The arm E ſhould enter the bow ſomewhere near the hawſer hole, and lead to any convenient place in the forecaſtle, where a ſmooth board or plate may be fixed, having the index H and graduated arch K upon it.

It is evident from the figure, that as the ſhip is urged forward by the wind, the palate D will be preſſed upon by the reſiſting medium, with a greater or leſs force according to the progreſſive motion of the ſhip: and this will operate upon the levers ſo as to immediately affect the index; making the leaſt encreaſe or diminution of the ſhip's way

way viſible on the graduated arch. The ſpring L always counteracting the preſſure upon the palate, and bringing back the index on any relaxation of the force impreſſed.

A ſhip going through the ſea opens a paſſage for herſelf, making a hole in the water equal to her immerged bulk. As ſhe paſſes on, this vacancy is filled up by the tumbling in of the waters from each ſide, and from underneath, at the ſtern, with great violence. So that there is a fair current of water from her bow to her ſtern, paſſing under the bottom and along ſide; the force of which current is in direct proportion to the velocity of the ſhip's progreſſive motion. This machine is, therefore, advantageouſly placed at the bow of the ſhip, where the current firſt begins, and acts fairly upon the palate; in preference to the ſtern, where the tumultuous cloſing of the waters cauſes a wake, viſible to a great diſtance. The palate D is ſunk nearly as low as the keel, that it may not be influenced by the heaping up of the water, and the daſhing of the waves at and near the water-line. The arch K is to aſcertain how many knots or miles ſhe would run in one hour, at her then rate of ſailing. But the graduations on this arch muſt be unequal; becauſe the reſiſtance of the ſpring L will encreaſe as it becomes more bent; ſo that the index will travel over a greater ſpace from one to five miles (which I ſuppoſe to be a medium) than from five to twelve. Laſtly, the palate, rod, ſpring and all the metallic parts of the inſtrument ſhould be covered with a ſtrong varniſh, to prevent ruſt from the corroſive quality of the ſalt water and ſea air.

IMPROVEMENT OF THIS MACHINE.

Let the rod or ſpear AB *Fig.* 1, be a round rod of iron or ſteel; and inſtead of moving on the fulcrum or joint as at C, let it paſs through and turn freely in a ſocket, to which ſocket the moveable joint muſt be annexed as, repreſented

presented in *Fig.* 2. The rod must have a shoulder to bear on the upper edge of the socket, to prevent its slipping quite down. The rod must also pass through a like socket at F, *Fig.* 1. The joint of the lower socket must be fixed to the bow of the ship, and the upper joint or socket must be connected with the horizontal arm E. On the top of the uppermost socket, let there be a small circular plate, bearing the 32 points of the mariner's compass; and let the top of the rod AB come through the centre of this plate, so as to carry a small index upon it, as is represented in *Fig.* 3. This small index must be fixed to the top of the rod on a square; so, that by turning the index round the plate, the rod may also turn in the sockets, and of course carry the palate D round with it. The little index always pointing in a direction with the face of the palate. The small compass plate should not be fastened to the top of the socket, but only fitted tightly on, that it may be moveable at pleasure. Suppose, then, the intended port to bear south-west from the place of departure; the palate must be turned on the socket till the south-west point thereon looks directly to the ship's bow; so that the south-west and north-east line on the compass plate may be precisely parallel with the ship's keel; and in this position the plate must remain during the whole voyage. Suppose then the ship to be sailing in the direct course of her intended voyage, with her bowsprit pointing south-west, let the little index be brought to the south-west point on the compass plate, and the palate D will necessarily present its broad face toward the port of destination; and this it must always be made to do, be the ship's sailing course what it may. If, on account of unfavourable winds, the ship is obliged to deviate from her intended course, the little index must be moved so many points from the south-west line of the compass plate, as the compass in the binnacle shall shew that she deviates from her true course. So that, in whatever direction the ship shall sail, the palate D will always

always look full to the ſouth-weſt point of the horizon, or towards the port of deſtination; and, conſequently, will preſent only an oblique ſurface to the reſiſting medium—more or leſs oblique as the ſhip deviates more or leſs from the true courſe of her voyage. As, therefore, the reſiſtance of the water will operate leſs upon the palate in an oblique than in a direct poſition, in exact proportion to its obliquity, the Index H will not ſhew how many knots the veſſel runs in her then courſe, but will (it is expected) indicate how many ſhe gains in the direct line of her intended voyage. Thus, in *Fig.* 5, if the ſhip's courſe lies in the direction of the line AB, but ſhe can ſail by the wind no nearer than AC; ſuppoſe then, her progreſſive motion ſuch as to perform AC, equal to five knots or miles in one hour; yet the index H will only point to four knots on the graduated arch, becauſe ſhe gains no more than at that rate on the true line of her voyage, viz. from A to B. Thus will the difference between her real motion and that pointed out by the index be always in proportion to her deviation from the intended port, until ſhe ſails in a line at right angles therewith, as AD; in which caſe the palate would preſent only a thin ſharp edge to the reſiſting medium; the preſſure of which ſhould not be ſufficient to overcome the friction of the machine, and the bearing of the ſpring L. So that at whatever rate the ſhip may ſail on that line yet the index will not be affected: Shewing that ſhe gains nothing on her true courſe. In this caſe, and alſo when the veſſel is not under way, the action of the ſpring L ſhould cauſe the index to point at 0; as repreſented by the dotted lines in figure 1 and 4.

As the truth of this inſtrument muſt depend on the equal preſſure of the reſiſting medium upon the palate D according to the ſhip's velocity, and the proportionable action of the ſpring L, there ſhould be a pin or ſcrew at the joints C and F, ſo that the rod may be readily unſhipped and taken in, in order to clean the palate from any

foulneſs

foulneſs it may contract; which would greatly increaſe its operation on the index H, and thereby render the graduated arch, falſe and uncertain.

Further, the ſpring L may be expoſed too much to injury from the ſalt water, if fixed on the outſide of the ſhip's bow. To remedy this it may be brought under cover by conſtructing the machine as repreſented by figure 4. Where, A B is the rod, C the fulcrum or centre of its motion; D the palate; E the horizontal arm leading through a ſmall hole into the forecaſtle: M is a ſtrong chain, faſtened at one end to the arm E and at the other to a rim or barrel on the wheel G, which by means of its teeth gives motion to the ſemicircle I and index H. The ſpring L is ſpiral and encloſed in a box or barrel, like the main ſpring of a watch: A ſmall chain is fixed to and paſſing round the barrel is faſtened by the other end to the fuzee W. This fuzee is connected by its teeth with the wheel G, and counteracts the motion of the palate D. NN are the two ſockets through which the rod A B paſſes, and in which it is turned round by means of the little index R. S is the ſmall compaſs plate, moveable on the top of the upper ſocket N. The plate S hath an upright rim round its edge cut into teeth or notches; ſo that when the index R is a little raiſed up, in order to bring it round to any intended point, it may fall into one of theſe notches and be detained there: Otherwiſe the preſſure of the water will force the palate D from its oblique poſition, and turn the rod and index round to the direction in which the ſhip ſhall be then ſailing. Should it be apprehended that the palate D, being placed ſo far forward, may affect the ſhip's ſteerage or obſtruct her ſailing, it ſhould be conſidered that a very ſmall plate will be ſufficient to work the machine. I ſhould ſuppoſe that one of three or four inches in diameter would fully anſwer the purpoſe: And yet not be large enough to have any ſenſible operation on the helm or ſhip's way.

The

The greateſt difficulty, perhaps, will be in graduating the arch K; (if the machine is conſtructed as in *figure* 1,) the unequal diviſions of which can only be aſcertained by actual experiment on board of each ſhip reſpectively; in as much as the accuracy of theſe graduations will depend on three circumſtances, viz. The poſition of the fulcrum C with reſpect to the length of the rod, the ſize of the palate D and the ſtrength or bearing of the ſpring L. When theſe graduations, however, are once aſcertained for the machine on board of any one veſſel, they will not want any future alterations; provided the palate D be kept clean, and the ſpring L retains its elaſticity.

But the unequal diviſions of the graduated arch will be unneceſſary, if the machine is conſtructed as in *figure* 4. For as the chain goes round the barrel L, and then winds through the ſpiral channel of the fuzee W, the force of the main-ſpring muſt operate equally, or nearly ſo, in all poſitions of the index; and conſequently, the diviſions of the arch K may, in ſuch caſe, be equal.

After all, it is not expected that a ſhip's longitude can be determined to a mathematical certainty by this inſtrument. The irregular motions and impulſes to which a ſhip is continually expoſed, make ſuch an accuracy unattainable perhaps by any machinery: But if it ſhould be found, as I flatter myſelf it will on fair experiment, that it anſwers the purpoſe much better than the common log, it may be conſidered as an acquiſition to the art of navigation.

It ſhould be obſerved that in aſcertaining a ſhip's longitude by a time-piece, this great inconvenience occurs, that a ſmall and trifling miſtake in the time, makes a very great and dangerous error in the diſtance run: Whereas the errors of this machine will operate no farther than their real amount; which can never be great or dangerous, if corrected by the uſual obſervations made by mariners for correcting the common log.

A like machine made in its ſimple form, (as at fig. 1.) ſo conſtructed as to ſhip and unſhip, might occaſionally be applied along ſide about midſhips in order to aſcertain the lee-way; which, if rightly ſhewn will give the ſhip's preciſe longitude. As to ſea-currents, this and all other machines hitherto invented, muſt be ſubject to their influence; and proper allowances muſt be made, according to the ſkill and knowledge of the navigator.

Laſtly, ſome diſcretion will be neceſſary in taking obſervations from the machine to be entered on the log-book. I mean, that the moſt favourable and equitable moment ſhould be choſen for the obſervation. Not whilſt the ſhip is rapidly deſcending the declivity of a wave; or is ſuddenly checked by a ſtroke of the ſea; or is in the very act of plunging. In all caſes, I ſuppoſe, periods may be found in which a ſhip proceeds with a true average velocity; to diſcover which a little experience and attention will lead the ſkilful mariner*.

N° XII.

Account of an Electrical Eel, or the Torpedo of Surinam, by WILLIAM BRYANT, *Eſquire.*

SURINAM a colony of South America belonging to the ſtates of Holland, abounds with as many natural curioſities as any country in the world. But that which I look upon to be as ſurpriſing as any in it, and which I believe has not yet been accurately deſcribed, is a fiſh of the ſpecies of eel, and is caught there in nets among other fiſh; generally in muddy rivers, and I believe is found in moſt of the neighbouring provinces. In ſize and colour

* An ingenious mechanic would probably conſtruct this machine to better advantage in many reſpects. The author only meant to ſuggeſt the principle; experiment alone can point out the beſt method of applying it. He is ſenſible of at leaſt one deficiency, viz. That the little index R, figure 4, will not be ſtrong enough to retain the palate D in an oblique poſition when the ſhip is ſailing by the wind; more eſpecially as the compaſs plate S, in whoſe notched rim the index R is to fall, is not fixed to, but only fitted tight on the ſocket N. Many means however might be contrived to remedy this inconvenience.

Plate.3.

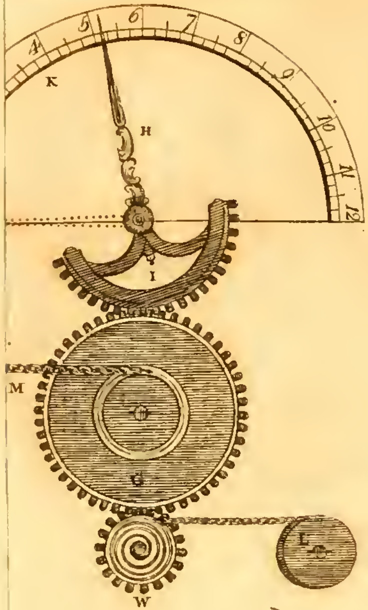


Fig. 6.

Fig. 7.

Fig. 8.

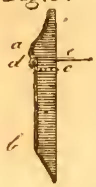


Fig. 9.

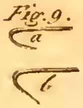


Fig. 10.

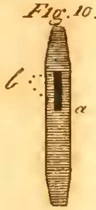
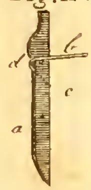


Fig. 11.



Page 185.

Fig. 12.

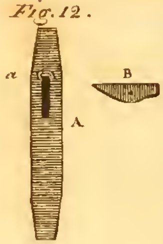


Fig. 13.

Fig. 5.

Fig. 14.

Fig. 15.

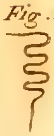


Fig. 17.

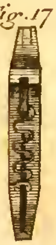


Fig. 16. *Page. 201.*

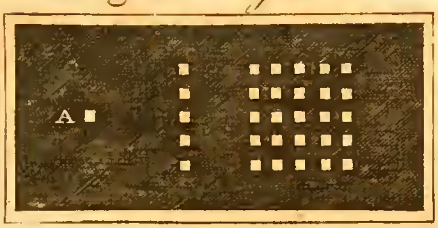
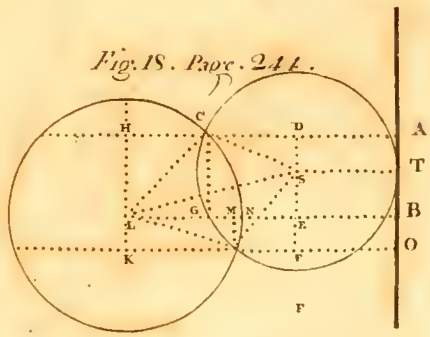
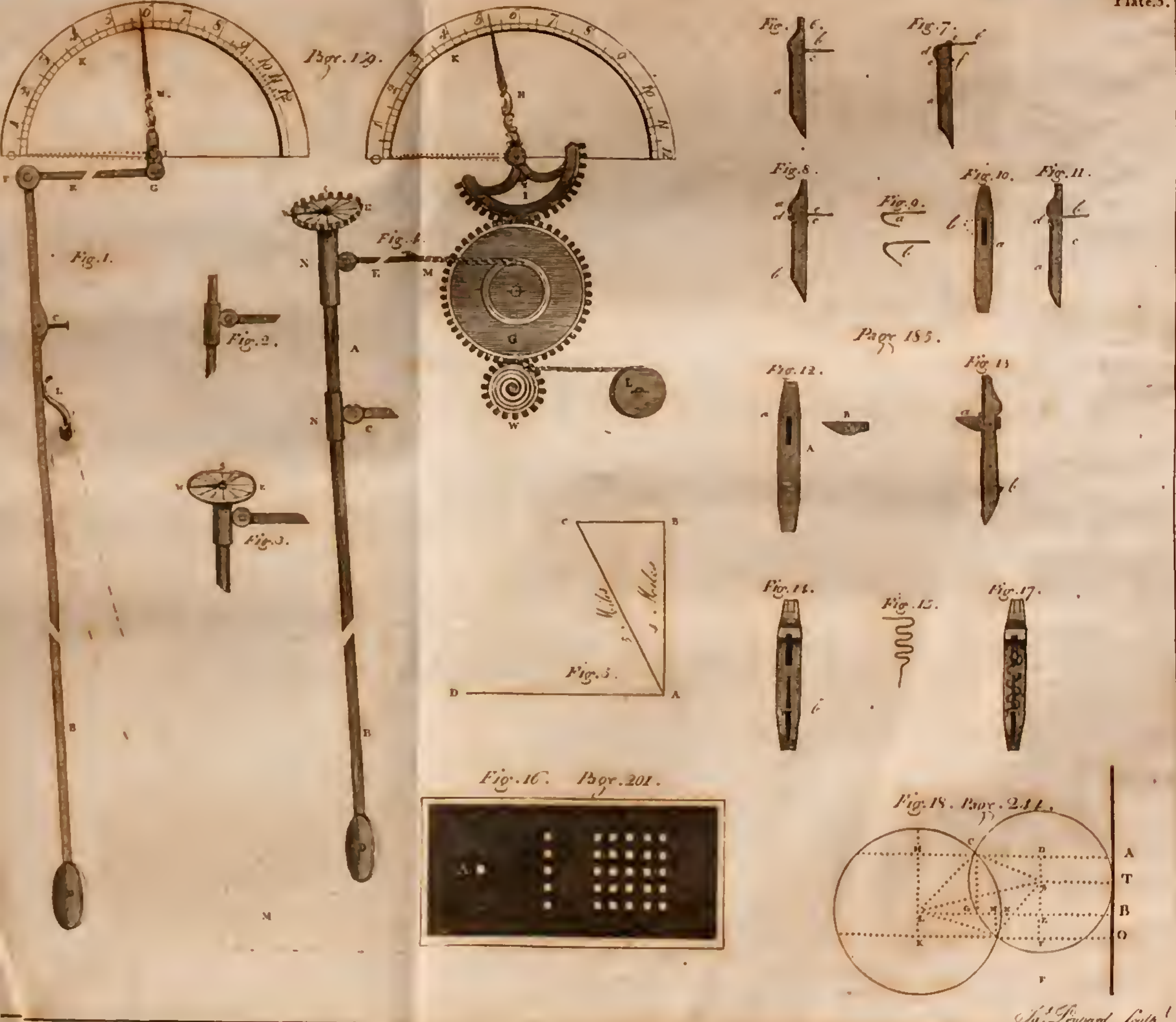


Fig. 18. Page. 244.



Ja.s Poupard sculp.t

Plate.3.


Page 179.
Fig.1.
Fig.2.
Fig.3.
Fig.4.
Fig.5.
Fig.6.
Fig.7.
Fig.8.
Fig.9.
Fig.10.
Fig.11.
Page 185.
Fig.12.
Fig.13.
Fig.14.
Fig.15.
Fig.17.
Fig.16. Page 201.
Fig.18. Page 214.
A
T
B
O

colour it is not unlike a common eel of Europe or America, and in ſhape reſembles it more, except that it is thicker in proportion to its length, and the head is more flat and not ſo pointed; but differs from them in this reſpect, that it comes to the ſurface to breathe in the air. It is called by the Dutch *Beave Aal*, and by the Engliſh inhabitants the Numbing Eel. As to the other qualities, of which I mean chiefly to take notice, and which I think are as different from the Torpedo of Europe, as the fiſh is in ſhape, they are as follows.

On touching the fiſh as it lies in the water in a tub provided for it, a ſudden and violent ſhock is received, in all reſpects like that which is felt on touching the prime conductor, when charged with the electrical fluid from the globe; and like that chiefly, affects the ends of the fingers and elbow. Gently holding the tail of the fiſh with one hand and touching the head with the other, a very violent ſhock is felt in both elbows and through the breaſt and ſhoulders. I at firſt imagined that the violence of the ſhock proceeded from both arms receiving it at the ſame time, and that the pain was no more than that of the two ſtrokes added together; but I found myſelf miſtaken. For upon ſeven perſons joining hands, and the firſt taking hold of the tail (which may with more eaſe be held than the head) and the ſeventh at the ſame time touching the head, we were all affected in both elbows, and that in the ſame manner as I remember to have been in the electrical experiment, when ſeveral perſons take hold of the wire and the equilibrium is reſtored by the fluids paſſing through their bodies.

I find the ſhock may be received through metallic ſubſtances. On touching the fiſh with an old ſword blade I was ſtrongly affected. But arming it with ſealing-wax and taking hold of that part which was covered with it, the electrical fluid (I cannot help calling it ſo) would not paſs. Neither has it any effect on the body when touched with

 glaſs

glafs bottle, fealing-wax, &c. Yet I cannot obferve the leaft diminution of this quality by placing the tub which contains the fifh on glafs bottles; it continues the fame in all refpects. So that whether it has an unaccountable faculty of collecting a quantity of the fluid from the furrounding waters, or through the body of the perfon touching it, or has in its own body a large fund which it can difcharge at pleafure, I am greatly at a lofs to think or imagine.

Although it has no effect on the human body when touched with a piece of wood, or indeed any other fubftance not metallic; yet an accident difcovered to me, that on fome occafions the effect would be fenfible through wood. For one morning while I was ftanding by, as a fervant was emptying the tub, which he had lifted intirely from the ground, and was pouring off the water to renew it, and the fifh left almoft dry, the negro received fo violent a fhock as occafioned him to let the tub fall, and calling another to his affiftance, I caufed them both to lift the tub free from the ground, when pouring off the remains of the water they both received fmart fhocks and were obliged to defift from emptying the tub in that manner. This I afterwards tried myfelf and received the like fhock. This fifh indeed was one of the largeft I have feen and but newly caught. For I obferve that after being fometime confined in a tub and wanting perhaps their natural food, they lofe much of the ftrength of this extraordinary quality. I am fometimes apt to conjecture, that this animal has the power of communicating the ftroke when, and with what degree of force it will; and that it ferves him as a weapon of defence againft his enemies. For I have often obferved that on firft taking hold of it, the fhock is tolerable; but as foon as he perceives himfelf the leaft confined, it is much more violent. This I experienced to my coft, as I one day took hold of it, about the middle of the fifh, I lifted it partly out of the water, when on a fudden I received fo fmart a fhock that

that it occaſioned a ſtrong contraction in the bending muſcles of my fingers, and I could not immediately let it go; but endeavouring to diſengage my hand threw it on the ground; taking hold of it a ſecond time, to return it into the tub, I was more ſtrongly affected than at firſt, and that not only in my hands and arms, but throughout my whole body; the forepart of my head and the back part of my legs ſuffered principally; and in the ſame manner as on receiving a very ſmart ſhock from a highly charged phial in electrical experiments.

On obſerving that the ſenſation occaſioned by the ſhock as to the nature and degree of ſtrength upon touching different parts of the fiſh, was different, I was at firſt inclined to think it might be owing to its having an extraordinary faculty of containing more of the fluid in one part of its body than in another. The tail part to above one third of its length, occaſions rather a numbneſs and tingling, than pain, but on applying the end of the fingers to the back, head, and under part of its body, it cauſes a ſharp pricking pain. This may poſſibly be accounted for by the difference in the texture of the ſurface of the ſkin, as the manner of the electrical fluids coming from a glaſs tube is different when its ſurface is altered by being rubbed with different ſubſtances, as has been lately taken notice of in a letter to the Royal Society.

Theſe are the principal obſervations, the ſhort time I reſided at Surinam, allowed me an opportunity of making, relating to this extraordinary animal.

Obſervations

N° XIII.

Obſervations on the Numb Fiſh, or Torporific Eel, by HENRY COLLINS FLAGG, *South-Carolina.*

Read March 7th, 1783.

I DO myſelf the pleaſure, though late I confeſs, to comply with my promiſe of communicating ſome obſervations on the Numb Fiſh, or *Torporific Eel*, which I think a more proper name. Theſe obſervations are contained in two letters I had the honor to write to the Rev. Dr. Stiles, a member of your philoſophical ſociety, from Rio Eſſequebo. Pleaſe to accept the following extracts.

The apparent difference between the torporific eel and that uſually caught in your harbour is, the former is flatter on the back and head, the upper part of which is perforated with ſeveral holes*, and has on each ſide, behind, a ſmall fin which ſome ſay are elevated or depreſſed as the fiſh is pleaſed or not†; the body I think is larger in proportion to the length, and it has a broad fin connected to the belly and continued to the tail. I have ſeen this fiſh four feet long. The ſenſation occaſioned by touching it appeared to me exactly ſimilar to an electric ſhock. I have as yet been able to procure only one of theſe eels, and that was injured by laying too long dry before it came to me. The following are the remarks I made the little time it lived. I received the fiſh from a negro in a wicker baſket, and laying it on the ground felt a conſiderable ſhock, as I did too when I turned the fiſh out of the baſket into a tub of

* Theſe holes do not penetrate to the mouth, nor could I diſcover the uſe of them. But I was not ſufficiently exact in my diſſection of the head, or I think I might have found the termination of theſe ducts.

† This is true.

of water. The ſhock is greater if the fiſh is enraged; but whether repeated touches will exhauſt this ſtrange power, as frequently repeated bites do the viperine and ſome other poiſons for a time, I believe no experiment has yet determined*. If a perſon hold his finger in the water ſeveral inches diſtant from the fiſh and another touch it, a ſhock equally ſevere is felt by him who does not touch it. The ſame thing happens if the fiſh exerts itſelf without being touched. If a number of perſons join hands, and one touch the eel, they are all equally ſhocked, unleſs there ſhould happen to be one of the number incapable of being affected by the eel, which is the caſe of a very worthy lady of my acquaintance, who can handle this fiſh at will. I am informed ſome Indians and negroes can do the ſame; whether by the aſſiſtance of any means to counteract the power of the eel, I know not; but am perſuaded it is ſomething in the conſtitution of the lady†. The eel I had obtained got out of the tub, and it was with ſome difficulty I returned it, for the repeated ſhocks I received through a piece of deal board eighteen inches long, with which I attempted to lift it, made my arms ache very much, and for a conſiderable time. I think the numbneſs occaſioned by touching this eel continues longer than that from an electric ſhock of the ſame degree of force, and I have been aſſured by a perſon of good ſenſe and veracity, that a negro fellow formerly being bantered by his companions for his fear of this eel, determined to give a proof of his reſolution, and attempted to graſp it with both hands. The unhappy conſequence was, a confirmed paralyſis of both arms. I hear this fellow is ſtill living in the iſland of St. Chriſtopher's; if ſo, I can obtain more ſatisfaction, for I have my doubts of the negro's honeſty‡. But very certain

* I am ſince convinced they do.

† This lady, when I became acquainted with her, was far gone in an hectic fever. And I did not think to enquire if ſhe could treat the fiſh with ſo much familiarity while in a perfect ſtate of health.

‡ This account was afterwards confirmed to me, with the further information, that after ſeveral years the negro recovered the uſe of his arms by ſlow degrees, and I think without any aſſiſtance from medicine.

tain it is, that many perſons have been knocked down by the ſeverity of the ſhock. The languid ſtate in which I found the eel the morning after it was taken, gave me an opportunity of obſerving that though I could perceive no ſhock by touching it on or near the tail; yet applying my finger near the belly, the torporific power was very conſiderable, notwithſtanding the fiſh was now almoſt dead. This I repeated ſeveral times, as a remark of ſome conſequence in aſſiſting us to determine whether, or how far, the emiſſion of torporific particles depends on the exertion of any muſcular force*; upon which principle Mr. Reamure accounts for the benumbing power of the Torpedo. I much doubt if the moſt acute eye can diſcern any motion in the eel at the time it ſhocks†. I have been ſo particular in taking notice of the baſket and deal-board, becauſe it has been aſſerted that the eel ſhocks only by immediate contact, through metal or very hard wood. This eel is frequently eat by the negroes, and reckoned very delicious. Its common food is ſhrimps or any ſmall fiſh.

I have lately made another experiment upon the torporific eel. It was ſuggeſted to me by the very great ſimilarity between the effects of a ſhock from the eel and an electric machine. I held an iron rod between two pieces of glaſs and touched the eel with it, but could not perceive the leaſt ſhock. I held the rod in a ſilk handkerchief with the ſame effect. I repeated theſe experiments on two eels with equal ſucceſs. I think this experiment demonſtrates that the electric and torporific particles are the ſame. I have tried the effects of this fiſh upon the needle of a compaſs but perceived no influence. I have not, however, done

* I have not ventured as yet to give any opinion of the ſtrange property by which this fiſh becomes the conductor of the electric fluid. But that the emiſſion of it depends upon the exertion of muſcular force may, I think, be concluded from hence; that, as has already been determined, repeated exertions will exhauſt its power to ſhock for a time, and before it can again exert its influence, a freſh quantity of fire muſt be collected; nor do I think the experiment I made on the dying eel invalidates this opinion, for to the beſt of my recollection it ceaſed to ſhock ſome time before its death.

† I am informed the motion is perceptible, though I confeſs I could not diſtinguiſh it.

done with the eel, and hereafter will repeat all the old and make new experiments upon it*.

This fiſh raiſes its head every few minutes above the water to reſpire.

I have ſeen negroes take hold of it, at firſt very cautiouſly, receiving many light ſhocks, but preſently have graſped it hard and taken it out of the water.

There is a kind of light wood through which the eel cannot ſhock.

Mrs. Behn, in her Oroonoko, gives a deſcription of this fiſh, which ſhe calls the numb-eel, and ſays it is taken in the river Surinam.

From the above experiments, partial as they are, I leave you, ſir, to judge how far the torporific and electric fluids are alike.

I am, with the greateſt reſpect and eſteem,

Your moſt humble ſervant,

South-Carolina, October 8, 1782. HENRY COLLINS FLAGG.

N° XIV.

To DAVID RITTENHOUSE, *Eſquire, from* JOHN PAGE, *Eſquire.*

Williamſburg, December 4, 1779.

DEAR SIR,

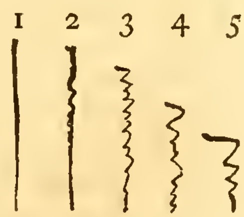
Read May 2d, 1783.

I HAVE often thought there was a ſtrong reſemblance between ſome of the phenomena of electricity and magnetiſm, and fancied I ſaw ſomething like the two electricities in the attraction and repulſion of the

Z

* I had not been long in South-America when I made my obſervations; ſoon after which, the neceſſary avocations of my profeſſion, together with that relaxation of the mental powers generally conſequent upon the laſſitude of body incident to the inhabitants of warm climates, indiſpoſed me to the farther proſecution of experiments I am now mortified at not having made.

the two poles. I have amuſed myſelf with ſuppoſing that magnetiſm is only a ſpecies of electricity, whoſe *matter* is as yet not diſcovered by human ſight; as that of electricity was, when a few years ago, it was perceivable only by its effects in attracting or repelling light bodies, as magnetiſm now is in attracting or repelling iron. Experiments by which polarity may be given to needles by means of electricity, perhaps, further improved and cloſely attended to, might throw great light on this ſubject. I wiſh we had more caſes ſtated of the effects of lightening and the Aurora Borealis on the needle. But mentioning the Aurora Borealis recalls to my mind, the meteor which was ſeen at many diſtant places in Virginia on the 31ſt of October, at about 6^{h} 10^{m} P. M. It was what is vulgarly called a falling ſtar. It fell as ſeen at Roſewell about three or four degrees to the north of weſt and left a bright trail of light behind it; which extended from the horizon perpendicularly above 7°; unluckily I loſt a view of it when falling, but was called out time enough to ſee the grand and beautiful appearance of its trail of light. It was ſeen for near 15^{m}, it was as bright as ſhining ſilver, and as broad as the enlightened part of the new moon, when firſt viſible, and about 7° in length. It might be repreſented by N° 1, when I firſt ſaw it, and by the other figures at intervals of about a minute after. Juſt before it diſappeared it reſembled the edge of a cloud. The ſky was remarkably clear and ſerene. It appeared in the ſame manner exactly to ſeveral gentlemen above an hundred miles from Roſewell, but on a different point of the compaſs. I have not yet had ſo accurate an account of its bearing as to aſcertain its height and diſtance. Did you ſee any thing of it?



I am, dear ſir, yours moſt ſincerely,

JOHN PAGE.

From DAVID RITTENHOUSE, *Esquire*, *to* JOHN PAGE, *Esquire*.

Philadelphia, January 16, 1780.

DEAR SIR,

Read May 2, 1783. I DESIGN to give you my thoughts on Magnetism in some future letter, at present I shall confine myself to the subject of the latter part of yours of the 4th of December last.

The extraordinary Meteor you mention was likewise visible here, the air being serene and clear. I did not see it until the bright streak was become very crooked, it then bore S. 70° W. nearly, from Philadelphia, and comparing this course with that observed by you, I find it must have fallen on or near the Ouasiota mountains mentioned in Lewis Evans's map, about 480 miles from Philadelphia and 365 from Williamsburg. And taking its altitude 7,° as observed by you, adding $2\frac{1}{2}$ degrees for the depression of that place below your horizon, its entire apparent altitude above the spot where it fell was $9°\frac{1}{2}$, which, on a radius of 365 miles, will be 61 miles perpendicular height. The breadth of the luminous vapour was, I think, in some places, when I saw it, not less than a quarter of a degree; this at 480 miles distance must have been at least two miles. It was certainly a grand appearance near the place where it fell, if any human eye was there.

May not these shooting stars be bodies altogether foreign to the earth and its atmosphere, accidentally meeting with it as they are swiftly traversing the great void of space? And may they not, either electrically or by some other means, excite a luminous appearance on entering our atmosphere? I am inclined to this opinion for the following reasons: 1st. It is not probable that meteors should be generated in the air at the height of 50 or 60 miles, on account of its extreme rareness; and many falling stars, besides this, are known with certainty to have been at very

 great

great heights. 2dly. Their motions cannot be owing to gravity, for they descend in all directions, and but seldom perpendicularly to the horizon. Besides, their velocities are much too great. This meteor would not have fallen by the force of gravity, from the place where it first appeared, to the earth, in less than two minutes of time; nor in less than ten seconds, if we suppose it impelled by gravity from the remotest distance. They are nevertheless affected by gravity in some manner, for I cannot find that any one was ever observed to ascend upwards in its course.

It is true that difficulties will likewise occur, if we suppose them to be foreign bodies of sufficient density to preserve such great degrees of velocity even in passing through the atmosphere, for it may be asked why do they not frequently strike the earth, buildings, &c.

Perhaps they are generally, if not always, exploded in passing through the air, something in the manner that filings of steel are exploded in passing through the flame of a candle. And at the same time that they afford us occasion to admire the variety and immensity of the Creator's works, they may perhaps produce some important and necessary effects in the atmosphere surrounding this globe, for the welfare of man and its other innumerable tribes of inhabitants.

I am, dear sir, your affectionate friend,

And very humble servant,

DAVID RITTENHOUSE.

Description

N° XV.

Deſcription of the Grotto at Swatara, by the Rev. PETER MILLER, *of Ephrata; communicated by* WILLIAM BARTON, *Eſquire.*

Read March 7, 1783.

"AS the courſe of my letter now tends this way, I muſt remind you, if ever you ſhould publiſh a natural hiſtory of Pennſylvania, not to conſign to oblivion that very curious petrifying cavern, of which, leſt you ſhould not have ſeen it already, I ſhall give ſome deſcription.

"It is ſituate on the eaſt ſide of Swatara, cloſe to the river. Its entrance is very ſpacious, and there is ſomewhat of a deſcent towards the other extremity; inſomuch that I ſuppoſe the ſurface of the river is rather higher than the bottom of the cave. The upper part is like an arched roof, of ſolid lime-ſtone rock, perhaps twenty feet thick. On entering, are found many apartments, ſome of them very high, like the choir of a church. There is, as it were, a continual rain within the cave, for the water drops inceſſantly from the roof upon the floor; by which, and the water petrifying as it falls, pillars are gradually formed to ſupport the roof. I ſaw this cave about thirty years ago, and obſerved above ten ſuch pillars, each ſix inches in diameter and ſix feet high; all ſo ranged that the place incloſed by them reſembled a ſanctuary in a Roman church: And I can aſſure you, that no royal throne ever exhibited more grandeur, than the delightful proſpect of this *luſus naturæ*. Satisfied with the view of this, we diſcovered the reſemblances of ſeveral monuments, incorporated into the walls, as if the bodies of departed heroes were there depoſited. Our guide then conducted us to a place, where, he he ſaid, hung the bell: This is a piece of ſtone iſſuing out of the roof, which when ſtruck ſounds like a bell.

"Some of the ſtalactites are of a colour like ſugar-candy, and others reſemble loaf-ſugar; but it is a pity that their beauty is now almoſt deſtroyed by the country people. The water, as it falls, runs down the declivity; and it is both wholeſome and pleaſant to drink, when it has diſcharged its petrifying matter. It is remarkable that we found ſeveral holes at the bottom of the cave, going down perpendicularly, perhaps into the abyſs, which renders it dangerous to be without a light. At the end of the cave, there is a pretty run, which takes its courſe through part of it, and then loſes itſelf among the rocks: Here is alſo its exit, by an aperture which is very narrow. Through this the vapours continually paſs outwards, with a ſtrong current of air; and, at night, theſe vapours aſcending reſemble a great furnace. Part of theſe vapours and fogs appear, on aſcending, to be condenſed at the head of this great alembic, and the more volatile parts to be carried off, through the aperture communicating with the exterior air before mentioned, by the force of the air in its paſſage.

"I beg pardon for having troubled you with ſuch a long detail. It appears ſtrange to me that none of our philoſophers have hitherto publiſhed a true account of this remarkable grotto."

N° XVI.

An Account of ſome Experiments on Magnetiſm, in a Letter to JOHN PAGE, *Eſquire, at Williamsburg.*

DEAR SIR,

Read Feb. 6, 1781.

AGREEABLE to the promiſe in my laſt, I ſhall now communicate to you ſome conjectures and experiments on magnetiſm, which may perhaps either

either afford you ſome amuſement, or induce you to purſue the ſubject to more certainty.

I ſuppoſe then, that magnetical particles of matter are a neceſſary conſtituent part of that metal which we call iron, though they are probably but a ſmall proportion of the whole maſs. Theſe magnetical particles I ſuppoſe have each a north and a ſouth pole, and that they retain their polarity, however the metal may be fuſed or otherwiſe wrought. In a piece of iron which ſhews no ſigns of magnetiſm theſe magnetical particles lie irregularly, with their poles pointing in all poſſible directions, they therefore mutually deſtroy each other's effects. By giving magnetiſm to a piece of iron we do nothing more than arrange theſe particles, and when this is done it depends on the temper and ſituation of the iron whether that arrangement ſhall continue, that is, whether the piece of metal ſhall remain for a long time magnetical or not.

There is ſome power, whenceſoever derived, diffuſed through every part of ſpace which we have acceſs to, which acts on theſe magnetical particles, impelling one of their poles in a certain direction with reſpect to the earth and the other pole in the oppoſite direction. The direction in which this power acts I take to be the ſame with that of the dipping needle.

By applying a magnet to a piece of iron it becomes magnetical; for the magnet acting ſtrongly on the above mentioned particles, that action arranges them properly; overcoming the reſiſtance of the ſurrounding parts of the iron, and this reſiſtance afterwards ſerves to ſecure them in their proper ſituations, and prevents their being deranged by any little accident.

If we place a piece of iron in or near the direction of the dipping needle, it will in time become magnetical; that general power producing in this caſe the ſame effect as the application of the magnet, though in a weaker degree.

Iron

Iron or soft steel receives magnetism more easily than hardened steel, but will not retain it; may not this be, because the magnetical particles are not so closely confined in soft as in hardened steel, and on that account more easily admit of arrangement or derangement. By making a piece of steel red hot, or by twisting it or beating it with a hammer, we may effectually destroy its magnetism. Now all these operations certainly derange the particles which compose the bar. By rubbing one piece of steel with another, magnetism may be produced, and it is easy to conceive how this operation, by the tremulous motion which it excites, may contribute to arrange the magnetical particles.

We took a soft steel ramrod, which did not discover the least sign of magnetism, and holding it in the direction of the dipping needle, struck it several smart blows with a hammer, on one end; then laying it on a watch chrystal it traversed very well; that end which was held downwards, when struck, becoming a north pole, whether the stroke was applied to the upper or the lower end. By turning the south end downwards and striking it afresh, the magnetism was destroyed or reversed, and it was curious to observe how very nicely you must adjust the number and force of the strokes, precisely to destroy the magnetism before communicated, without giving it anew, in a contrary direction. When we held the ramrod directly across the line of the dipping needle, whilst it was struck with a hammer, on many trials it did not discover any signs of magnetism. But when held in any other direction, that end which approached nearest to the point which the lower end of the dipping needle tends to, always became the north pole. From all this does it not seem very probable that during the concussion of the stroke, and whilst the magnetical particles of the rod were most disengaged from the surrounding matter, the active power abovementioned seized them and arranged them properly, where being confined,

confined, the rod afterwards remained magnetical. All this is neverthelefs little more than conjecture, until confirmed by further experiments.

I am, dear fir, yours, &c.

DAVID RITTENHOUSE.

N° XVII.

New Method of placing a Meridian Mark, in a Letter to the Rev. Dr. EWING, *Provoft of the Univerfity. By* D. RITTENHOUSE, *Efquire.*

DEAR SIR,

Read Nov. 1785. SOME time ago I mentioned to you a new invention I had for fixing a Meridian Mark for my Obfervatory. This I have fince executed, and as it anfwers perfectly well, I fhall give you a particular defcription of it.

When my obfervatory was firft erected, I placed a meridian mark to the northward at the diftance of about 1200 feet, my view to the fouth being too much confined by adjacent buildings, and that to the north was not diftant enough to have the mark free from a fenfible parallax. But laft fummer a new brick houfe was built directly north of the obfervatory, and much too nigh for diftant vifion with the tranfit inftrument. Now though a fixed mark is not abfolutely neceffary where you have a good tranfit inftrument, the pofition of which may be examined and accurately corrected, if neceffary, every fair day, by the paffage of the pole-ftar above and below the pole, it is neverthelefs very convenient, faves much trouble, and may fometimes prevent miftakes. We have an inftance in the obfervations of the Aftronomer Royal at Greenwich. His

 mark

mark being taken down at repairing the building to which it was fecured, the tranfit inftrument was accidentally thrown out of its true pofition, and the obfervations with it were continued for a confiderable time before the error was detected. My meridian mark being thus rendered ufelefs, I contrived feveral other methods of fupplying its place, all of which were, on fufficient deliberation, rejected for the following.

I faftened the object glafs of a thirty fix feet telefcope, firmly, to the wall which fupports the tranfit inftrument, oppofite to and as near as convenient to the object glafs of the tranfit, when brought to a horizontal fituation. In the focus of the thirty fix feet object glafs I fcrewed faft a piece of brafs to a block of marble, fupported by a brick pillar built on a good foundation, for this purpofe, in my garden. On this piece of brafs are feveral black concentric circles; the reft of the plate is filvered. The diverging rays of light which proceed from every point in thefe circles, after paffing through the thirty-fix feet glafs become parallel, and entering the tranfit inftrument, an image of the plate and its circles is formed in the fame place where the images of ftars or the moft diftant objects are formed. The circles are therefore diftinctly feen through the tranfit, and being placed in the fame meridian with the centre of the thirty fix feet glafs, the innermoft circle, about the fize of a brevier o, ferves for a meridian mark, to the centre whereof the crofs hair of the tranfit may be nicely adjufted.

This mark is in feveral refpects preferable to one placed in the common way. It is entirely free from parallax, which the other cannot be, unlefs placed at a very great diftance, when glaffes of great magnifying powers are ufed. It is not fenfibly affected by the undulation of the air, which very often renders it impoffible to fet the tranfit accurately to a diftant mark. And it can be illuminated at night without difficulty, fhould the fufpicion of any accident

accident to the tranſit make it neceſſary. But it has likewiſe one diſadvantage. Should the pillar in ſettling, carry the mark a little to the eaſt or weſt, the error will be greater in proportion to its nearneſs.

I am, dear ſir, your humble ſervant,

DAVID RITTENHOUSE.

P. S. The great improvement of object glaſſes by Dolland has enabled us to apply eye glaſſes of ſo ſhort a focus, that it is difficult to find any ſubſtance proper for the croſs hairs of fixed inſtruments. For ſome years paſt I have uſed a ſingle filament of ſilk, without knowing that the ſame was made uſe of by the European aſtronomers, as I have lately found it is by Mr. Hirſchell. But this ſubſtance, though far better than wires or hairs of any kind, is ſtill much too coarſe for ſome obſervations. A ſingle filament of ſilk will totally obſcure a ſmall ſtar, and that for ſeveral ſeconds of time, if the ſtar be near the pole. I have lately with no ſmall difficulty placed the thread of a ſpider in ſome of my inſtruments, it has a beautiful effect, it is not one tenth of the ſize of the thread of the ſilkworm, and is rounder and more evenly of a thickneſs. I have hitherto found no inconvenience from the uſe of it, and believe it will be laſting, it being more than four months ſince I firſt put it in my tranſit teleſcope, and it continues fully extended, and free from knots or particles of duſt.

N° XVIII.

Account of a Worm in a Horſe's Eye, by F. HOPKINSON, *Eſquire.*

Read Sep. 26, 1783.

HAVING been myſelf a witneſs to the following curious fact, I thought it ſhould

 not

not paſs unrecorded, eſpecially as it occurred in this city, under the immediate notice of the Philoſophical Society.

A report prevailed laſt ſummer that a horſe was to be ſeen which had a living ſerpent in one of his eyes. At firſt I diſregarded this report, but numbers of my acquaintance, who had been to ſee the horſe, confirming the account, I had the curioſity to go myſelf, taking a friend along with me. The horſe was kept in Arch-ſtreet and belonged to a free negroe. I examined the eye with all the attention in my power, being no ways diſpoſed to credit the common report, but rather expecting to detect a fraud or vulgar prejudice; I was much ſurpriſed, however, to ſee a real living worm within the ball of the horſe's eye. This worm was of a clear white colour, in ſize and appearance much like a piece of fine bobbin; it ſeemed to be from $2\frac{1}{2}$ to 3 inches in length, which however, could not be duly aſcertained, its whole length never appearing at one time, but only ſuch a portion as could be ſeen through the iris, which was greatly dilated. The creature was in a conſtant lively vermicular motion; ſometimes retiring ſo deep into the eye as to become totally inviſible, and at other times approaching ſo near to the iris as to become plainly and diſtinctly ſeen; at leaſt ſo much of it as was within the field of the iris. I could not diſtinguiſh its head, neither end being perfectly exhibited whilſt I viewed it, and indeed its motion was ſo briſk and conſtant, that ſo nice a ſcrutiny was not to be expected. The horſe's eye was exceedingly enflamed, ſwoln and running; I mean the muſcles contiguous to the eye ball, and ſeemed to give him great pain; ſo that it was with much difficulty the eye could be kept open for more than a few ſeconds at a time; and I was obliged to watch favourable moments for a diſtinct view of his tormentor. I believe the horſe was quite blind in that eye, for it appeared as if all the humours were confounded together, and that the worm had the whole orb to range in, which, however, was not of of a diameter ſufficient for the worm to extend its full length, as far as I could diſcover. The humours of the eye were beginning to grow opake like a chilled jelly, and became altogether ſo afterwards, as I was informed.

As this is a very uncommon circumſtance and may affect ſome philoſophical doctrines, it is much to be lamented that the horſe had not been purchaſed, and the eye diſſected for better examination. That there was a living, ſelf-moving worm within the ball of the horſe's eye, free from all deception or miſtake, I am moſt confident. How this worm got there, or if bred in ſo remarkable a place, where its parents came from, or how they contrived to depoſite their ſemen or convey their egg into the eye of an horſe, I leave for others to determine.

N° XIX.

An improved Method of Quilling a Harpſichord, by F. HOPKINSON, *Eſquire.*

Read Dec. 5, 1783.

MUCH of the pleaſure and effect in performing on a harpſichord depends on the equality of what is called *the touch;* and this is principally owing to a continuance of uniformity in the ſpring of the little quills, which by their impulſe ſet the ſtrings in vibration. Theſe quills, in the preſent manner of applying them, will not retain their elaſticity for any length of time, but require conſtant repair; which is one of the moſt troubleſome and difficult operations in keeping the inſtrument in order. To remedy this inconvenience, I have ſought for a ſubſtitute for the crow quill, and tried a variety of ſubſtances, but without ſucceſs. I then conſidered whether an improvement might not be made in the application of the quills themſelves, and to this purpoſe I examined

examined the cauſe of the quills being ſo liable to break, and obſerved that the piece of quill is thruſt through a ſmall hole in the tongue of the jack, projecting only about a quarter of an inch beyond the face of the tongue: That this quill is too ſhort to yield in all its parts, and ſo act properly as a ſpring; but bends only at the place where it iſſues from the hole in the tongue, and works up and down as upon a hinge, in that place; and *there only* is the quill ever known to break.

Thus in Plate III, Figure 6, *a*, is the tongue, *b*, the quill fixed firmly in it, which being too ſhort to act fairly as a ſpring, will bend only at *c*, when it is forced to paſs the ſtring; and by repeated exerciſe muſt neceſſarily break in that part, as any ſpring would do if compelled to act in the ſame manner.

But if this quill could be made longer, or applied ſo that its ſpring ſhould be part of a curve, it would probably preſerve its elaſticity for any length of time, as other ſprings do.

To effect this I have conſtructed the tongue and applied the quill as repreſented in figure 7, where *a*, is the tongue, the top of which is rounded off; the quill is firmly fixed in the hole at *c*, as uſual, but inſtead of paſſing through a length ſufficient to ſtrike the ſtring, it is cut off even with the face of the tongue at *f*. The quill thus fixed with its poliſhed face downwards, is bent upward round the top of the tongue, and then proceeds horizontally the proper length; being kept in the horizontal poſition by the little wire ſtaple *e*, being firmly driven into holes drilled for the purpoſe, but not ſo far as to pinch the quill againſt the top of the tongue; a little ſpace being left for the quill to play in.

From this conſtruction it is manifeſt, that the ſpring of the quill will be in its whole length, but chiefly in the curve *c*, *d*; and that a quill ſo applied will act fairly as a ſpring, and may be expected to retain its elaſticity for years,

years, ſubject to no variations but ſuch as may be occaſioned by alterations in the ſtate of the air, to which all known ſubſtances are more or leſs liable.

Read, 1784. IN the beginning of laſt winter, I had the honour to lay before the ſociety an improved method of quilling a HARPSICHORD.. Wiſhing to bring my diſcovery to the teſt of full experiment and to the judgment of abler critics, I forwarded a deſcription and a model of my improvement to a friend in London, requeſting that it might be ſubmitted to the examination of proper judges, and directing, in caſe it ſhould be approved of, that an inſtrument made by one of the firſt artiſts and quilled according to my propoſed method, ſhould be ſent to me. I have accordingly received an excellent double harpſichord, made by Meſſrs *Shudi* and *Broadwood* of London, and quilled according to my method; with this difference, I had rounded off the top of the tongue, and bending the quill over it, kept it in a horizontal poſition by means of a ſmall wire ſtaple; as will be more fully underſtood by referring to my former deſcription. But Mr. *Broadwood* has left the tongue of its full length and uſual form: But made the hole, in which the quill is commonly fixed tight, ſo large, that the quill has free room to play therein; and then fixing the quill below, has bent it round and brought it through this hole; which renders a ſtaple unneceſſary; the top of the tongue anſwering the ſame purpoſe. The principle on which the improvement depends is the ſame in both; but his is the beſt method of executing it.

He informs, however, that one inconvenience occurs viz. the quills being ſo forcibly bent in the curved part, are liable, in ſome inſtances, to ſpring back, and ſo become not only too ſhort to reach the ſtring it ſhould ſtrike, but the projection of the curve will be apt to touch the ſtring behind it, when the ſtop is puſhed back.

To

To explain this, let *a*, *b*, figure 8, represent the tongue, *c*, *d*, *e*, the quill, firmly fixed at *c*, then bent upwards and brought through a hole, which is large enough for the quill to play freely therein. But the curved part of the quill at *d*, being so forcibly bent, will in some instances spring back (as represented in the figure) not keeping close to the back of the tongue, as it should do: And as there is no waste room, the curve *d*, will be apt to touch the string behind it, when the stop is pushed back, I acknowledge that this inconvenience occurs in some few instances in the instrument Mr. *Broadwood* has sent me; but would observe that as it does not *always* happen, it is a fault in the execution and not in the principle. Yet, as it may be difficult to guard against it, I have considered how this evil may be effectually prevented.

Instead of punching the small hole, in which the quill is to be fixed, straight through the tongue, let it be punched slanting downwards; this will relieve the quill from that strained position which causes it to spring back. According to the first mode of application the curve formed by the quill will be as at *a*, figure 9, in the second as at *b*. I have constructed many tongues in this way, and found none of them liable to the inconvenience complained of, or shewing any tendency whatever to spring back; but to remove all jealousy on this head, should any remain, it will be easy to drive a small wire staple against the bottom of the curve behind, which must effectually retain it close to the back of the tongue.

I mention this expedient of the wire staple merely with a view of removing all doubt; but I do not think it necessary; the objection being perfectly remedied by the other method: To prove this, I have cut out the entire block between the two holes, in the manner of a mortise, and drove a pin across the upper part of it. I then caused the quill to lie in this slanting mortise, and bending it round brought it over the pin; and I found it would remain

main perfectly at eafe in its birth, although not pinched or reftrained in any part; *a*, figure 10, reprefents the tongue in front, and *b*, the mortife, of which the flanting fhape cannot be feen in this view; but will be better underftood by obferving the pofition of the quill in figure 11, where *a* is a profile of the fame tongue, *b*, *c*, the quill lying in the mortife, and *d*, the pin over which the top of the quill paffes.

I have need to apologize to the fociety for directing fo much of their attention, to an object which may appear to fome to be of little importance. To the mufical tribe, however, this improvement will prefent itfelf in a different light. Many perfons who play very well on the harpfichord, are not able to keep the inftrument in order: And to fend for a perfon to repair the quills and tune the inftrument as often as it fhall be neceffary, is not only troublefome and expenfive, but fuch affiftance is not always to be had, efpecially in the country. And for thefe reafons many a good harpfichord or fpinnet lies neglected and the fcholar loofes the opportunity of practice. To fuch perfons a method of quilling that fhall feldom want repair is a *difideratum* of no fmall importance. And this, I flatter myfelf I have accomplifhed.

The difficulty of *quilling* being thus removed, I confidered in what manner *tuning* might be made eafy to the practitioner in mufic. Harpfichords are tuned by means of *fifths* and *thirds*; but fuch is the mufical divifion of the monochord as to make it neceffary, that none of thefe *fifths* or *thirds* fhould be perfect; an allowance muft be made; and to do this with judgment, fo that the chords may be good and the inftrument be in tune, requires much attention and practice. Of the numbers that play, there will not be found one in an hundred that can tune a harpfichord. To render this tafk eafy, I have procured *twelve tuning forks*, for the *twelve femitones* of the octave; thefe I had perfectly tuned; and as they will not be fenfibly af-

 fected

fected by any change of weather, they remain as ſtandards. I take it for granted that any perſon at all accuſtomed to muſical ſounds can tell when one tone is *in uniſon* with another; and that a very little practice will enable him to tune one ſound *an octave* to another, theſe conchords are ſo manifeſt that they cannot eaſily be miſtaken. There is then nothing to be done but to tune the twelve ſtrings in uniſon with the twelve forks; this will fix the ſcale, or temperature for one octave, which is the whole difficulty; the reſt of the inſtrument is eaſily tuned by uniſons and octaves to the ſcale, ſo aſcertained*.

Having, I hope, fully accompliſhed the deſign I had in view when I turned my thoughts to this ſubject, I ſhall now take leave of it; and ſhall be highly gratified if I find others benefited by my attentions, although in a matter of no very ſerious import.

Nov. 1784.

Deſcription of a further Improvement in the HARPSICHORD.

Read January 28, 1786.

IN a former paper read before the ſociety, reſpecting an improved method of quilling a HARPSICHORD, I made ſome apology for troubling you with a ſubject not ſtrictly within the limits of your view as a philoſophical ſociety, and which might appear to ſome of ſmall importance. At the ſame time I took formal leave of a purſuit which had accidentally engaged my attention, and which I had obtruded upon your's. Notwithſtanding this, I find myſelf under a neceſſity of again requeſting your indulgence, whilſt I deſcribe a diſcovery I made in Auguſt laſt, of a ſtill further improvement to the ſame purpoſe.

Having

* My ſet of forks are tuned from the middle C ſharp to the C above, incluſive.

Having ſucceeded to the extent of my expectation in a more advantageous way of applying the crow quill in common uſe in a harpſichord, I thought to reſt content with that improvement; which had principally for its object the duration of the quill's elaſticity, and of courſe the duration of the equality of touch. But notwithſtanding the long eſtabliſhed prejudice in favour of the crow quill, and the prevailing opinion that no ſubſtance can ſupply its place to advantage, I think a candid critic will allow that one of the following poſitions is founded in fact, and the other in reaſon.

Firſt. Although the three ſtops of a harpſichord ſhould be quilled to the beſt advantage, the reſult of the whole will be an obſervable jingle or tinkling between the quills and wires, which depreciates the dignity and ſweetneſs of the inſtrument. The beſt harpſichords are ſo cenſurable for this imperfection, that the *Forte Piano*, which is free from it, ſtands a chance of rivalling that noble inſtrument, for this cauſe only; being far inferior in every other reſpect.

Second. Is it not reaſonable to ſuppoſe that ſo long a ſtring, ſo advantageouſly ſtretched over ſo large a box, ſhould yield a greater body of tone, than that which is produced by the impulſe of a quill? If the quill be made very ſtiff, this will render the touch diſagreeable and encreaſe the jingle, but not add to the *body* of tone. One reaſon why the quill does not draw a fuller tone from the ſtring, I ſuppoſe to be the ſmallneſs of its contact. The back of a quill is a portion of a circle, the extended ſtring is a right line, and a circle can touch a right line only in a point; the contact therefore muſt be ſo very ſmall, that mere ſtrength of impulſe is not ſufficient to put the ſtring into full vibration.

The method I am now to deſcribe of quilling, or rather *tonguing* a harpſichord, I have found by experiment, to draw forth the powers of the inſtrument to a ſurpriſing effect, caufing it to yield a full and pure body of tone, free from all jingle and very pleafant to the ear.

N. B. What hath hitherto been called the tongue of the jack, I fhall denominate the *palate;* and the fubftitute I have made for the quill, I fhall call the *tongue.* The propriety of this will appear in the defcription.

Plate III. Let A, figure 12, reprefent the palate in front, with a mortife cut through it for the tongue to work in. B, is the tongue, having two fmall holes drilled through it, one in the centre of its motion and the other at a little diftance behind, for the reception of one end of a wire fpring hereafter mentioned.

Figure 13, is the palate in profile, with the tongue properly mounted and moveable on the centre pin. This figure alfo fhews how the palate muft be hollowed in behind to expofe the root of the tongue, and the fmall hole in it for the reception of one end of the wire fpring.

Figure 14, is a back view of the palate, fhewing the groove in which the hair fpring of the jack lies, and a fmall wire ftaple at *b*, to which the lower end of the fteel fpring is to be faftened.

Figure 15, is the fpring which is to govern the tongue. It muft be of fine fteel wire, fomewhat annealed by being forcibly rubbed between pieces of leather or cork, and is formed by winding the wire backwards and forwards with a tight hand, over pins driven deep and firm into a piece of wood. As the palate muft play freely within the fork or jaws of the jack, the windings of the fpring muft not exceed the width of the palate. The upper end of the fpring being run through the fmall hole in the root of the tongue and bent round, fo as to fecure it, and the fuperfluous part cut off; the lower end of the fame fpring muft be run under the little ftaple (*b*, figure 14,) and bent upwards with a gentle ftrain, fo as to hook it on and fecure it to that ftaple; the fpring will then operate with all its elafticity, and the tongue will be fubjected to its operation.

Figure

Figure 17, reprefents the palate in a back view with the zig zag fpring faftened by one end to the root of the tongue, and by the other to the little ftaple.

To prevent the tongue from rifing by the force of the fpring above a horizontal pofition, there muft be a wire ftaple driven in the front of the palate immediately above the tongue (as at *a*, in figure 12 and 13;) and the tongue, if of wood, fhould be armed with a fmall piece of foft leather juft under the ftaple, to prevent noife.

It muft be left to future experiment to determine the moft proper of all fubftances of which the tongue fhould be made; different fubftances drawing different tones from the ftring. After many effays to this purpofe, I have concluded to furnifh my harpfichord in the following manner.

The tongues of the firft unifon are of *Ben fole-leather*. Thofe of the fecond are of a foft leather faced with Morocco, fuch as is frequently ufed in harpfichords, though applied in a different way, and the tongues of the octave are of wood, fuch as pear tree, laurel, or any wood of an even grain and not too hard in fubftance. But all mounted on fprings, as above defcribed, and their faces well polifhed with black lead where they come in contact with the ftrings.

My reafons are. The fole-leather produces a full, fweet and vigorous tone from the firft unifon. The fecond unifon, which is the piano of the inftrument when the pedal is preffed, is furnifhed with Morocco leather, which draws a full but more foft and fmothered tone from the ftring. And the octave is ftruck with wooden tongues for the fake of vivacity or brilliancy, which is the genius of that ftop; yet I am not fure but that the octave alfo had better be ftruck with fole-leather, like the firft unifon*.

A harpfichord thus furnifhed, will produce a body or quantity of found, and a purity of tone, that will aftonifh at

* Becaufe, after the ftroke has been given, the wooden tongue repaffing the ftring, yet in vibration, makes a jingle, which the leather tongues do not.

at the firſt hearing, much reſembling the diapaſon ſtop of an organ. And it is manifeſt that if the touch be well regulated at firſt, it will not afterwards be ſubject to alteration for a long courſe of time. The touch is in part regulated by the ſtrength of the ſerpentine ſpring and the number of its zig zag evolutions; and in part by the manner of rounding off the tip of the tongue; for the tip of the tongue muſt not be cut off ſquare, (in which caſe, the ſtring would leave the tongue too abruptly and cauſe a diſagreeable twang,) but ſhould be ſlanted off from underneath, and its extreme point rounded and well poliſhed by rubbing it very hard with a piece of black lead. As to the ſtrength of the ſpring, four ſizes of wire, viz. from n°. 4 to n°. 8, will be ſufficient for the whole inſtrument; but the touch is more immediately regulated by rounding off the tips of the tongues by the preſſure and poliſh of the black lead, more or leſs, as occaſion ſhall require. When the tongues are of wood, a ſtroke or two of a fine file will be neceſſary to take off the ſquare edge left by the knife, previous to the poliſhing it with the black lead.

After all, a harpſichord juſt furniſhed in this way, will not be ſo pleaſant to the touch or to the ear as it will be after a few weeks uſe; when the ſtrings will, by repeated friction, have rounded off and poliſhed the tips of the tongues, and have made for themſelves a broad bearing or contact, which cannot perhaps be ſo accurately produced by any care of the workman.

Laſtly, it is ſcarce neceſſary to obſerve that the ſerpentine ſpring and the root of the tongue muſt be compriſed within the thickneſs of the jack; otherwiſe they will be apt to interfere with the ſtring behind, when the ſtop is puſhed back.

F. HOPKINSON.

Obſervations

N° XX.

Obfervations on a Comet lately difcovered; communicated by DAVID RITTENHOUSE, *Efquire.*

Read Mar. 19, 1784.

ON the 21ft of January laft, John Lukens, Efquire, informed me that he had difcovered a comet the preceding evening, and on the evening of the fame day, affifted by Mr. Lukens and Mr. Prior, I obferved the apparent place of the comet to be in the 15th degree of Pifces, with 16° 6′ fouth latitude. By fubfequent obfervations I found its motion to be north eafterly, with refpect to the ecliptic, and that its neareft approach to us had preceded our firft obfervation. It paffed the ecliptic on the 31ft in the 25° of Pifces, and February the 17th it was in Pifces 29° with 13° 10′ north latitude. This was the laft time I faw it, clouds and moonlight having fince prevented.

The light of this comet was fo very faint that it was impoffible to obferve it with accuracy, at leaft without better inftruments than I am poffeffed of, efpecially as the comet was always involved in day light, moonlight or the thick atmofphere of the horizon. No pains or attention however were wanting, and from the beft obfervations I could make, I find it paffed its perihelion about the 20th of January, its diftance from the fun being about $\frac{7}{10}$ of the fun's diftance from us. The place of its afcending node is in the 25th deg. of Taurus, and the inclination of its orbit 53°. Its motion is retrograde, that is, contrary to the order of the figns. I have ftill hopes of feeing it in the morning, though its diftance is now fo very great that it can fcarcely be vifible to the naked eye.

Extract

N° XXI.

Extract of a Letter from the Rev. JEREMY BELKNAP, *containing Obſervations on the Aurora Borealis.*

Dover, New-Hampſhire, March 31ſt, 1783.

Read May 2, 1783.

"DID you ever, in obſerving the Aurora Borealis, perceive a *ſound?* I own I once looked on the idea as frivolous and chimerical, having heard it at firſt from perſons whoſe credulity, I ſuppoſed, exceeded their judgment; but, upon hearing it repeatedly, and from ſome others whom I thought judicious and curious, I began to entertain an opinion in favour of it. I was ſtrengthened in this opinion about two years ago, by liſtening with attention to the flaſhing of a luminous arch which appeared in a calm froſty night, when I thought I heard a faint ruſtling noiſe like the bruſhing of ſilk. Laſt Saturday evening I had full auricular demonſtration of the reality of this phenomenon. About ten o'clock the hemiſphere was all in a glow; the vapours aſcended from all points, and met in a central one in the zenith: All the difference between the ſouth and north part of the heavens was, that the vapour did not begin to aſcend ſo near the horizon in the ſouth as in the north. There had been a ſmall ſhower with a few thunder claps, and a bright rainbow in the afternoon; and there was a gentle weſtern breeze in the evening which came in flaws, with intervals of two or three minutes; in theſe intervals I could plainly perceive the ruſtling noiſe, which was eaſily diſtinguiſhable from the ſound of the wind, and could not be heard till the flaw had ſubſided. The flaſhing of the vapour was extremely quick; whether accelerated by the wind I cannot

cannot ſay; but from that quarter where the greateſt quantity of the vapour ſeemed to be in motion, the ſound was plaineſt; and this, during my obſervation, was the eaſtern. The ſcene laſted about half an hour, though the whole night was as light as when the moon is in the quarters."

N° XXII.

A Letter from J. Madison, *Eſq. to* D. Rittenhouse, *Eſq. containing Experiments and Obſervations upon what are commonly called the Sweet Springs.*

THESE waters riſe on the north ſide of a large mountain at the foot of it, called the Sweet Spring Mountain, in the county of Botetourt. The ſouth ſide is covered with ſtones of an ocrous appearance. In many places iron ore may be found; but on the north the mountain is fertile, covered with a rich mould, at leaſt near the ſpring. The remarkable efficacy of theſe waters in many diſorders, eſpecially, it is ſaid, in conſumptive complaints, firſt induced me to attempt their analyſis. Such experiments as I had time and opportunity to make, I ſhall faithfully relate, and leave it to others, better qualified than myſelf, to judge of their merits.

Experiment 1. Having plunged a very ſenſible mercurial thermometer in the ſpring, it ſtood at 73°. The temperature of air was about 69.

2. A good hydrometer ſunk one-twentieth of an inch deeper in common mountain water, than in the ſpring.

3. Nut-galls mixed with the water in a wine glaſs ſtruck a paliſh brown, which ſhewed that there was little or no iron in it.

4. Violets mixed with the water in a wine glaſs, turned

ed it in a ſhort time of a reddiſh colour. This was a proof that the waters contained ſome kind of acid.

5. Having made a ſolution of ſilver in the nitrous acid, and mixed a little of it with the water, it immediately became milky, and a white pulvurent precipitate enſued. This experiment ſhewed by the whiteneſs of the precipitate, that the waters contained nothing ſulphureous, and by the pulvurency of the precipitate that the acid contained in the waters was vitriolic.

6. A ſolution of lead in the nitrous acid being mixed with the water, it became ſomewhat milky, and a white precipitate was obſerved. This experiment alſo ſhews that the waters contain an acid, moſt probably the vitriolic, and alſo that they contain calcareous earth. Soap is not readily miſcible with them.

7. A ſolution of ſaccharum ſaturni in the nitrous acid being made, and lines marked upon paper with it, and placed over the water, the lines retained their former colour. This experiment alſo ſhews that the water contains nothing ſulphureous.

8. Having poured a little of the ſpirit of ſalt into the water, after ſome time a coloured precipitate was obſerved, but as the waters did not ſtrike a green or blue colour, it ſhewed that there was no copper in them.

9. A ſolution of vitriol of copper mixed with the water produced a thick, green, curdly appearance, but did not become bluer. This experiment ſhewed that there was no vol. alkali contained in them.

10. The vitriolic acid mixed with the water ſuddenly efferveſced, and produced a heat which raiſed the thermometer from 75 to 83, by applying the bulb to the outſide of the glaſs.

11. As the ſpring is continually diſcharging large bubbles of air, which riſing from the bottom break upon the ſurface of the water, I was deſirous of making ſome experiments upon the air, in order to determine whether the

the acidity of the water might not be owing to it; and alſo to determine the nature of the air, whether fixed or not. Having therefore caught a quantity of the air in a decanter, I communicated a part of it to an equal bulk of pure mountain water, and after agitating them for ſome time, gave it to ſeveral to taſte; who agreed that it had the taſte of the ſpring water. Upon a ſecond trial this experiment did not ſucceed. I had not an opportunity of trying the nature of the air by means of chalk-water, and was prevented from proſecuting any farther enquiries into the nature of theſe celebrated waters by a ſudden alarm, to which the frontiers were then continually expoſed.

Theſe waters have been falſely called *ſweet*, for their taſte is evidently acidulous. The experiments alſo ſhew that they contain an acid. Their taſte reſembles exactly that of waters artificially impregnated with fixed air, extricated from chalk, by means of the vitriolic acid, and I conceive muſt be nearly the ſame with the true Pyrmont water. They have little or no ſmell, do not form an incruſtation, nor do they leave a depoſit upon ſtanding many hours. Upon bathing in the morning, the ſkin has a ſoapy kind of feel. This was not obſerved in the evening.

There is near this ſpring another, a very ſtrong chalybeate.

I am, with great regard, yours,

J. MADISON.

N° XXIII.

A Letter from the Rev. JEREMY BELKNAP, *on the preſerving of Parſnips by drying.*

Dover, New-Hampſhire, March 5, 1784.

SIR,

Read Apr. 16, 1784.

AMONG the number of eſculent roots, the *parſnip* has two ſingular good qualities.

 One

One is that it will endure the ſevereſt froſt and may be taken out of the ground in the ſpring, as freſh and ſweet as in autum; the other is that it may be preſerved by drying to any deſired length of time.

The firſt of theſe advantages has been known for many years paſt; the people in the moſt northerly parts of New-England where winter reigns with great ſeverity, and the ground is often frozen to the depth of two or three feet for four months, leave their parſnips in the ground till it thaws in the ſpring, and think them much better preſerved than in cellars.

The other advantage never occurred to me till this winter, when one of my neighbours put into my hands a ſubſtance which had the appearance of a piece of buck's horn. This was part of a parſnip which had been drawn out of the ground laſt April and had lain neglected in a dry cloſet for ten months. It was ſo hard as to require conſiderable ſtrength to force a knife through it croſs-wiſe; but being ſoaked in warm water, for about an hour, became tender, and was as ſweet to the taſte as if it had been freſh drawn from the ground.

As many uſeful diſcoveries owe their origin to accident, this may ſuggeſt a method of preſerving ſo pleaſant and wholeſome a vegetable for the uſe of ſeamen in long voyages, to prevent the ſcurvey and other diſorders incident to a ſea-faring life, which is often rendered tedious and diſtreſſing for want of vegetable food; ſince I am perſuaded that parſnips dried to ſuch a degree, as above related, and packed in tight caſks, may be tranſported round the globe, without any loſs of their flavour or diminution of their nutritive quality.

I am ſir, your humble ſervant,

JEREMY BELKNAP.

An

N° XXIV.

An Optical Problem, propofed by Mr. Hopkinson, *and folved by* Mr. Rittenhouse.

Philadelphia, March 16th, 1785.

DEAR SIR,

Read Feb. 17, 1786.

I TAKE the liberty of requefting your attention to the following problem in *optics*. It is I believe entirely new, and the folution will afford amufement to you and inftruction to me.

Setting at my door one evening laft fummer, I took a filk handkerchief out of my pocket, and ftretching a portion of it tight between my two hands, I held it up before my face and viewed, through the handkerchief, one of the ftreet lamps which was about one hundred yards diftant; expecting to fee the threads of the handkerchief much magnified. Agreeably to my expectation I obferved the filk threads magnified to the fize of very coarfe wires; but was much furprifed to find that, although I moved the handkerchief to the right and left before my eyes, the dark bars did not feem to move at all, but remained permanent before the eye. If the dark bars were occafioned by the interpofition of the magnified threads between the eye and the flame of the lamp, I fhould have fuppofed that they would move and fucceed each other, as the threads were made to move and pafs in fucceffion before the eye; but the fact was otherwife.

To account for this phenomenon exceeds my fkill in *optics*. You will be fo good as to try the experiment, and if you find the cafe truly ftated, as I doubt not you will, I fhall be much obliged by a folution on philofophical principles. I am fir, with great fincerity,

Your moft effectionate friend,

And very humble fervant,

F. HOPKINSON.

The Anſwer, by Mr. RITTENHOUSE.

DEAR SIR,

THE experiment you mention, with a ſilk handkerchief and the diſtant flame of a lamp, is much more curious than one would at firſt imagine. For the object we ſee is not the web of the handkerchief magnified, but ſomething very different, as appears from the following conſiderations. 1ſt. A diſtinct image of any object, placed cloſe to the eye, cannot be formed by parallel rays, or ſuch as iſſue from a diſtant luminous point: for all ſuch rays, paſſing through the pupil, will be collected at the bottom of the eye, and there form an image of the luminous point. The threads of the handkerchief would only intercept part of the rays, and render the image leſs brilliant. 2dly. If the croſs bars we ſee were images of the ſilk threads, they muſt paſs over the retina, whilſt the threads are made to paſs over the pupil; but this, as you obſerve, does not happen; for they continue ſtationary. 3dly. If the image on the retina was a picture of the object before the eye, it muſt be fine or coarſe, according to the texture of the handkerchief. But it does not change with changing the ſilk, nor does it change on removing it farther from the eye. And the number of apparent threads remains the ſame, whether 10, 20, or 30 of the ſilk threads paſs acroſs the pupil at the ſame time. The image we ſee muſt therefore be formed in ſome different manner; and this can be no other than by means of the *inflection* of light in paſſing near the ſurfaces of bodies, as deſcribed by NEWTON.

It is well known in optics that different images of the different points of objects without the eye are formed on the retina by pencils of rays, which, before they fall on the eye, are inclined to each other in ſenſible angles. And the great uſe of teleſcopes is to encreaſe theſe angles, regularly, in a certain ratio; ſuffering ſuch rays as were parallel

parallel before they enter the telefcope to proceed on, parallel, after paffing through it. The extended image which we fee in this experiment muft therefore be formed by pencils of rays, which before they entered the eye, had very confiderable degrees of inclination with refpect to each other. But coming from a fmall diftant flame of a lamp, they were nearly parallel before they paffed through the filk handkerchief. It was therefore the threads of filk which gave them fuch different directions.

Before the filk is placed to the eye, parallel rays of light will form a fingle lucid fpot, as at A, Plate III. Figure 16. And this fpot will ftill be formed afterwards by fuch rays as pafs through the little mefhes uninfluenced by the threads. But fuppofe the perpendicular threads by their action on the rays, to bend a part of them one degree to the right and left, another part two degrees; there will now be four new images formed, two on each fide of the original one at A. By a fimilar action of the horizontal threads, this line of five lucid points will be divided into five other lines, two above and two below, making a fquare of twenty-five bright fpots, feparated by four perpendicular dark lines and four horizontal ones; and thefe lucid fpots and dark lines will not change their places on moving the web of filk over the eye parallel to any of its threads. For the point of the retina on which the image fhall fall is determined by the incidence of the rays, with refpect to the axis of the eye, before they enter, and not by the part of the pupil through which they pafs.

In order to make my experiments with more accuracy, I made a fquare of parallel hairs about half an inch each way. And to have them nearly parallel and equidiftant, I got a watchmaker to cut a very fine fcrew on two pieces of fmall brafs wire. In the threads of thefe fcrews, 106 of which made one inch, the hairs were laid 50 or 60 in number. Looking through thefe hairs at a fmall opening in the window fhutter of a dark room, $\frac{1}{30}$ of an inch wide and

and three inches long, holding the hairs parallel to the ſlit, and looking toward the ſky, I ſaw three parallel lines, almoſt equal in brightneſs, and on each ſide four or five others much fainter and growing more faint, coloured and indiſtinct, the farther they were from the middle line, which I knew to be formed by ſuch rays as paſs between the hairs uninfluenced by them. Thinking my apparatus not ſo perfect as it might be, I took out the hairs and put in others, ſomething thicker, of theſe 190 made one inch, and therefore the ſpaces between them were about the $\frac{1}{250}$ part of an inch. The three middle lines of light were now not ſo bright as they had been before, but the others were ſtronger and more diſtinct, and I could count ſix on each ſide of the middle line, ſeeming to be equally diſtant from each other, eſtimating the diſtance from the centre of one to the centre of the next. The middle line was ſtill well defined and colourleſs, the next two were likewiſe pretty well defined, but ſomething broader, having their inner edges tinged with blue and their outer edges with red. The others were more indiſtinct, and conſiſted each of the priſmatic colours, in the ſame order, which by ſpreading more and more, ſeemed to touch each other at the fifth or ſixth line, but thoſe neareſt the middle were ſeparated from each other by very dark lines, much broader than the bright lines.

Finding the beam of light which came through the window ſhutter divided into ſo many diſtinct pencils, I was deſirous of knowing the angles which they made with each other. For this purpoſe I made uſe of a ſmall priſmatic teleſcope and micrometer, with which I was favoured by Dr. Franklin. I faſtened the frame of parallel hairs before the object glaſs, ſo as to cover its aperture entirely. Then looking through the teleſcope, I meaſured the ſpace between the two firſt ſide lines, and found the angular diſtance between their inner edges to be $13', 15''$; from the middle of one to the middle of the other $15', 30''$, and

from

from the outer edge of one, to the outer edge of the other 17′, 45″. In the firſt caſe I had a fine blue ſtreak in the middle of the object, and in the laſt a red ſtreak. The other lines were too faint, when ſeen through the teleſcope, to meaſure the angles they ſubtended with accuracy, but from ſuch trials as I made I am ſatisfied that from the ſecond line on one ſide to the ſecond on the other ſide, and ſo on, they were double, triple, quadruple, &c. of the firſt angles.

It appears then that a very conſiderable portion of the beam of light paſſed between the hairs, without being at all bent out of its firſt courſe; that another ſmaller portion was bent at a medium about 7′, 45″ each way; the red rays a little more, and the blue rays a little leſs; another ſtill ſmaller portion 15′, 30″; another 23′, 15″, and ſo on. But that no light, or next to none, was bent in any angle leſs than 6′, nor any light of any particular colour, in any intermediate angle between thoſe which ariſe from doubling, tripling, &c. of the angle in which it is bent in the firſt ſide lines.

I was ſurprized to find that the red rays are more bent out of their firſt direction, and the blue rays leſs; as if the hairs acted with more force on the red than on the blue rays, contrary to what happens by refraction, when light paſſes obliquely through the common ſurface of two different mediums. It is, however, conſonant to what Sir Iſaac Newton obſerves with reſpect to the fringes that border the ſhadows of hairs and other bodies; his words are, "And therefore the hair in cauſing theſe fringes, "acted alike upon the red light or leaſt refrangible rays "at a greater diſtance, and upon the violet or moſt re- "frangible rays at a leſs diſtance, and by thoſe actions "diſpoſed the red light into larger fringes, and the violet "into ſmaller fringes."

By purſuing theſe experiments it is probable that new and intereſting diſcoveries may be made, reſpecting the

 properties

properties of this wonderful ſubſtance, light, which animates all nature in the eyes of man, and perhaps above all things diſpoſes him to acknowledge the Creator's bounty. But want of leiſure obliges me to quit the ſubject for the preſent.

I am, dear ſir, your affectionate friend,
And very humble ſervant,
DAVID RITTENHOUSE.

N° XXV.

An Enquiry into the Cauſe of the Increaſe of Bilious and Intermitting Fevers in Pennſylvania, with Hints for preventing them. By BENJAMIN RUSH, M. D. *Profeſſor of Chemiſtry in the Univerſity of Pennſylvania.*

Read December 16, 1785.

IT has been remarked, that Pennſylvania for ſome years paſt has become more ſickly than formerly. Fevers which a few years ago appeared chiefly on the banks of creeks and rivers, and in the neighbourhood of mill-ponds, now appear in parts remote from them all, and in the higheſt ſituations. This change with reſpect to the healthineſs of our country, may be traced to the three following cauſes.

1. The eſtabliſhment and increaſe of mill-ponds. There are whole counties in Pennſylvania in which intermittents were unknown, until the waters in them were dammed, for the purpoſe of erecting mill-ponds.

2. The cutting down of wood, under certain circumſtances, tends to render a country ſickly. It has been remarked that intermittents on the ſhores of the Suſquehannah have kept an exact pace with the paſſages which have been opened for the propagation of marſh effluvia, by

cutting

cutting down the wood which formerly grew in its neighbourhood. I remember the time, when intermittents were known only within half a mile, in ſome places, of that river. They are now to be met with ten miles from it in the ſame parts of the ſtate.

I beg a diſtinction to be made here between *clearing* and *cultivating* a country. While clearing a country makes it ſickly in the manner that has been mentioned, *cultivating* a country, that is, draining ſwamps, deſtroying weeds, burning bruſh, and exhaling the unwholſome or ſuperfluous moiſture of the earth, by means of frequent crops of grain, graſſes, and vegetables of all kinds, render it healthy. I could mention, in ſupport of theſe facts, ſeveral countries in the United States, which have paſſed through each of the ſtages that have been deſcribed. The firſt ſettlers received theſe countries from the hands of nature pure and healthy*. Fevers ſoon followed their improvements, nor were they finally baniſhed, until the higher degrees of cultivation that have been named took place. I confine myſelf to thoſe countries only where the ſalutary effects of cultivation were not rendered abortive by the neighbourhood of mill-ponds.

A 3d cauſe of the late increaſe of bilious and intermitting fevers, muſt be ſought for in the different and unequal quantities of rain which have fallen within theſe laſt ſeven years. While our creeks and rivers, from the uniformity of our ſeaſons, were confined to ſteady bounds, there was little or no exhalation of febrile miaſmata from their ſhores. But the dry ſummers of 1780, 1781, and 1782, by reducing our creeks and rivers far below their ancient marks; while the wet ſprings of 1784 and 1785, by ſwelling them both beyond their natural heights, have, when they have fallen, as in the former caſe, left a large

 and

* A phyſician who travelled through part of Bedford county, in Pennſylvania, in the year 1782, informed me that he was witneſs of ſome country people having travelled twenty miles, to ſee whether it was poſſible for a German girl who laboured under an intermittent, to be HOT and COLD at the ſame time.

and extenſive ſurface of moiſt ground expoſed to the action of the ſun, and of courſe to the generation and exhalation of febrile miaſmata. The hiſtory of epidemics in foreign countries, favours this opinion of the cauſe of their increaſe in Pennſylvania. The inhabitants of Egypt are always healthy during the overflowing of the Nile. Their fevers appear only after the receſs of the river. It is remarkable that a wet ſeaſon is often healthy in low, while it is ſickly in hilly countries. The reaſon is obvious. In the former the rains entirely cover all the moiſt grounds, while in the latter, they fall only in a ſufficient quantity to produce thoſe degrees of moiſture which favour febrile exhalations. The rains which fall in the ſummer are rendered harmleſs only by covering the *whole* ſurface of marſhy ground. The rains which fall in our ſtate after the middle of September, are ſo far from producing fevers, that they generally prevent them. The extraordinary healthineſs of the laſt autumn, I believe was occaſioned by nothing but the extraordinary quantity of rain that fell during the autumnal months. The rain probably acts at this ſeaſon by diluting, and thus deſtroying, the febrile miaſmata that were produced by the heat and moiſture of the preceding ſummer. In ſupport of the truth of this third cauſe of the increaſe of fevers in Pennſylvania, I have only to add a fact lately communicated to me by Dr. Franklin. He informed me that in his journey from Paſſy to Havre de Grace, laſt ſummer, he found the country through which he travelled, unuſually ſickly with fevers. Theſe fevers it was generally ſuppoſed, were produced by the extraordinary dry weather, of which the public papers have given us ſuch melancholy and frequent accounts.

I come now to ſuggeſt a few hints for obviating and preventing fevers, and for rendering our country again healthy. For this purpoſe I beg leave to recommend in the firſt place, the planting of trees around all our millponds, (beſides cleaning them occaſionally) in order to prevent

prevent the diſeaſes that have juſtly been aſcribed to them. Let the trees be planted in the greateſt number, and cloſeſt together, to leeward of the ordinary current of the ſummer and autumnal winds. I have known ſeveral inſtances of families being preſerved from fevers by an accidental copſe of wood ſtanding between a mill-pond and a dwelling houſe, and that in caſes too where the houſe derived no advantage from an high ſituation. The trees *around* or *near* a mill-pond, act perhaps in a ſmall degree *mechanically*. By ſheltering the pond from the action of the ſun, they leſſen exhalation, as well as obſtruct the paſſage of the vapors that are raiſed to the adjacent parts. But they act likewiſe *chemically*. It has been demonſtrated that trees abſorb unhealthy air, and diſcharge it in a highly purified ſtate in the form of what is now called "de-"flogiſticated" air. The willow tree, according to Mr. Ingenhauſz, has been found to purify air the moſt rapidly of any tree that he ſubjected to his experiments. The rapidity of its growth, its early verdure, and the late fall of its leaf, all ſeem to mark it likewiſe as a tree highly proper for this purpoſe.

A ſecond method of preventing fevers, is to let the cultivation always keep pace with the *clearing* of our lands. Nature has in this inſtance connected our duty, intereſt and health together. Let every ſpot covered with moiſture from which the wood has been cut, be carefully drained, and afterwards ploughed and ſowed with graſs ſeed; let weeds of all kinds be deſtroyed, and let the waters be ſo directed as to prevent their ſtagnating in any part of their courſe.

Theſe are the two principal means of extirpating intermitting and bilious fevers from our country, but as theſe means are ſlow in their operation, I ſhall ſubjoin a few directions for preventing fevers till the above remedies can take effect.

1. Whether

1. Whether the matter which produces fevers be of an organic, or inorganic nature, I do not pretend to determine, but it is certain, that *fire* or the *ſmoke* or *heat*, which iſſue from it, deſtroy the effects of marſh miaſmata upon the human body; hence we find cities more healthy than country places, and the centre of cities more healthy than their ſuburbs in the ſickly months. To derive the utmoſt poſſible benefit from this method of preventing ſickneſs, I would adviſe large fires to be made every evening of bruſh between the ſpots from whence the exhalations are derived, and the dwelling houſe, and as near to the latter as is ſafe, and not diſagreeable. This practice ſhould be continued till the appearance of two or three froſts, for froſts as well as heavy rains in the autumnal months never fail to put a ſtop to the progreſs of intermittents.

During the ſickly ſeaſon, fires ſhould be likewiſe kept in every room in the dwelling houſe, even in thoſe caſes where the heat of the weather makes it neceſſary to keep the doors and windows open.

2. Let me adviſe my countrymen in ſickly ſituations, to prefer woolen and cotton to linen clothes in the ſummer and autumnal months. The moſt ſickly parts of the iſland of Jamacia have been rendered more healthy, ſince the inhabitants have adopted the uſe of woolen and cotton garments inſtead of linen.

During the late war, I knew many officers both in the Britiſh and American armies who eſcaped fevers in the moſt ſickly places, by wearing woolen ſhirts, or waiſtcoats conſtantly next to their ſkins. I have heard the preſent diminution of the human body in ſtrength and ſize, compared with its ancient vigor and form, aſcribed in part to the introduction of linen garments. I am not diſpoſed to controvert this opinion, but I am ſure of the efficacy of woolen clothes in wet and cold climates in preventing fevers of all kinds. The parliament of Great Britain

Britain compels every body that dies within the ifland to be buried in a woolen fhirt or winding fheet. The law would be much wifer if it compelled every body to wear woolen garments next to their fkins during life, and linen after death.

3. The diet in the fickly months fhould be generous. Wine and beer fhould be the drinks of this feafon inftead of fpirits and water. I do not think that fruit and vegetables of any kind produce fevers, but as the feafon of the year produces languor and weaknefs, a larger quantity of animal food than ufual is beft calculated to oppofe them. Salted meat for this reafon is preferable to frefh meat. Food of all kinds eaten during the fickly months fhould be well feafoned.

4. The evening air fhould be avoided as much as poffible. There are at prefent few places in Pennfylvania where it is fafe to fleep, or even to fet, after the going down of the fun, in the fickly months, with the windows open. The morning air before the fun rifes, fhould not be breathed, until the body has been fortified with a little folid aliment, or a *draught* of bitters. Thefe bitters fhould be made of centaury, wormwood, camomile, or the bark of the willow or dogwood trees, infufed in *water*. Bitters made with fpirits, or even wine, cannot be taken in a fufficient quantity to do fervice, without producing intoxication, or the deadly habit of loving and drinking fpirituous liquors.

5. Too much cannot be faid in favour of cleanlinefs, as a means of preventing fevers. The body fhould be bathed or wafhed frequently. It has been proved that in the highlands of Jamaica adding falt to water, renders it more powerful in preventing difeafes when applied to the body. Equal pains fhould be taken to promote cleanlinefs in every fpecies of apparel. Offal matters, efpecially thofe which are of a vegetable nature, fhould be removed from the neighbourhood of a dwelling houfe. The dung of domeftic animals during its progrefs towards manure

may

may be excepted from this direction. Nature, which made man and these animals, equally necessary to each other's subsistence, has kindly prevented any inconvenience from their living together. On the contrary, to repay the husbandman for affording a shelter to these useful and helpless animals, nature has done more. She has endowed their dung with a power of destroying the effects of marsh exhalations, and of preventing fevers. The miserable cottagers in Europe who live under the same roof, and in some instances in the same room with their cattle, are always healthy. In Philadelphia, fevers are less known in the neighbourhood of livery stables, than in any other part of the city. I could mention a family that has lived near thirty years near a livery stable in a sickly part of the city, that has never known a fever but from the measles or small-pox.

N° XXVI.

An Account of the late Dr. HUGH MARTIN'*s Cancer Powder, with brief Observations on Cancers. By* BENJAMIN RUSH, M. D. *&c. &c.*

Read February 3, 1786.

A FEW years ago a certain Dr. Hugh Martin, a surgeon of one of the Pennsylvania regiments stationed at fort Pitt, during the latter part of the late war, came to this city, and advertised to cure cancers with a medicine which he said he had discovered in the woods, in the neighbourhood of the garrison. As Dr. Martin had once been a pupil of mine, I took the liberty of waiting upon him, and asked him some questions respecting his discovery. His answers were calculated to make me believe, that his medicine was of a vegetable nature, and that it was originally an Indian remedy. He shewed

ſhewed me ſome of the medicine, which appeared to be the powder of a well dried root of ſome kind. Anxious to ſee the ſucceſs of this medicine in cancerous ſores, I prevailed upon the doctor to admit me to ſee him apply it in two or three caſes. I obſerved in ſome inſtances, he applied a powder to the parts affected, and in others only touched them with a feather dipped in a liquid which had a white ſediment, and which he made me believe was the vegetable root diffuſed in water. It gave me great pleaſure to witneſs the efficacy of the doctor's applications. In ſeveral cancerous ulcers, the cures he performed were complete. Where the cancers were much connected with the lymphatic ſyſtem, or accompanied with a ſcrophulous habit of body, his medicine always failed, and in ſome inſtances did evident miſchief.

Anxious to diſcover a medicine that promiſed relief in even a few caſes of cancers, and ſuppoſing that all the cauſtic vegetables were nearly alike, I applied the phytolacca or poke root, the ſtramonium, the arum, and one or two others, to foul ulcers, in hopes of ſeeing the ſame effects from them which I had ſeen from Dr. Martin's powder, but in theſe I was diſappointed. They gave ſome pain, but performed no cures. At length I was furniſhed by a gentleman from fort Pitt with a powder which I had no doubt, from a variety of circumſtances, was of the ſame kind as that uſed by Dr. Martin. I applied it to a fungous ulcer, but without producing the degrees of pain, inflammation, or diſcharge, which I had been accuſtomed to ſee from the application of Dr. Martin's powder. After this, I ſhould have ſuſpected that the powder was not a *ſimple* root, had not the doctor continued upon all occaſions to aſſure me that it was wholly a vegetable preparation.

In the beginning of the year 1784 the doctor died, and it was generally believed that his medicine had died with him. A few weeks after his death, I procured from Mr. Thomas Lieper, one of his adminiſtrators, a few ounces of the doctor's powder, partly with a view of applying it to a cancerous fore which then offered, and partly with a view of examining it more minutely than I had been able to do during the doctor's life. Upon throwing the powder, which was of a brown colour, upon a piece of white paper, I perceived diftinctly a number of white particles fcattered through it. I fufpected at firft that they were corrofive fublimate, but the ufual tefts of that metallic falt foon convinced me that I was miftaken. Recollecting that arfenic was the bafis of moft of the celebrated cancer powders that have been ufed in the world, I had recourfe to the tefts for detecting it. Upon fprinkling a fmall quantity of the powder upon fome coals of fire, it emitted the garlic fmell fo perceptibly as to be known by feveral perfons whom I called into the room where I made the experiment, and who knew nothing of the object of my enquiries. After this with fome difficulty I picked out about three or four grains of the white powder, and bound them between two pieces of copper, which I threw into the fire. After the copper pieces became red hot, I took them out of the fire, and when they had cooled, difcovered an evident whitenefs imparted to both of them. One of the pieces afterwards looked like dull filver. Thefe two tefts have generally been thought fufficient to diftinguifh the prefence of arfenic in any bodies, but I made ufe of a third, which has lately been communicated to the world by Mr. Bergman, and which is fuppofed to be in all *cafes* infallible.

I infufed a fmall quantity of the powder in a folution of a vegetable alkali in water for a few hours, and then poured it upon a folution of blue vitriol in water. The colour of the vitriol was immediately changed to a beautiful green, and afterwards precipitated.

I fhall clofe this paper with a few remarks upon this powder, and upon the cure of cancers and foul ulcers of all kinds.

The

1. The ufe of cauftics in cancers and foul ulcers is very ancient, and univerfal. But I believe *arfenic* to be the moft efficacious of any that has ever been ufed. It is the bafis of Plunkett's and probably of Guy's well known cancer powders. The great art of applying it fuccefsfully, is to dilute and mix it in fuch a manner as to mitigate the violence of its action. Dr. Martin's compofition was happily calculated for this purpofe. It gave lefs pain than the common or lunar cauftic. It excited a moderate inflammation, which feparated the morbid from the found parts, and promoted a plentiful afflux of humours to the fore during its application. It feldom produced an efcar; hence it infinuated itfelf into the deepeft receffes of the cancers, and frequently feparated thefe fibres in an unbroken ftate which are generally called the roots of the cancer. Upon this account, I think, in an ulcerated cancer it is to be preferred to the knife. It has no action upon the found fkin. This Dr. Hall proved by confining a fmall quantity of it upon his arm for many hours. In thofe cafes where Dr. Martin ufed it to extract cancerous or fchirrous tumors that were not ulcerated, I have reafon to believe that he always broke the fkin with Spanifh flies.

2. The arfenic ufed by the doctor was the pure white arfenic. I fhould fuppofe from the examination I made of the powder with the eye, that the proportion of arfenic to the vegetable powder, could not be more than $\frac{1}{40}$ part of the whole compound. I have reafon to think that the doctor employed different vegetable fubftances at different times. The vegetable matter with which the arfenic was combined in the powder which I ufed in my experiments, was probably nothing more than the powder of the root and berries of the folanum lethale, or deadly nightfhade. As the principal, and perhaps the only defign of the vegetable addition was to blunt the activity of the arfenic, I fhould fuppofe that the fame propor-

tion of common wheat flour as the doctor used of his caustic vegetables, would answer nearly the same purpose. In those cases where the doctor applied a feather dipped in a liquid to the sore of his patient, I have no doubt but his phial contained nothing but a weak solution of arsenic in water. This is no new method of applying arsenic to foul ulcers. Dr. Way of Wilmington, has spoken in the highest terms to me of a wash for foulnesses on the skin, as well as old ulcers, prepared by boiling an ounce of white arsenic in two quarts of water to three pints, and applying it once or twice a day.

3. I mentioned formerly that Dr. Martin was often unsuccessful in the application of his powder. This was occasioned by his using it indiscriminately in *all* cases. In schirrous and cancerous tumours, the knife should always be preferred to the caustic. In cancerous ulcers attended with a scrophulous or a bad habit of body, such particularly as have their seat in the neck, in the breasts of females, and in the axillary glands, it can only protract the patient's misery. Most of the cancerous sores cured by Dr. Martin were seated on the nose, or cheeks, or upon the surface or extremities of the body. It remains yet to discover a cure for cancers that taint the fluids, or infect the whole lymphatic system. This cure I apprehend must be sought for in diet, or in the long use of some internal medicine.

To pronounce a disease incurable, is often to render it so. The intermitting fever, if left to itself, would probably prove frequently, and perhaps more speedily fatal than cancers. And as cancerous tumours and sores are often neglected, or treated improperly by injudicious people, from an apprehension that they are incurable, (to which the frequent advice of physicians "to let them alone," has no doubt contributed) perhaps the introduction of arsenic into regular practice as a remedy for cancers, may invite to a more early application to physicians, and thereby pre-

vent

vent the deplorable cafes that have been mentioned, which are often rendered fo by delay or unfkilful management.

4. It is not in cancerous fores only that Dr. Martin's powder has been found to do fervice. In fores of all kinds, and from a variety of caufes, where they have been attended with fungous flefh or callous edges, I have ufed the doctor's powder with advantage.

I flatter myfelf that I fhall be excufed in giving this detail of a *quack* medicine, when the fociety reflect that it was from the inventions and temerity of quacks, that phyficians have derived fome of their moft active and ufeful medicines.

N° XXVII.

Illuftriffimæ ac celeberrimæ Societati Scientiarum quæ eft Philadelphiæ.

S. P. D.

CHRISTIANUS MAYER Ser^{mi} Electoris Palatini Aftronomus.

SCRIBENDI occafionem a Cl. D. Ferdinando Farmer oblatam eo minus negligendam putavi quod hac ratione aliquantum refpondeam honori, quo me illuftriffima focietas affecit, cum me in album fuorum fociorum adfcripfit. Ex libro Philadelphiæ impreffo & ad me tribus circiter abhinc annis tranfmiffo intellexi non fine magno animi mei fenfu, etiam Philadelphiæ excoli aftronomiam. Libro illo fcriptifque meis aftronomicis infelici incendio abhinc biennio confumtis, de novis meis quibufdam in cœlo inventis ad focietatem illuftriffimam aliquid fcribendum effe, duxi. Speculam novam ad omnes ufus accommodatam

tam Manhemii inhabito; nec desunt pretiosissima Londinensia instrumenta, in quibus præcipue eminet quadrans muralis æneus 8 pedum in rhadio a cel. artifici Bird anno 1775 confectus & plane insigni tubo achromatico instructus, solidissimeque muro affixus ad plagam cœli meridionalem, quo instrumento, quoties cœlum favet, utor quotidie. Adverti autem statim abhinc biennio in stellis fixis plane multis a primo gradu magnitudnis ad sextum usque, distingui alias stellulas parvulas comites, quarum aliæ ob lucem tranquillam & obtusam planetarum speciem referunt, aliæ telescopicam parvitatem non excedunt. Quod maxime mirabar, illud est, quod has stellulas comites, paucissimis duntaxat exceptis, nullo noto catalogo contineri viderem, cum tamen earum usum ad determinandum motum proprium fixarum esse plane insignem evidenter colligerem. Cum enim ibi, ubi paucorum plerumqe secundorum reperitur differentia ascensionis rectæ & declinationis inter fixam lucidiorem, ejusque comitem, lapsus temporis haud aliam variationem stellæ fixæ, quam ejus comiti inducere possit, unde demum cunque ista mutatio oriatur, sive a præcessione æquinoctiorum, sive a variatione obliquitatis eclipticæ, sive a deviatione instrumenti, seu ab aberratione luminis aut nutationis, sive ab alia quacunque causa, quæ pendeat a mutabili statu atmospheræ aut locorum latitudine, contingit sane, ut omnis mutatio visa inter fixam, ejusque comitem, motus proprii argumentum præbeat certissimum, sive is fixam sive ejus comitem afficiat. Noveram Halleum cel. Angliæ astronomum primum fuisse, qui anno 1719 ex instituta comparatione observationum Flamsteadii cum illis Ptolomæi in paucis quibusdam fixis, Syrio, Arcturo & Aldebaran deprehendit has stellas moveri motu singulari proprio. Sed simul noveram in Historia Cœlesti Brittannica Flamsteadii jam anno 1690 usurpatam fuisse a Flamsteadio vocem comitis fixarum, cum vir summus nec dum de motu proprio fixarum cogitasset. Astronomi reliqui Halleo posteriores quotquot in motum proprium fixarum inquisiverunt,

inquiſiverunt, Hallei methodum ſecuti ſunt, comparando obſervationes ſuas cum obſervationibus antiquorum: methodus hæc prolixos requirit calculorum labores, multiſque dubitationibus manet obnoxia ob incertitudinem, lubricamque conditionem inſtrumentorum, & obſervationum antiquarum; non item methodus mea nova, qua ex variatione inter comitem & fixam illuſtriorem obſervata, ſtatim conſequitur dari motum proprium, vel utriuſque vel allerutrius ſideris. Itaque ducentos fere diverſarum fixarum comites a biennio obſervavi, eundem fere paralellum ſtatim ante vel poſt fixam decurrentes, & obſervationes hujuſmodi plures cum cel. Angliæ aſtronomo Nevil Maſkelyne communicavi, qui eas ſibi gratiſſimas accidiſſe reſpondet. Ex multis, obſervationes paucas ad illuſtriſſimam ſocietatem ſpeciminis loco tranſmitto, quarum reſpondentes in Hiſtoria Cœleſti Britannica Flamſteadii invenio, unde ſimul patet, quam obſervationes hujuſmodi præclare ſerviant motui proprio detegendo. Prima et ſecunda columna ſiniſtima tabulæ ſequentis ex titulo facillime intelligitur. Tertia columna differentiam A. R. in tempore medio indicat inter ſtellam ejuſque comitem: comes præcedens fixam, primo loco in tabula ſcribitur, comes ſequens poſt fixam ponitur. Quarta columna differentiam declinationis inter fixam ejuſque comitem notat, qualem ego Manhemii obſervavi. Litera A ſignificat comitem eſſe auſtraliorem, litera B magis borealem. Sequentes columnæ, obſervationes ejuſdem ſtellæ factas a Flamſteadio, continent.

Mayer, Manheim.				Flamstaed, Greenwich.		
	Nomina Fixarum.	Diff. A. R. in tempore.	Differ. Declina.	Diff. A. R.	Differ. Declina.	
1777 Die 28 Feb.	ρ Leonis Comes 7æ. ſive 8væ.	2′. 15″.	38′.58″.6.A	2′. 18″.	38′. 50″. A	1690 Die 25 Mar.
1777 28 Feb. item 23 Feb.	Com. γ Leonis 6tæ. γ Leonis 4tæ	5″.	21′.57″.2.A	2″.	22′. 5″.	1691 6 Aprilis.
1777 4 Aprilis.	Comes τ Leonis	1′. 5″.	8′. 0″. 15. B	56.	8′. 30″. B	1690 25 Martii.
1777 11 Martii.	Propus Comes 5tæ	5′. 36″.	7′. 30.″	5′. 38″.	7′. 0″.	1690 7 Feb.
1777 8 Martii.	Procyon Comes 11. 8væ	1′. 40″. 5.	3′. 6″. 2. A	1′. 34″.	4′. 40″. A	1691 4 Octo.
1777 13 Nov.	Com. 7mæ ε Piſcium	51″.	2′. 57″. 1. B	47″.	2′. 30″. B	1700 Nov. 13.
1777 Aprilis 6.	Comes Π Cancri	2′. 49″.	1′. 40″. B	2′. 44″.	1′. 12″. B	1690 Feb. 18.
1777 Aprilis 8.	Com. 7æ β Leonis	30″.	18′.27″.1.A	33″½		1692 Aprilis 24.
1776 Mai. 18.	Arcturus Comes.	0. 0. 6″.	0. 23. 37. 3	Com. 0. 5″. Arcturus.	0. 26. 30.	1690 Febru. 14.
1777 Mart. 11.	Procyon Comes 7æ.	39″.	3′. 8″. 1. A	35″.		1692 22 Janu.
1776 Mai. 18.	Arcturus Comes.	0. 0. 6.	0. 23. 37. 4	Maskelyne. A. R. 0. 0. 4.	0.23′.58″.8.	1765 20 Mai.
1777 Aprilis 1.	O Cancri ſeq. Π Cancri 6.	14′. 56″.	32. 29. 6. B	15. 1.	32. 25. B	1691 10 Martii.

Apparet ex omnibus Arcturum omnium celerrime ferri motu proprio in occaſum, ſequidem idem comes, qui tempore Flamſteadii 1690, die 14 Februarii Arcturum præcedebat 5″ in tempore nunc 6″ poſt Arcturum meridianum ingreditur: ex imminuta quoque differentia declinationis inter Arcturum ejuſque comitem, patet, Arcturum motu proprio quotannis fere 2″ in circulo moveri verſus auſtrum. Id ex eo perſpicuum eſt, quod declinatio a me obſervata comitis reducta ad parallelum grenovicenſem eandem producat altitudinem poli grenovicenſis, qualis ex obſervatione Flamſteadii eruitur, non item declinatio hodie obſervata Arcturi etiam aberratione & nutatione correcta. Similis indagatio fieri poteſt in reliquis fixis, earumque comitibus, atque ex inſtituta comparatione cum aliis fixis

deprehendi

deprehendi poteſt, num fixæ an comiti vel utrique motus proprius tribuendis ſit.

Obſervationes omnes in plano meridiani quadrante murali factæ ſunt Manhemii in nova ſpecula a me ædificata S. Electoris Palatini: ejus longitudo ad ortum Grenovicii eſt fere 34′ 6″ in tempore, latitudo fere 49° 27′ 50″ Gaudebo maxime ſi has meas obſervationes illuſtriſſimæ ſocietati haud ingratas accidiſſe intellexero, cujus benevolentiæ me demiſiſſime commendo.

Illuſtriſſimæ ac celeberrimæ Societati,
Cuetor et ſervus perpetuus,
CHRISTIANUS MAYER, Sereniſſimi Electoris Palatini et Ducis Bavariæ Aſtronomus.

Manhemii in Germania, die 24 Aprilis 1778.

(TRANSLATION.)

To the illuſtrious and celebrated Society of Sciences at Philadelphia,

CHRISTIAN MAYER, Aſtronomer to his Serene Highneſs the Elector Palatine,
Wiſheth Proſperity.

I THOUGHT it my duty to embrace the opportunity which my good friend the Rev. Mr. Ferdinand Farmer has procured me of writing to your illuſtrious ſociety, that I may make ſome return to the honour which you have done me by electing and enrolling me among your members. It gave me a very ſenſible pleaſure to find, by the printed volume of tranſactions, which you ſent me about three years ago, that the ſcience of aſtronomy was cultivated even at Philadelphia. That volume of yours, together

gether with my own aftronomical papers, having been unhappily deftroyed by fire about two years ago, I have refolved to give your illuftrious fociety fome fhort account of certain new celeftial difcoveries which I had made.

My refidence is now at Manheim, in a new obfervatory, fitted for every aftronomical purpofe; and well furnifhed with the moft precious and accurate inftruments made at London; amongft which the chief is a brafs mural quadrant of eight feet radius, the workmanfhip of that celebrated artift Mr. Bird, finifhed in the year 1775, fitted with an achromatic telefcope, and fixed to a folid wall towards the meridian. With this inftrument I make daily obfervations of the heavens, when the weather will permit, and two years ago I diftinctly difcovered, among many of the *fixed ftars* (from the *firft* to the *fixth* magnitude) other *concomitant* or *attendant little ftars*; fome of which, from their mild, faint (or unfparkling) light, have the appearance of planets, while others of them have the appearance of telefcopic ftars, in refpect to their fmallnefs.

But what furprifed me moft was, that none of thefe *attendant* little ftars, a few perhaps excepted, have ever been noted in any catalogue which I have feen; although I could clearly collect the fingular ufe which may be made of them for afcertaining and determining the proper motion of the fixed ftars, as it is called. When the difference of right afcenfion and declination between two ftars is at moft but a few feconds, any variation arifing from the preceffion of the equinoxes, the variation of the obliquity of the ecliptic, the deviation of the inftrument, the aberration of light or the nutation, or from any other caufe depending on the mutable ftate of the air or latitude of places, muft affect them both equally. Therefore when after any length of time a greater variation of right afcenfion or declination is found in one of fuch ftars than in the other, it affords a certain argument of the proper motion of one or the other, whether that change affects the fixed ftar or its attendant.

I know

I know that the celebrated Engliſh aſtronomer Halley, was the firſt who, about the year 1719, by a careful comparison of the obſervations of Flamſtead with thoſe of Ptolemy, reſpecting a few fixed ſtars, viz. Sirius, Arcturus and Aldebaran, diſcovered that theſe ſtars had a proper motion of their own. But I likewiſe know, that in Flamſtead's Britiſh celeſtial hiſtory the word *concomitant or attendant of fixed ſtars* is made uſe of, when that great man had not even thought of a proper motion of the fixed ſtars.

The other aſtronomers, poſterior to Halley, as far as they inveſtigated the proper motion of the *fixed ſtars*, followed the Italian method of comparing their own obſervations with thoſe of the ancients. This method requires the labour of prolix calculation, and remains liable to doubts and uncertainty, on account of the inaccuracy of ancient inſtruments and obſervations. My new method is not liable to ſuch objections, becauſe from the obſerved variation of the attendant ſtar and the brighter fixed ſtar, it immediately follows that there is a proper motion given, either of the one or the other.

I have, therefore, in the ſpace of two years, obſerved almoſt two hundred attendants of different fixed ſtars, running almoſt the ſame parallel, immediately before or after the fixed ſtar; and have communicated many of ſuch obſervations to the celebrated Engliſh aſtronomer Nevil Maſkelyne, who has expreſſed his high ſatisfaction therewith.

Out of many obſervations, I ſend your illuſtrious ſociety a few by way of ſpecimen, being ſuch whereof I find correſpondent obſervations in the Flamſteadian celeſtial hiſtory; whence it appears at once how excellently obſervations of this kind ſerve for diſcovering the proper motion of ſuch ſtars.

The firſt and ſecond column of the following table next to the left hand is eaſily underſtood from its title. The third column ſhews the difference of A. R. in mean time

 between

between the *attendant* and *ſtar*. The attendant which precedes the fixed ſtar occupies the firſt place in the table. The ſubſequent attendant is placed after the fixed ſtar. The fourth column ſhews the difference of declination between the fixed ſtar and attendant. The letter A, ſignifies that the attendant is more ſouth, B, that it is more north than the ſtar. The ſubſequent columns contain Flamſtead's obſervations of the ſame fixed ſtars.

The TABLE.

By Mayer, at Manheim.				By Flamstead, at Greenwich.		
Time of Obſervation.	Names of Star.	Diff. A. R. in time.	Differ. of Declina.	Diff. A. R. in time.	Differ. of Declina.	Time of Obſervation.
1777 Februar. 28.	ρ Leonis attendant 7m.	2′. 15″.	38′.58″.6.A	2′. 18″.	38′. 50″. A	1690 March 25.
1777 Feb. 25, 28.	Attend. 6m. γ Leonis 4m.	5″.	21′.57″.2.A	2″.	22′. 5″.	1691 6 April.
1777 April 4.	Attendant, τ Leonis	1′. 5″.	8′. 0″. 15. B	56.	8′. 30″. B	1690 25 March.
1777 March 11.	Propus attendant 5m.	5′. 36″.	7′. 30.″	5′. 38″.	7′. 0″.	1690 7 Feb.
1777 March 8.	Procyon attendant 8m.	1′. 40″. 5.	3′. 6″. 2. A	1′. 34″.	4′. 40″. A	1691 4 Octo.
1777 13 Nov.	Attendant 7m. ε Piſcium	51″.	2′. 57″. 1. B	47″.	2′. 30″. B	1700 Nov. 13.
1777 April 6.	Attendant, Π Cancri	2′. 49″.	1′. 40″. B	2′. 44″.	1′. 12″. B	1690 Feb. 18.
1777 April 8.	Attendant 7m. β Leonis	30″.	18′.27″.1.A	33″½		1692 April 24.
1776 May 18.	Arcturus Attendant,	0. 0. 6″.	0. 23. 37. 3	Com. 0. 5″. Arcturus.	0. 26. 30.	1690 Febru. 14.
1777 March 11.	Procyon Attendant 7m.	39″.	3′. 8″. 1. A	35″.		1692 22 Janu.
				Maskelyne.		
1776 May 18.	Arcturus Attendant,	0. 0. 6.	0. 23. 37. 4	A. R. 0. 0. 4.	0. 23′.58″.8.	1765 20 May.
1777 April 1.	O Cancri Π Cancri 6m.	14′. 56″.	32. 29. 6. B	15. 1.	32. 25. B	1691 10 March.

It appears from every obſervation, that of all the ſtars, Arcturus, by his proper motion, is carried with the greateſt celerity weſtward; ſince the ſame attendant, which in Flamſtead's

Flamstead's time, 1690, February 14, preceded Arcturus 5″ in time, now comes to the meridian 6″ after him. Likewise from the very small difference of declination between Arcturus and his attendant, it appears that Arcturus by his proper motion moves almost 2″ southerly every year in a circle.

N° XXVIII.

Observations on the Cause and Cure of the Tetanus, by BENJAMIN RUSH, M. D. *Professor of Chemistry in the University of Pennsylvania.*

Read Mar. 17, 1786.

DURING my attendance upon the military hospitals of the United States, in the course of the late war, I met with several cases of the Tetanus. I had frequently met with this disorder in private practice, and am sorry to say that I never succeeded with the ordinary remedy of opium in any one case that came under my care. I found it equally ineffectual in the army. Baffled in my expectations from a remedy that had been so much celebrated, I began to investigate more particularly the nature of the disorder. I found it to be a disorder of warm climates, and warm seasons. This led me to ascribe it to relaxation. I resolved to attempt the cure of it by a set of medicines in some measure the opposites of most of the medicines that had been employed in that disorder. Soon after I adopted this resolution, I was called to visit Col. John Stone, who was wounded through the foot at the battle of Germantown on the 4th of October 1777. He was in the third day of a Tetanus. His spasms were violent and his pains so exquisite that his cries were heard near a hundred yards from his quarters. His head was thrown a little backwards, and his jaw had become stiff and contracted.

He

He was under the care of a ſkilful regimental ſurgeon who was pouring down opium in large quantities without effect.

Duty and friendſhip both led me to do my utmoſt to ſave the life of this valuable officer. I immediately diſmiſſed the opium, and gave him large quantities of wine and bark, to the amount of two or three ounces of the latter, and from a bottle to three pints of the former in the day. In a few hours I was delighted with their effects. His ſpaſms and pains were leſs frequent and violent, and he ſlept for ſeveral hours, which he had not done for ſeveral days and nights before.

With the ſame indication in view, I applied a bliſter between his ſhoulders, and rubbed in two or three ounces of mercurial ointment upon the outſide of his throat. He continued to mend gradually under the operation of theſe medicines, ſo that in ten days he was out of danger, although the ſpaſm continued in his wounded foot for ſeveral weeks afterwards. In the ſummer of the year 1782 I was called to viſit a ſervant girl of Mr. Alexander Todd, merchant of this city, who had brought on a Tetanus by ſleeping in the evening on a damp brick pavement, after a day in which the mercury in Farenheit's thermometer had ſtood at near 90°. The caſe was nearly as violent and alarming as the one I have deſcribed. I treated her in the ſame manner, and with the ſame ſucceſs. To the above named medicines, I added only the oil of amber which ſhe took in large doſes, after I ſuſpected the tonic powers of the bark and wine began to looſe their effects. The good effects of the oil were very obvious. She recovered gradually and has continued ever ſince in good health. In the ſummer of the ſame year I was called to Alexander Leſlie, a joiner, who had run a nail in his foot. I found him the day afterwards in extreme pain, with ſmall convulſions and now and then a twinge in his jaw. The wound in his foot was without ſwelling

ſwelling or inflammation. I dilated the wound and filled it with lint moiſtened with ſpirit of turpentine. This in a little while produced a good deal of pain and a great inflammation in his foot. While I was preparing to treat him in the manner I had treated the two former caſes, the pains and ſpaſms in his body ſuddenly left him, and in twenty-four hours after I ſaw him, he complained of nothing but of the pain and ſwelling in his foot, which continued for ſeveral weeks and did not leave him till it ended in a ſuppuration. From the hiſtory of theſe three caſes, I beg leave to make the following remarks.

1. That the prediſpoſition to the Tetanus depends upon relaxation. This relaxation is generally produced by heat; but exceſſive labour, watchings, marches, or fatigue from any cauſe, all produce it likewiſe, and hence we find it more frequent from wounds received in battles, than from ſimilar wounds received in any other way. Theſe wounds more certainly produce the Tetanus, if they have been preceded for ſome time with warm weather. Dr. Shoepft, the phyſician general of the Anſpach troops that ſerved at the ſiege of York in the year 1781, informed me of a ſingular fact upon this ſubject. Upon converſing with the French ſurgeons after the capitulation, he was informed by them that the troops who arrived juſt before the ſiege from the Weſt-Indies with Count de Graſſe, were the only troops belonging to their nation that ſuffered from the Tetanus. There was not a ſingle inſtance of that diſorder among the troops who had ſpent a winter in Rhode-Iſland.

2. As the Tetanus ſeems to be occaſioned by relaxation, the medicines indicated to cure it are ſuch only as are calculated to remove this relaxation and to reſtore a tone to the ſyſtem. The bark and wine appear to act in this way. The operation of the bliſters is of a more complicated nature. That they are ſedative and antiſpaſmodic in fevers is univerſally acknowledged, but in the peculiar ſtate of

irritability

irritability which occurs in the Tetanus, perhaps their effects are more ſimply ſtimulating. But I will go one ſtep further. In order to cure this diſorder, it is neceſſary not only to produce an ordinary tone in the ſyſtem, but ſomething like the inflammatory diatheſis. The abſence of this diatheſis is taken notice of by all authors, particularly by Dr. Cullen*.

Mercury appears to act only by promoting this diatheſis. Hence it never does any ſervice unleſs it be given time enough to produce a ſalivation. The irritation and inflammation produced in the mouth and throat, ſeldom fail to produce the inflammatory diatheſis, as blood drawn in a ſalivation has repeatedly ſhewn.

I apprehend that the oil of amber acts as a ſtimulant chiefly in this diſorder. I have heard of a Tetanus being cured in the iſland of Grenada by large doſes of muſtard. Dr. Wright, lately of the iſland of Jamaica, relates in the 6th volume of the London Medical Eſſays, ſeveral remarkable caſes of the Tetanus being cured by the cold bath. Both theſe remedies certainly act as ſtimulants and tonics. By reaſoning *à priori*, I conceive that electricity would be found to be an equally powerful remedy in this diſorder.

As a general inflammatory diatheſis diſpoſes to topical inflammation, ſo topical inflammation diſpoſes to general inflammatory diatheſis. Wounds upon this account are leſs apt to inflame in ſummer than in winter. In the Tetanus I have uniformly obſerved an abſence of all inflammation in the wounds or injuries that produced it. A ſplinter under the nail produces no convulſions, if pain, inflammation and ſuppuration follow the accident. It is by exciting pain and inflammation I apprehend that the ſpirit of turpentine acts in all wounds and punctures of nervous and tendinous parts. I have never known a ſingle inſtance of a Tetanus from a wound, where this remedy had been applied in time. It was to excite an inflammation in the foot of Mr. Leſlie, that I dilated the wound and

* Firſt Lines, Vol. III.

and filled it with the ſpirit of turpentine. I was not ſurpriſed at its good effects in this caſe, for I was prepared to expect them.

I find a remarkable caſe related in Dr. W. Monroe's Theſis, publiſhed in Edinburgh in the year 1783, of a black girl who had a Tetanus from running a nail in her foot, being perfectly cured by deep and extenſive inciſions being made in the wounded part by Dr. John Bell, of the iſland of Grenada.

It is by producing inflammation in a particular part, and tone in the whole ſyſtem, I apprehend that the amputation of a wounded limb ſometimes cures a Tetanus; and it is becauſe the degrees of both are too inconſiderable to oppoſe the violence of the ſpaſms in the advanced ſtages of the Tetanus, that amputation often fails of ſucceſs.

I have been informed by a phyſician who reſided ſome time at St. Croix, that the negroes on that iſland always apply a plaiſter made of equal parts of ſalt and tallow to their freſh wounds, in order to prevent a locked jaw. The ſalt always produces ſome degree of inflammation.

If the facts that have been ſtated are true, and the inferences that have been drawn from them are juſt, how ſhall we account for the action of opium in curing this diſorder? I do not deny its good effects in many caſes, but I believe it has failed in four caſes out of five in the hands of moſt practitioners. It is remarkable that it ſucceeds only where it is given in very large doſes. In theſe caſes I would ſuppoſe that its ſedative powers are loſt in its ſtimulating. It is upon a footing, therefore, in one reſpect, with the ſtimulating medicines that have been mentioned; but from its being combined with a ſedative quality, it is probably inferior to moſt of them. I am the more inclined to adopt this opinion, from an account I once received from Dr. Robert, of the iſland of Dominique, who informed me that after having cured a negro man of a Tetanus with large doſes of opium, he was afterwards ſeized with a diſorder in his ſtomach, of which he died in a few days. Upon opening him, he found his ſtomach inflamed and mortified. I do not forbid the uſe of opium altogether in this diſorder. I think ſmall doſes of it may be given to eaſe pain, as in other ſpaſmodic diſorders; but as its qualities are complicated, and its efficacy doubtful, I think it ought to yield to more ſimple and more powerful remedies.

To the caſes that have been mentioned, I could add many others, in which I have reaſon to believe that the excitement of a topical inflammation by artificial means, has effectually prevented a Tetanus.

To this account of the Tetanus, I beg leave to ſubjoin a few words upon a diſorder commonly called the jaw-fall in infants, or the Triſmus Naſcentium of Dr. Cullen, which is nothing but a ſpecies of Tetanus.

I have met with three caſes of it in this city, all of which proved fatal. The ſtage of the diſorder in which I was conſulted, and the age and weakneſs of the infants, forbad me to attempt any thing for their relief. I have introduced the ſubject of this diſorder in children, only for the ſake of mentioning a fact communicated to me by the late Dr. Cadwalader Evans of this city. This gentleman practiſed phyſic for ſeveral years in Jamaica, where he had frequent opportunities of ſeeing the Tetanus in the black children. He found it in every caſe to be incurable. He ſuppoſed it to be occaſioned by the retention of the meconium in the bowels. This led him invariably to purge every child that was born upon the eſtates committed to his care. After he adopted this practice, he never met with a ſingle inſtance of the Tetanus among children.

Perhaps it may tend to enlarge our ideas of the Tetanus, and to promote a ſpirit of enquiry and experiment, to add, that this diſorder is not confined to the human ſpecies. I have known ſeveral inſtances of it in horſes from nails running

running in their feet, and other accidents. It is attended with a rigidity of the muſcles of the neck, a ſtiffneſs in the limbs, and ſuch a contraction of the jaw as to prevent their eating. It is generally fatal. In two caſes I had the pleaſure of ſeeing the diſeaſe perfectly cured by applying a potential cauſtic to the neck under the mane, by large doſes of oil of amber, and by plunging one of them into the river, and throwing buckets of cold water upon the other.

How far the reaſonings contained in this paper may apply to the hydrophobia, I cannot determine, having had no opportunity of ſeeing the diſeaſe ſince I adopted theſe principles; but from the ſpaſmodic nature of the diſorder, from the ſeaſon of the year in which it generally occurs, and above all, from the caſe related by Dr. Fothergill, of a young woman having eſcaped the effects of the bite of a mad cat by means of the wound being kept open, (which from its ſeverity was probably connected with ſome degrees of inflammation) is it not probable that the ſame remedies, which have been uſed with ſucceſs in the Tetanus, may be uſed with advantage in the hydrophobia?—In a diſeaſe ſo deplorable, and hitherto ſo unſucceſsfully treated, even a conjecture may lead to uſeful experiments and enquiries.

N° XXIX.

To His Excellency BENJAMIN FRANKLIN, *Eſq. L. L. D. Preſident of the State of Pennſylvania, and of the American Philoſophical Society, &c.*

SIR, Philadelphia, January 12, 1786.

Read March 17, 1786.

THE ſubject of ſmoky chimneys, of which I had the honor of converſing with you at your own houſe laſt evening, is of ſo much importance to

 every

every individual, as well as to every private family, that too much light cannot be thrown upon it.

A ſmoky houſe and a ſcolding wife,
Are (ſaid to be) two of the greateſt ills in life.

And however difficult it may be to remedy one of thoſe ills, yet any advances we may be able to make towards removing the inconveniencies ariſing from the other, cannot fail to be favourably received by the public. As they are ſhortly to be favoured with your ſentiments on that ſubject, poſſibly the following obſervations, which were in fact occaſioned by neceſſity, and are the reſult of my own experience, may not be altogether undeſerving of notice.

When I left London and went to live in Devonſhire in the latter end of the year 1777, it happened to be my lot to dwell in an old manſion which had been recently modernifed, and had undergone a thorough repair. But as in moſt of the old houſes in England, the chimneys, which were perhaps originally built for the purpoſe of burning wood, though they had been contracted in front, ſince coal fires came into general uſe, to the modern ſize, yet they were ſtill, above, out of ſight, extravagantly large. This method of building chimneys may perhaps have anſwered well enough while it was the cuſtom to ſit with the doors and windows open; but when the cuſtoms and manners of the people began to be more poliſhed and refined, when building and architecture were improved, and they began to conceive the idea of making their chambers cloſe, warm, and comfortable, theſe chimneys were found to ſmoke abominably, for want of a ſufficient ſupply of air. This was exactly the caſe with the houſe in which I firſt lived, near Exeter, and I was under the neceſſity of trying every expedient I could think of to make it habitable.

The firſt thing I tried, was that method of contracting the chimneys by means of earthen pots, much in uſe in England,

England, which are made on purpoſe, and which are put upon the tops of them; but this method by no means anſwered. I then thought of contracting them below, but as the method of contracting them in front to the ſize of a ſmall coal-fire grate has an unſightly appearance, as it makes a diſagreeable blowing like a furnace, and as it is the occaſion of conſuming a great deal of unneceſſary fuel, the heat of which is immediately hurried up the chimney, I rejected this method, and determined to contract them above, a little out of ſight. For this purpoſe I threw an arch acroſs, and alſo drew them in at the ſides. This had ſome effect, but as this contraction was made rather ſuddenly, and the ſmoke, by ſtriking againſt the corners that were thereby occaſioned, was apt to recoil, by which means ſome part of it was thrown out into the room; I determined to make the contraction more gradually, and therefore run it up at the back, where the depth of the chimney would admit of it, and alſo ſhelving or ſloping in a conical kind of direction at the ſides, as high as a man, ſtanding upright, could conveniently reach, and by this means brought the cavity within the ſpace of about twelve by fourteen or ſixteen inches, which I found ſufficiently large to admit a boy to go up and down to ſweep the chimnies. This method I found to ſucceed perfectly well, as to curing the chimneys of ſmoking, and it had this good effect of making the rooms conſiderably warmer; and as this experiment ſucceeded ſo well, ſince the only uſe of a chimney is to convey away the ſmoke, I determined to carry it ſtill farther, in order to aſcertain with preciſion, how much ſpace is abſolutely neceſſary for that purpoſe, becauſe all the reſt that is ſhut up, muſt be ſo much gained in warmth. Accordingly I laid a piece of ſlate acroſs the remaining aperture, removable at pleaſure, ſo as to contract the ſpace above two thirds, leaving about three inches by twelve remaining open; but this ſpace, except when the fire burnt remarkably clear, was ſcarcely

ſcarcely ſufficient to carry away the ſmoke. I therefore enlarged it to half the ſpace, that is, to about ſix by ſeven or eight inches, which I found fully ſufficient to carry away the ſmoke from the largeſt fires.

When I removed into the Bedford Circus in Exeter, though the houſe was modern, and almoſt perfectly new, yet the chimneys were large; in conſequence of which almoſt every room of it ſmoked. My predeceſſor, who was the firſt inhabitant, had been at great expence in patent ſtoves, &c. but without effect; but by adopting the method I have juſt now deſcribed, I not only cured every chimney of ſmoking, but my houſe was remarked for being one of the warmeſt and moſt comfortable to live in of any in that large and opulent city.

The houſe I now live in, in Philadelphia, I am told, has always had the character of being both cold and ſmoky; and I was convinced, as ſoon as I ſaw the rooms and examined the chimneys, that it deſerved that character; for though the rooms were cloſe, the chimneys were large: And we ſhall ever find, that if our chimneys are large, our rooms will be cold even though they ſhould be tolerably cloſe and tight; becauſe the conſtant ruſhing in of the cold air at the cracks and crevices, and alſo at every opening of the door will be ſufficient to chill the air, as faſt as it is heated, or to force the heated air up the chimney; but by contracting the chimneys I have cured it of both theſe defects. There was one remarkable circumſtance attending the contraction of the chimney in the front parlour, which deſerves to be attended to; which was, that before I applied the caſt iron plate, which I made uſe of inſtead of ſlate, to diminiſh the ſpace requiſite for a chimney ſweeper's boy to go up and down, the ſuction or draught of air was ſo great, that it was with difficulty I could ſhut the door of the room, inſomuch that I at firſt thought it was owing to a tightneſs of the hinges, which I imagined muſt be remedied, but upon applying the

the iron plate, by which the ſpace was diminiſhed one half, the door ſhut to with the greateſt eaſe. This extraordinary preſſure of the air upon the door of the room, or ſuction of the chimney, I take to be owing in ſome meaſure to the unuſual height of the houſe.

Upon the whole, therefore, this fact ſeems clearly aſcertained, viz. That the flue or ſize of the chimney, ought always to be proportioned to the tightneſs and cloſeneſs of the room, ſome air is undoubtedly neceſſary to be admitted into the room in order to carry up the ſmoke, otherwiſe as you juſtly obſerved we might as well expect ſmoke to ariſe out of an exhauſted receiver; but if the flue is very large, and the room is tight, either the ſmoke will not aſcend, in conſequence of which will be, that the air of your room will be ſo frequently and ſo conſtantly changed that as faſt as it is heated, it will be hurried away, with the ſmoke, up the chimney, and of courſe your room will be conſtantly cold.

One great advantage attending this method of curing ſmoky chimneys is, that, in the firſt place, it makes no aukward or unſightly appearance, nothing being to be ſeen but what is uſual to chimneys in common; and in the ſecond place that it is attended with very little expence, a few bricks and mortar with a plate or covering to the aperture, and a little labour, being all that is requiſite. But in this new country where crops of houſes may be expected to riſe almoſt as quick as fields of corn, when the principles upon which chimneys ought to be thoroughly underſtood, it is to be hoped, that not only this expence, ſmall as it is, but that all the other inconveniencies we have been ſpeaking of, will be avoided, by conſtructing the flues of the chimneys ſufficiently ſmall.

From your humble ſervant,

THOMAS RUSTON.

N° XXX.

Obſervations on the annual Paſſage of Herrings, by Mr. JOHN GILPIN.

AS this very uſeful part of the finny race has never been found in the freſh rivers, or waters of Europe, it remains a query amongſt the naturaliſts, where they go to ſpawn and perpetuate their ſpecies. I apprehend this query may be anſwered to the ſatisfaction of the curious by an account of their annual progreſs, from which it will appear they are a fiſh of paſſage, and obſerve one regular annual rout in the ſea, ſhifting their climate with the ſun, and that it is the ſame ſcoole which is found at different times about Britain and in America. This opinion is founded on obſervations made on ſeeing them caught at Whitehaven and in this country, from which I have not obſerved that there is any viſible difference in the fiſh in the different places, except that thoſe at Whitehaven are fatter and rounder than thoſe in America; but this difference is not ſo great as that between the ſpring and fall mackarel, and which I conceive might be accounted for from the time of the year, and manner in which they appear on each coaſt. For they are found on the other ſide the Atlantic, or rather in the North ſea, in the favourable month of June about the iſlands of Shetland, from whence they proceed down to the Orkneys, and then dividing, they ſurround the iſlands of Great-Britain and Ireland, and unite again off the Land's End in the Britiſh channel in September, from whence this grand united ſcoole ſteers ſouth-weſt, and is not found any more on that ſide or in the Atlantic, until the ſame time the enſuing year, but appear next on the American coaſts, coafts. They arrive in Georgia and Carolina the latter end of January, and in Virginia in February; and coafting from thence eaftward to New-England, they divide and go into all the bays, rivers, creeks and even fmall ftreams of water in amazing quantities, and continue fpawning in the frefh water until the latter end of April, when the old fifh return into the fea, where they change their latitudes by a northward direction and arrive at Newfoundland in May; after which we neither hear or fee any thing more of them in America, until their return amongft us the enfuing fpring, and bring with them a providential bleffing to the poor. Their coming fooner or later up our rivers depends on the warmth or coolnefs of the feafon: And it is further obferved that if a few warm days invite them up, and cool weather fucceeds, it totally checks their paffage until more warm weather returns. From all which circumftances it appears probable there is a certain degree of warmth particularly agreeable to them, which they endeavour to enjoy by changing their latitude according to the diftances of the fun. Thus they are found in the Britifh channel in September, but leave it when the fun is at too great a diftance from them in the fouthern hemifphere, and pufh for a more agreeable climate; and when the weather in America becomes too warm in May, (after having depofited their eggs in fhallow water and fecured their young fry from the fifh of prey,) fteer the courfe which leads to the cooler northern feas, and by that prudent change of place perpetually enjoy the temperature of climate beft adapted to their nature; which from the table hereto annexed, fhewing the places and times of their vifitation, and the calculation of the diftance of the fun at thofe times from them, is that degree of warmth which is produced by the mean diftance between 37 to 43 degrees; except whilft they are fpawning; during which they bear a greater degree of heat from the neceffity of remaining in it a fhort time to fpawn; and alfo on the other extreme, when detained at too great a diftance by the ifland of Great-Britain and its dependencies.

Here another query occurs, what becomes of the young fry, the produce of the ſpawn they left in the freſh waters of America? We know they do not follow the old ones the firſt ſeaſon, becauſe they are found in great ſcooles in all the American bays during the ſummer, and diſappear in the fall, from whence it may rationally be ſuppoſed that from their natural propenſity to keep at a certain diſtance from the ſun, the ſeaſon leads them to a different courſe from the old ones, by which they meet their parentage about the latitude 23° N. and 70° W. longitude, and there tack about and follow the older ones; which, being larger and ſtronger than the younger, come firſt into our harbours, but are fewer in number than the leſſer, probably from having ſuffered great loſs and pillage in their long rout from the fiſh of prey, and their greater enemies the fiſhermen in the different parts of the world.

A Table ſhewing nearly about the place of the grand ſcooles of herrings, and their mean diſtance from the ſun.

* Place and Time.		Latitude.	Longitude.	Sun's Declination.	The Mean Diſtance.
I.	January,	23	70	20 S.	43
II.	February,	32	79	12	44
III.	March,	36	75	0	36
IV.	April,	39	72	10N.	29
V.	May,	49	50	19	30
VI.	June,	65	15	23	42
VII.	July,	58	0	21	37
VIII.	Auguſt,	52	0	14	38
IX.	September,	48	6	0	48
X.	October,	35	22	9	44
XI.	November,	22	40	18	40
XII.	December,	18	52	23	41

* See Map B, in Plate V.

Some Obfervations and Reafons given for the courfe of the Herrings, and the variation in their Mean Diftance from the Sun in different months of the year.

[See Map B, Plate V.]

JANUARY. In this month the herrings are fuppofed to be returning from too warm a climate and the approaching fun, from which they retreat faft.

FEBRUARY. The time of fpawning now drawing nigh, the herrings, in this month pafs through the gulph ftream, and fall on the coaft of America, in order to depofit their fpawn in frefh fhoal water.

MARCH. Now being the beginning of the time of fpawning, the largeft and ftrongeft fifh, which perhaps are the oldeft, rufh up into the bays, inlets and frefh water ftreams.

APRIL. In this month the leffer, weaker, and perhaps younger fifh, rufh up even to the heads of fmall ftreams, as far as it is poffible for them to get, and lay their fpawn. Thefe are twice as numerous as the other.

MAY. Having been detained by the fpawning feafon, they are overtaken by the fun, and nearer to it now than at any other time; they therefore haften out of the rivers in this month, and make great way towards the North fea.

JUNE. Now having by a rapid progrefs pufhed into a cold climate, on a chilly, icy coaft, and the fun beginning to draw towards the fouth, they whirl round eaftward.

JULY. The coldnefs of this fea, and the fun's declination towards the fouth, now inclines them that way, in which they fall on the Orkneys, and the fcoole divides.

AUGUST. The grand fcoole being divided, now furround the whole ifland of Great-Britain and Ireland, and are caught on every fide.

SEPTEMBER. Having been detained the laft month by their obftruction amongft the iflands, and being harraffed by the fifhermen, their mean diftance is now the greateft; they collect into one body and haften to the fouthward.

OCTOBER. Being now under great way, they leffen their mean diftance, and by the courfe which they fteer, which perhaps is inclined more weftward by the current of the trade wind, they pafs the Atlantic.

NOVEMBER. Being now more in the trade, and having approached a warmer climate, their motion is fuppofed to incline more weftward.

DECEMBER. The fun now beginning to return, they are fuppofed to incline more northward, to the place where we began; where they are fuppofed to meet their young fry.

N° XXXI.

Obfervations on a Solar and a Lunar Eclipfe, communicated to the Society by M. M. De GRAUCHAIN, *Major General of the French Squadron.*

(Tranflated from the French.)

GENTLEMEN, Newport, 5th December, 1780.

THE ftudy of aftronomy having often occupied my leifure during the peace, I could not refufe myfelf even in the midft of the preparations for war, an oppor-

 tunity

tunity which presented of making two important observations, which I have the honour of sending you.

Eclipses form the basis of chronology; this may one day serve to fix the epocha of the independence of America, one of the most interesting in the history of mankind. This is a motive to dedicate these observations to you; and I pay this respect with the greatest pleasure to an illustrious society, whose members know how at the same time to enlighten their country by their knowledge in mathematics and philosophy, and to serve them successfully in their councils and armies.

I am, &c.

DE GRAUCHAIN, Major General
of the French Squadron.

MESSIEURS,

L'ETUDE de l'astronomie ayant souvent occupé mon loisir pendant la paix, je n'ai pu me refuser, même au milieu de l'appareil de la guerre, a l'occasion qui fut presentée de faire deux observations importantes et j'ai l'honneur de vous les adresser. Les eclipses forment la base de la chronologie, et celles cy pourront un jour servir a fixir l'epoque de l'independance de l'Amerique l'une des plus interressantes de l'histoire du genre humain. C'est un motif pour vous en dedier les observations, messieurs, et je rends cet hommage avec le plus grand plaisir a une societé illustre dont les membres sçavant en même temps eclaires leur patrie par leur connoisances dans les mathematiques et dans la phisique, et la servir utilement dans les conseils et dans les armées.

Je suis avec respect, Messieurs,

Votre tres humble et tres obeissant serviteur,

DE GRAUCHAIN, Major general de l'escadre Francoise.

A Newport le 5 Novembre, 1780.

An

An Obſervation of an Eclipſe of the Sun on the 27th of October, 1780, at Newport in the State of Rhode-Iſland.

	Time by the Clock.			True Time.		
	h.	′	″	h.	′	″
The time that the eclipſe was perceived to begin,	9	24	32	11	0	12*
The preceding limb of the ſun at the vertical,	11	21	39	0	57	27
The upper edge of the ſun at the horizontal,		21	54		57	42
The upper horn of the moon at the horizontal,		22	3		57	51
The edge of the moon at the vertical, - -		22	45		58	33
The upper horn at the vertical, - - -		23	7		58	55
The lower horn at the vertical, - - -		23	35		59	23†
The lower horn at the horizontal, - - -		29	31	1	5	20
The lower limb of the ſun at the horizontal, -		31	2		6	51‡
The preceding limb of the ſun at the vertical,	11	37	12	1	13	1
The upper limb ditto at the horizontal, - -		37	59		13	48
The upper horn ☽ at the horizontal, - -		38	52		14	21
The limb ditto at the vertical, - - -	not obſerved.					
The upper horn ditto at the vertical, - -		38	57		14	16
The lower horn ditto at the vertical, - - -		39	19		15	8
The lower horn ditto at the horizontal, - -		43	38		19	8
The lower limb of the ſun at the horizontal, -		45	27		21	17
The preceding limb ☉ at the vertical, - -	11	47	8	1	22	58
The upper edge of ditto at the horizontal, -		48	17		24	7
The limb of the ☽ at the vertical, - - -		49	0		24	50
The upper horn at the vertical, - - -		49	5		24	55
The upper horn at the horizontal, - -		47	7		24	57
The lower limb of ☉ at the horizontal, - -		55	2		30	52
The end of the Eclipſe, - - - -	12	4	50	1	40	41
The rate of the clock,	At Noon.					
October 21,	10	35	12,8			
24,	10	29	42,0			
25,	10	27	52,3			
27,	10	24	15,8			

* When the ſun was perceived to be indented, it was about 1′ 20″ after the eclipſe began, therefore the true time of beginning was at 10h. 58′ 52″.

† Uncertain.

‡ The ſuperior limb is called the inferior, &c. as the glaſs of the quadrant inverted the objects.

The latitude of the place of obſervation on Goat-Iſland, 41° 30′ 20″ N.

An

An Obfervation of the Eclipfe of the Moon on the 11th of Nov. 1780, at Newport in the State of Rhode-Ifland.

	Time by the Clock.			True Time.		
	h.	′	″	h.	′	″
The beginning of the Eclipfe, - - - -	7	40	5	10	24	39
Immerfion of Grimaldi begins, - - -	7	48	50	10	33	25
Ditto, - - - ends, - - -	7	51	25	10	36	10
Immerfion of Tycho begins, - - -	8	0	36	10	45	12
Ditto, - - ends, - - - - -	not obferved.					
The fhadow to Gallileo, - - - - -	8	3	42	10	48	18
Immerfion of Copernicus begins, - - -	8	27	54	11	12	31
Ditto, - - - ends, - - -	8	32	35	11	17	12
The fhadow in the middle of Dionyfius, - -	8	46	55	11	31	33
The fhadow of the Pointed Promontary, - -	8	55	42	11	40	21
Copernicus begins to appear, - - - -	9	17	54	12	2	34
Grimaldi - - ditto, - - - - -	9	22	2	12	6	42
Copernicus wholly appears, - - - - -	9	23	35	12	8	15
Grimaldi - ditto, - - - - -	9	26	45	12	11	26
The Pointed Promontary appears, - - -	9	51	12	12	35	55
Tycho wholly appears, - - - - -	10	10	6	12	54	51
The end of the Eclipfe, - - - - -	10	32	10	13	16	57
The rate of the Clock,	At Noon.					
November 11,	9	16	15,7			
12,	9	14	30,7			

Remarks upon the Obfervation of the Eclipfe of the Sun.

THE clock by which the time was obferved, is a pendulum one with a verge of compenfation, made by Mr. Barthond, a celebrated clock maker at Paris. It was regulated many days before and after the obfervation by correfponding altitudes taken with a quadrant of Ramfden, having a radius of one Englifh foot; it is a very good one and well graduated.

The fame quadrant ferved to obferve the time when the horns and limbs of the moon and fun arrived at the hori-

Eclairciffements fur l'obfervation de l'eclipfe de Soleil.

LA pendule dont on s'eft fervi pour obtenir l'heure eft une pendule a verge de compenfation faite par M. Barthond celebre horloger de Paris, elle a eté reglée plufieurs jours avant et apres l'obfervation par des hauteurs correfpondantes prifes avec un quart de cercle de Ramfden d'un pied anglois de rayon tres bon, et tres bien divifé.

Le même quart de cercle a fervi pour les obfervations des paffages des cornes et des bords du foleil et de la lune au fil horifontal, et au fil vertical de la lunette qui y eft adaptée.

L'obfervateur qui en embarquant des inftruments d'aftronomie n'avoit eu pour objet que de regler des montres marines, n'etoit pas auffi bien pourvu de lunettes que de pendules et de quart de horizontal and vertical threads of the glaſs which is fitted to the quadrant.

The obſerver who provided and ſhipped the inſtruments, had no other object in view but to rectify the clock belonging to the ſhip, which was the cauſe that he was not ſo well provided with teleſcopes as with clocks and quadrants, he was therefore obliged to make uſe of a ſimple achromatic ſea-glaſs of four feet focus, to obſerve the beginning and end of the eclipſe.

Yet he believes he can anſwer for the end of the eclipſe within about four or five ſeconds. The inſtant of time which it began is much more uncertain. The ſun was already indented when it was firſt perceived, but in order that he might eſtimate groſsly the true time of its beginning, he has eſtimated pretty nearly the diſtance of the horns, the moment when the ſun's limb was firſt perceived to be indented. By comparing the time elapſed after the end of the eclipſe to the inſtant when the diſtance of the horns of the moon were ſenſibly the ſame, hence he judged that it ſhould have been about 1′ 20″ from the true time in which the eclipſe began until the obſerved time.

At

de cercle, et il a eté obligé de ſe ſervir pour les obſervations du commencement et de la fin de l'eclipſe d'une ſimple lunette achromatic de mer de quatre pieds de ſoyer.

Cependant on croit pouvoir repondre de la fin de l'eclipſe a quatre ou cinq ſecondes pres; l'inſtant du commencement eſt beaucoup plus incertain; le ſoleil etoit deja conſiderablement entamé, lors qu'on s'en eſt appercu : pour conclure au moins groſſierement l'inſtant vrai du commencement de l'eclipſe, on a eſtime a peu pres quelle etoit la diſtance des cornes au moment ou on s'eſt apperçû que le diſque du ſoleil etoit entamé, et vers la fin de l'eclipſe on a examiné combien il s'eſt ecoulé de tems depuis l'inſtant ou la diſtance des cornes a eté ſenſiblement la même juſqu'á la fin de l'eclipſe. C'eſt de cette maniere que l'on a jugé qu'il devoit s'etre ecoulé environ 1′ 20″ depuis l'inſtant vrai du commencement de l'eclipſe, juſqûà celui ou on s'eſt apperçû, quelle etoit commencée.

On a dabord crû inutile de chercher a obſerver la grandeur de l'eclipſe avec le quart de cercle, a cauſe de la lenteur du movement des deux aſtres dans le ſens vertical, cependant a la reflexion on a penſé que les paſſages des cornes et des bords du ſoleil et de la lune au fil vertical ſeulement ſuffiroient pour donner les differences de hauteur et d'azimuth des centres des deux aſtres, et par conſequent leur difference de latitude et de longitude. On les a donc obſervès vers la fin de l'eclipſe, et en même tems on a obſervé les paſſages au fil horiſontal, mais ſans eſperer quils puſſent etre d'un grand ſecours pour calculer la diſtance des centres.

Dans la premiere obſervation le paſſage de la corne ſuperieure au fil vertical eſt un peu douteux. On penſe donc qu'il eſt a propos d'employer de preference dans le calcul de cette obſervation les paſſages du bord de la lune, et de la corne inferieure au même fil vertical. De cette maniere on connoitra immediatement les lignes N B et C O dont la premiere combinée avec les deux diametres L N et S T donnera L E; difference d'azimuth des centres des deux aſtres. On obtiendra auſſi facilement la difference apparente de hauteur S E des mêmes centres en calculant

At firſt it was thought uſeleſs to endeavour to obſerve the magnitude of the eclipſe with the quadrant, becauſe of the ſlow movement of the two planets vertically, yet upon reflection he thought that the paſſage of the horns and limb of the ſun and moon to the vertical thread only, would be ſufficient to give the differencies of the altitude and azimuth of the centers of the ſun and moon, and of courſe their difference of latitude and longitude. He then obſerved them towards the end of the eclipſe, and at the ſame time obſerved the time of their arrival at the horizontal thread, but without any expectation of their being of great ſervice to calculate the diſtance of the centers from.

In the firſt obſervation the paſſage of the upper horn to the vertical thread, is a little doubtful; at the time it was thought proper to give the preference to the paſſages of the limb and lower horn of the moon to the ſame vertical thread, in making the calculations from this obſervation; by this means we may find the lines NB and CO, the firſt of which combined with the two diameters LN and ST will give LE, the difference of azimuths and of the centers of the two planets; the apparent difference of the altitude from the ſame centers may be eaſily obtained, by calculating SF and CM, ſides of the right-angled triangles CSF and CLM, in which are known the other two ſides.

Plate III. Figure 18.

In

culant S F et C M cotes des triangles rectangles C S F et C L M dans les quels on connoit deja les deux autres cotés.

Dans la ſeconde obſervation on a obmis par diſtraction, l'inſtant du paſſage du bord de la lune au fil vertical, on ſe ſervira donc pour la calculer des paſſages des cornes au même fil les quels donneront immediatement C A et C O d'ou ſouſtrayant S T, on aura C D et C F connoiſſant ces deux lignes et le demi-diametre du ſoleil on calculera les angles C S D, C S F ſouſtrayant leur ſomme de 180°, on aura l'angle C S L partageant cet angle par la moitié on aura l'angle C S L du triangle C L S, on connoitra donc aiſement S L coté de cetriangle et du triangle S L E calculant enfin ce dernier triangle S L E dans le quel on connoit deux angles et un côtè on obtiendra L E; difference d'azumuth et S E; difference apparente de hauteur des centres des deux aſtres.

La derniere obſervation etant plus complette on pourra la calculer indifferement de l'une ou l'autre maniere. On pourra même faire uſage du paſſage au fil horiſontal pour conclure la difference de hauteur attendu que le movement des deux aſtres dans le ſens vertical commencoit a devenir moins lent lorſque cette obſervation a eté faite.

Si

In the ſecond obſervation we obtain the inſtant of the arrival of the limb of the moon to the vertical thread, which may be made uſe of to calculate the paſſage of the horns by the ſame thread, by which C A and C O are obtained: By ſubtracting S T from theſe, the remainder will be C D and C F, having theſe two lines and the ſemi-diameter of the ſun, the angles C S D, C S F, may be had, and ſubtracting their ſum from 180° the remainder will be the angle C S C, the half of which is the angle C S L; from the triangle C S L you may readily obtain S L, a ſide of this triangle, and of the triangle S L E; from theſe at length this laſt triangle S L E may be calculated, in which are had two angles and one ſide, from which may be obtained L E the difference of azimuth, and S E the apparent difference of altitude of the centers of the ſun and moon.

My laſt obſervation being more complete, might be calculated either from the one or the other method, and the paſſage to the horizontal thread might even be uſed to determine the difference of the altitude of the centers, as the motion of theſe planets with reſpect to the vertical, began to be quicker when this obſervation was made.

If we are deſirous to aſcertain the time of the paſſage of the upper horn of the moon to the vertical thread in the firſt obſervation, or to know in the ſecond obſervation the time of the paſſage of the limb of the moon by the ſame thread, they may eaſily be calculated by the help of the quantities already found. Perhaps it might be uſeful to make this calculation, to determine the variation which ought to have place in the poſition of the two points obſerved, relatively

Si l'on vouloit dans la premiere obſervation veriffier l'inſtant du paſſage de la corne ſuperieure au fil vertical, ou connoitre dans la ſeconde obſervation l'inſtant du paſſage du bord de la lune au même fil on pourroit aiſement les calculer avec le ſocours des quantites deja connues. Il ſera peutetre même utile de faire ce calcul pour ſe mettre en etat de determiner la variation qui a du avoir lieu dans la poſition des deux points obſervér relativement l'un a l'autre, pendant l'eſpace de tems ecoulé entre leurs paſſages au même fil.

Obſervation

relatively to each other during the time elapsed between their passage to the same thread.

Observation de l'eclipse de Lune.

ON a fait usage dans cette observation de la même quart de cercle, et de la même lunette qui avoient servi pour l'observation de l'eclipse de soleil. La marche de la pendule etoit cependant un peu differente parce qu'on y avoit touché.

N° XXXII.

An Account of the Transit of Venus over the Sun, June 3d, 1769, as observed at Newbury, in Massachusetts; by the Rev. SAMUEL WILLIAMS, A. M.

THE transit of Venus over the sun, being one of the most uncommon and useful phenomena in astronomy, I determined to make as careful an observation of it as I could. Early in May I received an invitation from *Tristram Dalton*, Esq. a gentleman of Newbury-Port, to observe it with him. He had a seat at Newbury, in a high elevated situation, very convenient for this purpose, at which we agreed to make the observation. The weather for several days had been dull and rainy, but clearing up on Tuesday evening I went early on Wednesday to put every thing in readiness. The regulation of our clock being an article of great importance, I was very careful to have it thoroughly examined, and well fitted up. To adjust it to apparent time we took corresponding altitudes of the sun, both before and on the day of the transit. In these observations, it was easy to arrive to a pretty great exactness; and as they were very numerous, the going of the clock was well ascertained by them, and found to be steady and regular. The telescope we had prepared was a reflector made by *Nairne*, magnifying about 55 times; a good instrument, but not fitted with a micrometer,

micrometer, or with vertical and horizontal hairs, as we could have wifhed.

The third of June proved favourable to our wifhes. The air was uncommonly clear, and the fky ferene. About twenty minutes before the tranfit, I began to keep my eye fteadily fixed on that part of the fun's limb, on which the planet by calculation was to enter; an affiftant counting the clock in the mean time, while another ftood by to write down the obfervations. Thus prepared, we waited with a kind of agreeable anxiety for the high fatisfaction of feeing Venus on the fun; a fatisfaction I had once before enjoyed in viewing the tranfit of 1761*, and which I knew muft end with that of 1769! The firft impreffion of Venus on the fun, I expected would not appear like a diftinct well defined black fpot coming on as it were in an inftant, but rather like an ill defined mixture of limbs. The event was agreeable to the conjecture, for at 2^h $30'$ $14''$, apparent time, I imagined I faw *a fmall difturbance* on the fun's limb; but the impreffion was then fo fmall, irregular and ill defined, that it was not till after feveral feconds that I was certain the tranfit was begun. But the impreffion increafing and growing more diftinct, I fixed on the time mentioned above as the time of the *external contact*. To obfervers with telefcopes and eyes equally good, and fixed on that part of the fun on which the planet entered, I conceive this firft impreffion might have been obferved to an agreement of 5 or 6 feconds. Though perhaps it might be the contact of the atmofphere, rather than of the body of Venus with the fun.

In about ten minutes after the *external*, I began to look for the *internal contact*. From the form in which Venus appeared, being furrounded with a glimmering light, not very diftinctly defined, I concluded it would be difficult if not impoffible to fix upon the precife moment when her

Ii 2 limb

* At St. *John's*, in *Newfoundland*.

limb would be exactly coincident with that of the fun; and therefore determined to wait till there fhould appear a fmall thread of light between them. As the contact drew near, the thread of light began to form, and feemed to dart on each fide of the planet for feveral feconds without being fixed or fettled. At $2^h\ 48'\ 44''$, with a feeming uncertainty of not more than $7''$ it became clofed and fixed; Venus then appeared wholly within the fun, feparated from its limb by a fine ftream of light flowing gently round it. This I fixed upon as the *internal contact*, though this might alfo be the contact not of Venus but of her atmofphere with the fun. Not having a *micrometer* or *hairs* fixed in the reflector, inftead of making any further obfervations, we could only enjoy the pleafure of viewing this curious phenomenon, and fhowing it to a number of gentlemen that had affembled on the occafion.

To determine the latitude of the place, we took the meridian altitude of the fun on the day of the tranfit, by tranfmitting his rays from a ftyle 10 feet high, upon a large horizontal platform. From this obfervation our latitude came out $43°\ 2'$ north. Sufpecting the obfervation was not fufficiently accurate, I have fince carefully examined the matter, and from feveral obfervations which nearly agree, I find it to be but $42°\ 57'$. With regard to our longitude, the mean of fix or feven obfervations of the eclipfes of Jupiter's firft fatellite, gives it about $4^h\ 42'\ 30''$ weft from *Greenwich*.

In the above account of the *contacts*, the duration of the ingrefs, or paffage of Venus over the fun's limb, is $18'\ 30''$; near a minute longer than in moft of the *American* obfervations. By theory it fhould be $18'\ 56''$, but as this muft have been contracted at the place of obfervation, $15''$, by parallax, the apparent duration of the ingrefs, would be but $18'\ 41''$; that is, $11''$ longer than it was made by obfervation. I much doubt whether it was poffible to difcern the planet fo foon as $11''$ after the firft first contact, when not a second of its diameter had entered upon the sun. It is most probable that the *internal contact* was past before the thread of light appeared to me to be compleated. It seems as though something of the same kind, must also have been the case in most of the *European* observations; as they make the ingress near a minute longer, than it was seen by most of the *American* observers, when by theory it must rather have been shorter. But the different appearances of Venus, different ideas of the contacts, with the unavoidable difference of eyes, telescopes, the state of the atmosphere and the like, might easily occasion such differences in the observations. Though in the same circumstances, it can hardly be thought but that the *European* and the *American* observations would have more nearly agreed.

An Account of the Transit of Mercury over the Sun, November 9th, 1769, as observed at Salem, in Massachusetts; by the Rev. SAMUEL WILLIAMS, A. M.

THE transits of Mercury, though they are not of equal use in astronomy with those of Venus, are yet of great advantage to perfect the elements of his theory, and to determine the longitude of places on the earth. I had an opportunity to observe one of these transits, November 9, 1769, in company with *Andrew Oliver*, Esq. at *Salem*. Mr. Oliver had a good reflector, magnifying about sixty times. But his clock not being in so good order as was to be wished, and not having any instrument to take altitudes, I was obliged to have recourse to the following method to determine the time. The day before the transit I drew a meridian line, with which I examined the going of the town clock on the day of the transit, and on the day after, and found it had kept time very well. Comparing my watch with the clock, the time was pointed out to minutes pretty exactly. Taking the minutes from

from the watch, I endeavoured to count the ſeconds, which by a perſon uſed to it may be done pretty near the truth. This method of determining the time, though ſuch as an aſtronomer would by no means chuſe, was the only one that I could make uſe of; and from the pains I took to be exact, I believe it might be depended upon to eight or ten ſeconds.

At the *firſt contact* I expected *Mercury* would have appeared as Venus had done, ſomething irregular, uneven, and not very diſtinctly defined. But at $2^h\ 54'\ 40''$ apparent time, I was agreeably diſappointed by ſeeing the planet come on as it were in an inſtant, in the form of a clear, regular, well defined black ſpot. The *internal contact* was equally inſtantaneous; at $2^h\ 56'\ 0''$ the thread of light cloſed to appearance in a moment, without a ſeeming uncertainty of a ſecond. The ſky being perfectly clear and ſerene, nothing could be better defined than the limbs of Mercury and the ſun. There was no appearance of any thing like an atmoſphere round the planet, but all the time the ſun was viſible, Mercury appeared like a ſteady diſtinct black ſpot, much leſs than ſome that were then upon the ſun. Not having a micrometer, it was not in our power to make any further obſervations, either on the diameter of the ſun or Mercury, or of the leaſt diſtance of their limbs.

An Obſervation of an Eclipſe of the Sun, November 6th, 1771, *at Bradford, in Maſſachuſetts; by the Rev.* SAMUEL WILLIAMS, A. M.

FROM the beginning of the year 1769 till the end of 1771, there were but two eclipſes that could be obſerved at *Bradford*. One of theſe was a total eclipſe of the moon, June 19th, 1769; of this I had no obſervation, being prevented by an indiſpoſition. The other was an eclipſe of the ſun, November 6th, 1771. The weather for ſeveral

ral days before, was ſo cloudy that I attempted in vain to regulate my clock, though I watched every favourable opportunity. On the day of the eclipſe I got it pretty well adjuſted by ſeveral correſponding altitudes of the ſun. About 1^h P. M. the clouds gathered ſo much round the ſun, that I was apprehenſive they would prevent any obſervation. But being pretty much ſcattered, at $1^h\ 36'\ 42''$ apparent time, I could very plainly perceive that the eclipſe was juſt begun. This I judged was very near the beginning, if not exactly ſo, though it was attended with ſome uncertainty. In a few minutes the ſun was wholly covered with the clouds, and remained thus till $3\frac{1}{2}^h$, when they began again to ſcatter, and left that part of the heavens in which the ſun appeared, perfectly clear. The weather continued thus till the end of the eclipſe, which by a good obſervation was at $3^h\ 47'\ 2''$. Theſe obſervations were made with a reflector made by *Nairne*, magnifying as near as I could judge about ſixty times; but as to the *quantity* of the eclipſe, no obſervation could be made, the ſun being obſcured by the clouds the biggeſt part of the time.

N° XXXIII.

An eaſy and accurate Method of finding a true Meridian Line, and thence the Variation of the Compaſs. *By* ROBERT PATTERSON.

Read Apr. 7, 1786.

OF the various methods which aſtronomers employ for finding a true meridian line, none ſeems ſo well adapted, as could be wiſhed, to the common uſe of ſurveyors, in finding the variation of the Compaſs.

To find the azimuth of the ſun by a ſingle obſervation of his altitude, beſides a quadrant which is neceſſary ſo

thi.

this purpofe, requires the previous knowledge either of the latitude of the place, or hour of the day, at the time of obfervation; neither of which can, by the common apparatus of a furveyor, be found with fufficient accuracy.

The fun's azimuth may, it is true, be found without knowing either the latitude of the place or hour of the day, by taking equal altitudes before and after noon; but this requires time, attention and inftruments, which furveyors can but feldom command.

That method, which is perhaps the moft exact, viz. meafuring the time between the paffage of two ftars which differ confiderably, in declination and but little in right afcenfion, over the fame vertical circle, is ftill farther out of the reach of common furveyors.

The following table of the pole ftar will, it is prefumed, furnifh a more eafy, and yet fufficiently accurate method of determining this problem; free from all the above inconveniencies, and requiring no difficult calculation, nor any other inftrument than the common theodolite, or circumferentor. For though the latitude of the place fhould not be known within a whole degree, nor the hour of the night within 2 or 3 minutes, this table, by a fingle obfervation of the magnetic azimuth or bearing of the pole ftar, will generally give the variation of the needle true to a fingle minute of a degree. Nay if the obfervation be made (as it may be every night) when the ftar is near its greateft elongation, an error of 10, or even 20 minutes in time will, as is plain from the table, produce little or no fenfible error in the azimuth. And as thefe obfervations may be repeated at pleafure during the night, and a mean of all taken, the variation may, by this means, be found to any degree of accuracy that can be defired. Befides, the needle is not at this time affected with any diurnal variation; which in the day-time is very uncertain, and frequently amounts to more than one quarter of a degree.

The

The beſt inſtrument for obſerving the ſtar's magnetic azimuth is a theodolite, furniſhed with ſpirit-levels, and a ſmall teleſcope with a perpendicular wire. A common circumferentor may, however, anſwer the purpoſe. When this inſtrument is uſed, a fine thread or hair muſt be ſtretched along from the top of one ſight to that of the other, directly over the center of the compaſs; and the obſerver muſt be very careful to place the ſights perpendicular to the horizon when he makes the obſervation; for this purpoſe a ſmall pocket ſpirit-level, in the form of a carpenter's ſquare, would be very convenient.

By the common circumferentor we cannot, indeed, take the bearing of an object with very minute accuracy; for though the eye can very well judge of the coincidence of two lines, or of the point of the needle with any whole degree on the compaſs, yet the parts of a degree cannot readily be obſerved to greater exactneſs than one third or one fourth of the whole. This inconvenience may, however, be eaſily remedied, and at a very trifling expence, in the following manner.

Let one of the ſights, by means of a ſcrew, be made movable at right angles to the index; and on the end of the index, cloſe to the movable ſight, ſet off, on each ſide of the central line, the tangent of three degrees to a radius equal to the whole length of the index, or diſtance between the two ſights. Let each of theſe degrees be divided into ſix equal parts; then will a nonius diviſion on the ſight, where ten equal parts muſt correſpond with eleven on the index, ſubdivide theſe parts into minutes of a degree.

It will be unneceſſary to make the ſight move in the arch of a circle, the difference between this and the tangent, in ſo ſmall an arch, being quite imperceptible. With this ſimple improvement the common circumferentor will take the bearing of an object true to a minute, thus: Let the end of the needle be made exactly to coincide with the neareſt whole degree, then move the ſcrew till the object appears in the direction of the ſights, and the nonius on the movable ſight will point out the odd minutes.

Explanation and Uſe of the TABLE.

The left hand double column of the table contains the time before the ſtar's paſſage over the meridian above the pole, for every twenty minutes of its whole diurnal circuit. The firſt column, under each particular latitude, ſhews the azimuth of the ſtar at theſe times, reſpectively, in degrees, minutes and tenths of a minute. The ſecond column ſhews the difference of azimuth in every twenty minutes of intermediate time, in minutes and tenths.

To find the true azimuth of the ſtar in any latitude, at any given time.

From the ſtar's right aſcenſion, viz. $0^h\ 49^m$, increaſed by 24^h if neceſſary, ſubtract the right aſcenſion of the ſun computed to the time of the ſtar's paſſage over the meridian, above the pole, nearly, the remainder will be the time of ſaid paſſage, reckoned from noon. From which, increaſed by 24^h if neceſſary, ſubtract the time of the obſervation, reckoned alſo from noon, the remainder will ſhew the time before the ſtar comes to the ſaid meridian. Look for this time in the left hand column of the table, oppoſite to which in the column of azimuth, under the proper latitude, you will have the true azimuth of the ſtar at that time.

If the time before the ſtar comes to the meridian be leſs than 12 hours, its azimuth will be eaſterly; but if more than 12 hours, its azimuth will be weſterly.

If the magnetic azimuth, and the true azimuth at the time of the obſervation, be both eaſterly or both weſterly, their difference will be the variation of the needle. But if one be eaſterly and the other weſterly, their ſum will be be the variation. And if the magnetic be to the weftward of the true azimuth, the variation will be wefterly; but if to the eaftward, the variation will be eafterly.

If the time before the ftar's paffage over the meridian be fome intermediate minute, or the latitude of the place fome intermediate degree, not found in the table, a proportional intermediate azimuth, by means of the differences, muft be taken.

The right afcenfion of the pole ftar annually increafes 10 feconds of time, and its polar diftance decreafes 20 feconds of a degree, therefore to its prefent right afcenfion (in 1785,) viz. $0^h\ 49^m$, muft be added one minute every year; and from its prefent polar diftance ($1°\ 50'.5$) one minute muft be fubtracted, and a proportional part from all the numbers in the columns of azimuth, every three years. The effect of aberration and nutation may be fafely neglected; as the error arifing from thefe caufes can never amount to more than half a minute of a degree in azimuth.

In computing the fun's right afcenfion to the time of the ftar's paffage over the meridian nearly, the following little table will be ufeful.

 TABLE.

TABLE.

Time.		Star paſſes Meridian nearly at
April	2	Noon
	19	11 A. M.
May	5	10
	20	9
June	4	8
	18	7
July	3	6
	17	5
Auguſt	2	4
	17	3
September	3	2
	19	1
October	6	Midnight
	22	11 P. M.
November	6	10
	21	9
December	5	8
	19	7
January	1	6
	15	5
	29	4
February	13	3
March	1	2
	17	1

EXAMPLE I.

Suppoſe on the 12th of September 1785, at 8 o'clock in the evening, in the latitude of 40° N. the magnetic azimuth of the pole-ſtar had been obſerved to be 0° 38′ eaſterly; required the variation of the needle at the time and place of obſervation.

Star's

	H.	M.
Star's R. A. increafed by 24 hours, -	24	49
Sun's R. A. computed to 1ʰ A. M. (taken from the nautical almanac, or any other table of the fun's R. A.) fubtract -	11	25
True time of ftar's paffage over meridian, reckoned from noon, - -	13	24
Hour of the night fubtract, - -	8	0
Time before ftar's paffage, - -	5	24

Which correfponds to true azimuth, 2° 23′ E.
Magnetic azimuth, - - 0 38 E.

Variation of the needle, - 1 45 W.

Example II.

In the latitude of about 32° north, on the 4th of July 1785, at 48 minutes after 10 o'clock at night, fuppofe the magnetic azimuth of the pole ftar, to be 2° 40′ eaft; required the variation of the needle.

	H.	M.
Star's R. A. + 24ʰ, - - -	24	49
Sun's R. A. - - - - -	6	54
Time of ftar's paffing meridian, -	17	55
Time of obfervation, - - -	10	48
Time before ftar comes to meridian, -	7	7

Which correfponds to true azimuth, 2° 4′ E.
Magnetic azimuth, - - 2 40 E.

Variation, - - 0 38 E.

Ex-

EXAMPLE III.

Latitude of the place 42° north, time of obſervation, January 17th. 1785, at 2^h 40^m A. M. Magnetic azimuth, 1° 5′ eaſterly.

	H.	M.
Star's R. A. + 24^h - - -	24	49
Sun's R. A. - - - -	20	1
Time of ſtar's paſſing the meridian, -	4	48
	24	
	28	48
Time of obſervation reckoned from noon,	14	40
Time before ſtar comes to meridian, -	14	8

Correſponding to true azimuth,	1° 16′ W.
Magnetic azimuth, - -	1 5 E.
Variation, - - -	2 21 E.

A TABLE

A TABLE of the Azimuth of the Pole-ſtar for every 20 Minutes of its diurnal Motion round the Pole.

Time before the ſtar comes to the meridian above the pole.				Latitude 30°		Latitude 35°		Latitude 40°		Latitude 45°		Latitude 50°		Latitude 55°	
H.	M.	H.	M.	Star's azimuth. ° ′	Diff. ′	Star's azimuth. ° ′	Diff. ′	Star's azimuth. ° ′	Diff. ′	Star's azimuth. ° ′	Diff. ′	Star's azimuth. ° ′	Diff. ′	Star's azimuth. ° ′	Diff. ′
0	0	24	0	0 0.0	11.3	0 0.0	12.0	0 0.0	12.9	0 0.0	14.1	0 0.0	15.6	0 0.0	17.6
	20	23	40	11.3	11.3	12.0	12.0	12.9	12.8	14.1	13.9	15.6	15.4	17.6	17.4
	40		20	22.6	11.1	24.0	11.7	25.7	12.6	28.0	13.7	31.0	15.2	35.0	17.2
1	0		0	33.7	10.7	35.7	11.5	38.3	12.3	41.7	13.4	46.2	14.8	52.2	16.7
	20	22	40	44.4	10.4	47.2	11.0	50.6	11.9	55.1	12.9	1 1.0	14.3	1 8.9	16.0
	40		20	54.8	10.0	58.2	10.6	1 2.5	11.3	1 8.0	12.3	15.3	13.6	24.9	15.4
2	0		0	1 4.8	9.5	1 8.8	10.0	13.8	10.9	20.3	11.8	28.9	12.9	40.3	14.5
	20	21	40	14.3	8.9	18.8	9.4	24.7	10.0	32.1	10.9	41.8	12.0	54.8	13.5
	40		20	23.2	8.2	28.2	8.8	34.7	9.3	43.0	10.1	53.8	11.2	2 8.3	12.5
3	0		0	31.4	7.6	37.0	7.8	44.0	8.5	53.1	9.1	2 5.0	10.0	20.8	11.2
	20	20	40	39.0	6.7	44.8	7.1	52.5	7.5	2 2.2	8.2	15.0	9.0	32.0	10.0
	40		20	45.7	5.9	51.9	6.3	2 0.0	6.7	10.4	7.2	24.0	7.7	42.0	8.7
4	0		0	51.6	5.0	58.2	5.2	6.7	5.5	17.6	6.0	31.7	6.6	50.7	7.3
	20	19	40	56.6	4.1	2 3.4	4.3	12.2	4.5	23.6	4.9	38.3	5.4	58.0	6.6
	40		20	2 0.7	3.1	7.7	3.3	16.7	3.6	28.5	3.7	43.7	4.0	3 4.6	3.6
5	0		0	3.8	2.2	11.0	2.3	20.3	2.4	32.2	2.5	47.7	2.6	8.2	3.0
	20	18	40	6.0	1.3	13.3	1.4	22.7	1.3	34.7	1.4	50.3	1.4	11.2	1.4
	40		20	7.3		14.7		24.0		36.1		51.7		12.6	
6	0		0	7.6	.7	14.8	.6	24.2	.9	36.2	1.0	51.8	1.1	12.5	1.5
	20	17	40	6.9	1.7	14.2	1.9	23.3	2.0	35.2	2.2	50.7	2.5	11.0	2.9
	40		20	5.2	2.5	12.3	2.8	21.3	3.0	33.0	3.3	48.2	3.9	8.1	4.3
7	0		0	2.7	3.5	9.5	3.8	18.3	4.1	29.7	4.5	44.3	4.9	3.8	5.6
	20	16	40	1 59.2	4.5	5.7	4.6	14.2	5.0	25.2	5.5	39.4	6.2	2 58.2	7.0
	40		20	54.7	5.1	1.1	5.6	9.2	6.0	19.7	6.5	33.2	7.1	51.2	8.2
8	0		0	49.6	6.2	1 55.5	6.4	3.2	6.9	13.2	7.5	26.1	8.4	43.0	9.3
	20	15	40	43.4	6.8	49.1	7.3	1 56.3	7.6	5.7	8.5	17.7	9.2	33.7	10.4
	40		20	36.6	7.5	41.8	7.9	48.7	8.5	1 57.2	9.2	8.5	10.2	23.3	11.5
9	0		0	29.1	8.2	33.9	8.7	40.2	9.4	48.0	9.9	1 58.3	11.0	11.8	12.1
	20	14	40	20.9	8.8	25.2	9.2	30.8	9.9	38.1	10.8	47.3	11.7	1 59.7	13.2
	40		20	12.1	9.3	16.0	9.8	20.9	10.4	27.3	11.3	35.6	12.4	46.5	13.8
10	0		0	2.8	9.7	6.2	10.3	10.5	11.0	16.0	11.8	23.2	12.9	32.7	14.5
	20	13	40	0 53.1	10.2	0 55.9	10.7	0 59.5	11.4	4.2	12.3	10.3	13.4	18.2	14.9
	40		20	42.9	10.5	45.2	11.0	48.1	11.7	0 51.9	12.7	0 56.9	14.0	3.3	15.6
11	0		0	32.4	10.7	34.2	11.3	36.4	12.0	39.2	12.9	42.9	14.2	0 47.7	15.7
	20	12	40	21.7	10.8	22.9	11.4	24.4	12.2	26.3	13.1	28.7	14.3	32.0	15.9
	40		20	10.9	10.9	11.5	11.5	12.2	12.2	13.2	13.2	14.4	14.4	16.1	16.1
12	0		0	0.0		0.0		0.0		0.0		0.0		0.0	

N° XXXIV.

Astronomical Observations, communicated by Mr. RITTENHOUSE.

By Mr. *James Six*, of Canterbury.

Geocentric place of the New Planet.

April 1st, 1782,	29°	5′	30″	in ♊ lat. 13′ N.
October 15th,	7	21	18	♋ nearly stationary.
December 26th,	5	2	30	in opposition.
March 10th, 1783,	3	15	0	stationary.
October 15th,	11	53	10	stationary.
December 30th,	9	47	25	in opposition.
March 14th, 1784,	7	46	0	stationary.

Lat. 23 N.

By *D. Rittenhouse*, at Philadelphia.

Transit of Mercury over the sun's disk, Nov. 12th, 1782.

First external contact,	9h	34′	50″	morn.	Mean Time.
Internal, uncertain,		40	0		
Second internal,	10	51	30		
Last contact,		57	35		

Greatest distance of ☿ center from sun's limb, 31″.

1784,		On Meridian.		
Jan. 29th.	γ Gemino	9h	49′	20″
	ε Gem.	9	54	45
	New Planet	10	1	48
	ζ Gem.	10	15	19
Feb. 12th.	γ Gem.	8h	54′	19″
	New Planet,	9	4	54
	ζ Gem.	9	20	18

1785.		On Meridian.							
January 26th.	ζ Gemino	10h	24′	14″					
	New Planet	10	31	22					
	δ Gem.	10	40	7					
February 4th.	ζ Gem.	9	48	48					
	New Planet	9	54	32					
	δ Gem.	10	4	41					
Feb. 12th. Observed zenith distance,	ζ Gemino					19°	5′	7″	
	New Planet					16	40	45	
	δ Gem.					17	35	33	
Febr. 17th.	ζ Gem.	8h	57′	42″					
	New Planet	9	1	46	zenith distance,	16°	40′	42″	
	δ Gem.	9	13	35	ditto,	17	35	40	
Febr. 27th.	ζ Gem.	8	18	34					
	New Planet	8	21	32					
	δ Gem.	8	34	27					
March 12th.	ζ Gem.	7	27	14					
	New Planet	7	29	41½					
	δ Gem.	7	43	6½					
March 15th.	Sirius	6	59	51					
	ζ Gemino	7	15	30					
	New Planet	7	17	54					
	δ Gem.	7	31	23					
March 17th.	Sirius	6	51	58					
	ζ Gem.	7	7	37					
	New Planet	7	9	59					
March 22d.	Sirius	6	32	18					
	ζ Gem.	6	47	56					
	New Planet	6	50	19					
March 27th.	Sirius	6	12	38					
	ζ Gem.	6	28	16					
	New Planet	6	30	45					
28th.	ζ Gem.	6	24	19					
	New Planet	6	26	50½					
1786,					Micrometer measure of Z. D.				Difference in min. & seco.
January 25th.	δ Gemino	10h	45′	1″					
	New Planet	10	56	48					
						r.	d.		
26th.	δ Gem.	10	41	5	+	1	35		
	New Planet	10	52	44	—	+	37½		
					Difference,	6	24½	=	20′ 59″

1786,		On Meridian.				Micrometer measure of Z. D. r.	d.		Difference in min. & seco.
January 27th.	δ Gemino	10h	37′	8″	+	1	34		
	New Planet	10	48	37	—	4	34		
					Diff.	6	29	=	21′ 17″
31st.	δ Gem.	10	21	24					
	New Planet	10	32	13					
February 1st.	δ Gem.	10	17	28	Diff.	7	0	=	22 33
	Pluto	10	28	7					
2d.	μ Gem.	9	16	23½					
	δ Gem.	10	13	33		8	40		
	New Planet	10	24	2		16	0		
					Diff.	7	8	=	23 5
3d.	δ Gem.	-	-	-	-	8	39		
	New Planet	-	-	-		16	3½		
					Diff.	7	12½	=	23 24
6th.	δ Gem.	9	57	50		8	39¾		
	New Planet	10	7	43		16	17		
					Diff.	7	25¼	=	24 7
10th.	μ Gem.	-	-	-	-	13	21½		
	δ Gem.	9	42	7					
	New Planet	9	51	24		16	29½		
					Diff.	3	8	=	10 13
21st.	δ Gem.	8	58	52					
	New Planet	9	6	46					
March 10th.	δ Gem.	7	52	2		12	5½		
	New Planet	7	58	31		21	21		
					Diff.	9	15½	=	30 5
11th.	μ Gem.	6	50	55					
	δ Gem.	7	48	5		12	5		
	New Planet	7	54	31		21	20¾		
					Diff.	9	15¾	=	30 4
12th.	δ Gem.	7	44	9		13	17½		
	New Planet	7	50	32		22	34½		
					Diff.	9	17	=	30 9
18th.	μ Gem.	6	23	23					
	δ Gem.	7	20	33		13	16¾		
	New Planet	7	26	43½		22	35½		
					Diff.	9	18¾	=	30 16

1786,		On Meridian.				Micrometer measure of Z. D.		Difference in min. & seco.
						r.	d.	
March 19th.	δ Gem.	7	16	37½		14	36	
	New Planet	7	22	47½		24	8¼	
					Diff.	9	20¾	= 30 24
21st.	δ Gem.	7	8	46		5	18	
	New Planet	7	14	53		14	37¼	
					Diff.	9	19¼	= 30 18
23d.	δ Gem.	7	0	54½		3	28½	
	New Planet	7	7	1½		12	47	
					Diff.	9	18½	= 30 15
24th.	δ Gem.	6	56	59		8	14¾	
	New Planet	7	3	6		17	34	
					Diff.	9	19¼	= 30 18
25th.	δ Gem.	6	53	3				
	New Planet	6	59	16				
27th.	δ Gem.	6	45	11		8	13¾	
	New Planet	6	51	18		17	31	
					Diff.	9	17¼	= 30 10
April 2d.	δ Gem.	6	21	36				
	New Planet	6	27	53				

N. B. In thefe obfervations the declination of the New Planet was conftantly greater than that of δ Geminorum, but lefs than μ.

N° XXXV.

A Letter from Mr. OTTO, *to* Dr. FRANKLIN, *with a Memoir on the Difcovery of America.*

New-York, 1ft April, 1786.

SIR,

Read Apr. 7, 1786.

ALMOST all the authors who have written upon the difcovery of America, make mention of fome information which Chriftopher Columbus procured at Madeira, upon the exiftence of a weftern continent; but they do not tell us, pofitively, how far this in-

formation

formation aſſiſted him, or from what ſource he derived it. I have always been curious to clear up this intereſting part of hiſtory; and in running over many ancient hiſtorians, as well German as Spaniſh, I have found ſome circumſtances, which have appeared to me to eſtabliſh, in the cleareſt manner, a diſcovery anterior to that of Columbus. I have the honor to ſend you the reſult of my enquiries; and if you think this piece worthy of being ſubmitted to the conſideration of the Philoſophical Society, I beg you to preſent it to them as a mark of my homage, and of the deſire which I have of being of ſome ſervice.

I have the honor to be, with reſpectful attachment,

Your excellency's very humble and

Moſt obedient ſervant,

OTTO.

His excellency Dr. Franklin.

A New-York, le 1 Avril, 1786.

MONSIEUR,

PRESQUE tous les auteurs qui ont ecrit ſur la decouverte de l'Amerique, ſont mention de quelques renſeignemens que Chriſtophe Colomb s'eſt procurés dans l'iſle de Madere ſur l'exiſtence d'un continent occidental, mais ils ne nous diſent pas poſitivement juſqu' à quel point ces relations ont pu lui étre utiles, ou quelle en a été la ſource. J'ai toujours été curieux de debrouiller cette partie intéreſſante de l'hiſtoire ; et en parcourant pluſieurs anciens hiſtoriens, tant Allemans qu'Eſpagnols, j'ai trouvé quelques détails qui m'ont paru établir d'une maniere indubitable une decouverte anterieure à celle de Colomb. J'ai l'honneur de vous en adreſſer le reſumé, et ſi vous croyés que cette piece ſoit digne d'être miſe ſous les yeux de la ſocieté philoſophique, je vous ſupplie de la lui preſenter comme une marque de mon hommage et du deſir que j'ai de lui être de quelqué utilité.

J'ai l'honneur d'être avec un reſpectueux attachement,

MONSIEUR,

De votre Excellence,

Le très humble et très obeiſſant ſerviteur,

OTTO.

S. E. M. FRANKLIN.

Memoir upon the Difcovery of America.

IT has always been looked on 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 were the firft difcoverers of this immenfe country, and that this honor 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.

I fhall not here enter into an examination of the reveries of fome hiftorians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Madoc prince of Wales and fon of Owen Guinnedd, of which Hackluyt has preferved fome account, nor on the voyages of Bacchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or falfe, cannot leffen the glory of Columbus, were there not proof that he received, juft before

MEMOIRE SUR LA DECOUVERTE DE L'AMERIQUE. MARS 1786.

ON a regardé jufqu'ici comme une injuftice qu'on n'ait point donnè le nom de Chriftophe Colomb à la belle partie du monde qu'il a decouverte, et qu'Americ Vefpuce, qui n'a fait que marcher fur fes traces, ait eu le bonheur de faire paffer fon fouvenir á la pofterité la plus éloignée au prejudice de fon prédéceffeur. Que diroit 'on s'il étoit prouvé qu'aucun de ces grands navigateurs n'a le merite de la premiere decouverte de ce pays immenfe, et que l'honneur en eft du à un homme prefqu'inconnu dans la republique des lettres? C'eft ce que je me propofe de faire dans ce mémoire, et fi l'obfcurité des écrivains contemporains et l'eloignement des époques ne me permettent pas de pouffer mes argumens jufquà l'évidence, ils fuffiront au moins pour établir des doutes fondés fur la prétendue decouverte de Chriftophe Colomb.

Nous n'examinerons point ici les réves de quelques hiftoriens fur la navigation des Carthaginois, fur l'Atlantide de Platon, fur les expeditions hardies de Madox, prince de Galles, et fils d'Owen Guyneth, dont Hakluit nous a conferve les détails, ni fur les voyages de Bachus ni fur l'Ophir de Salomon; ces conjectures vraies ou fauffes ne fauroient diminuer la glorie de Chriftophe 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 honor 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 honor 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

Chriſtophe Colomb, s'il n'étoit prouvé, que, peu de tems avant ſon expédition, un aſtronome ſavant avoit été en Amérique, et lui avoit communiqué ſes cartes et ſes journaux.

Garcilaſſo de la Vega, né à Cuſco en Perou, nous a donné une hiſtoire de ſa patrie dans laquelle, pour oter a Colomb le merite de la decouverte de l'Amerique, et pour en faire honneur aux Eſpagnols, il aſſure que ce navigateur avoit été inſtruit de l'exiſtence d'un autre continent par Alonzo Sanchez de Huelva, qui faiſant route pour les Canaries avoit été pouſſé aux Antilles par un coup de vent; mais *qu'il avoit ſur tout tiré grand parti des informations d'un celebre geographe nomm. Martin Bebenira.* Garcilaſſo ne nous dit rien de plus ſur ce *Bebenira*, et comme on ne connoit point de géographe Eſpagnol de ce nom, on a ſoupçonné de la Vega dávoir ſacrifié la verité au deſir de ne pas laiſſer à un Génois la gloire d'avoir decouvert le nouveau monde.

En parcourant avec attention la liſte de tous les ſavans du XV ſiecle, je trouve le nom de *Martin Behem*, grand géographe et navigateur, je trouve que le nom de batême eſt conforme à celui qui eſt cité par Garcilaſſo, que les ſyllabes *ira* ajoutées à ſon nom doivent être dues à une circonſtance particuliere, et cette circonſtance je la trouve dans la confiance dont il a été honnoré par Jean II roi de Portugal. Il eſt donc poſſible que ce *Martin Behem* ſoit le même homme que ce *Martin Bebinera* mentionné par Garcilaſſo; mais cette conjecture vague aura tous les caracteres de l'évidence par les détails ſuivans.

L'hiſtoire

The literary hiſtory of Germany gives an account of a Martin Behem, Beheim, or Behin, who was born at Nurenburgh, 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 exiſtance of the Antipodes and of a weſtern continent. Filled with this great idea, he paid a viſit in 1459 to Iſabella daughter of John the 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 Nurenburg, from which the following is copied. " Martin Behem tender-
" ed his ſervices to the daughter of John king of Luſita-
" nia, who reigned after the death of Philip of Burgun-
" dy ſirnamed 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,
" abound-

L'hiſtoire litteraire de l'Allemagne nous apprend que *Martin Behem*, *Beheim* ou *Behin* eſt né à Nurenberg, ville imperiale du cercle de Franconie, d'une famille noble dont quelques branches exiſtent encore aujourdhui. Dès ſa plus tendre jeuneſſe il ſe livra à l'étude de la geographie, de l'aſtronomie et de la navigation. Parvenu à un age mur il reflechit beaucoup ſur la poſſibilité de l'exiſtence des Antipodes et d'un continent occidental. Rempli de cette grande idée, i'l fut trouver en 1459 Iſabelle fille de Jean I. roi de Portugal et regente du duché de Bourgogne et de Flandre. Après lui avoir fait part de ſes projets, il en obtint un vaiſſeau avec lequel il fit, en 1460, la decouverte de l'iſle de Fayal. Il y établit une colonie Flamande, dont les deſcendans exiſtent encore aujourdhui aux Açores, qu'on a appellées pendant quelque tems les iſles Flamandes. Cette circonſtance eſt prouvée, non ſeulement par les auteurs contemporains, mais par des manuſcrits conſervés dans les archives de Nurenberg, dont voici la copie : " Martinus
" Behemus, Joannis Luſitaniæ regis filiæ, quæ poſt obitum Philippi Burgundi cognomento
" boni, rerum dominabatur, operam ſuam addixit, et ab ea navim impetravit, qua occidentalis
" oceani hactenus cognitos terminos et fines prætervectus *primus* poſt hominum memoriam
" *Fayalem* inſulam, *Fago* arbore, quam Luſitani *Faye* vocant, ac unde appellatio ei hæſit abun-
" dantem

" abounding with beachtrees, 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 writers, and especially by Wagenceil, one of the most learned men of the last century; who after having travelled into Africa, and throughout all Europe, was made doctor of laws at Orleans and chosen fellow of the academy of Turin and Padua, although he was a German by birth. The particulars are to be found in his universal history and geography. I have moreover received, from the records of Nurenberg, a note written in German on parchment, which contains the following facts. " Martin Behem, esquire, son of Mr. Martin Behem of Schroperin, lived in the reign of John II. king of Portugal, in an island which he discovered, and called the island of Fayal, one of the Azores, lying in the western ocean."

After having obtained from the regent Isabella a grant of Fayal, and resided there about twenty years, during which

" dantem reperit, nec minus postea finitimas insulas, uno nomine ab accipitrum ibi nidificantium multitudine *Açores* dictas (Lusitani enim hoc vocabulo *accipitres* efferunt, et Galli quoque in aucupiis verbum *Essos* et *Essores* adhibent) detexit, ac flandrorum colonias in iis reliquit, unde et insulæ illæ Flandriæ vocari cæperunt, &c." Quoique ce monument soit contraire à l'opinion généralement reçue que les Açores ont été decouvertes par un Portugais nommé Gonzalve Velho, on ne sauroit douter de son authenticité; il se trouve confirmé par plusieurs auteurs contemporains, et surtout par Wagenseil, un des plus grands savans du dernier siecle, qui après avoir voyagé par l'Afrique et par toute l'Europe, a été fait docteur en droit à Orleans, et academicien à Turin et à Padoue quoiqu'il fut né Allemand. On en trouve des détails dans son histoire universelle et dans sa geographie. On m'a d'ailleurs communiqué dans les archives de Nurenberg une note en Allemand écrite sur parchemin, contenant les faits suivans: " M. Martin Beham, ecuyer, fils de M. Martin Beham de Scopperin, a vecu sous le regne de Jean II. roi de Portugal, dans une isle qu'il a trouvée lui même, et qu'il a appellée isle de Fayal; elle est située aux Açores dans l'océan occidental."

Aprés avoir obtenu de la regente Isabelle la proprieté de Fayal, et aprés y avoir employé environ 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 ftreights 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 "ftrait which bears the name of Magellan, before either "Chriftopher Columbus or Magellan failed thofe feas; "and even mathematically delineated on a geographical "chart for the king of Lufitania, the fituation of the "coaft, around every part of that famous and renowned "ftrait." 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 let-

 ters

viron 20 années à faire des recherches ulterieures fur la géographie, dans de petites excurfions que ne meritent par d'être rapportées ici; Behem s'adreffa en 1484, c'eft à dire huit années avant l'expedition de Chriftophe Colomb, à Jean II. roi de Portugal, pour obtenir de lui les moyens d'entreprendre une grande expedition vers le fud-oueft. Ce prince lui confia quelques vaiffeaux avec lefquels il decouvrit la partie de l'Amerique connue fous le nom de Brefil, et il étendit meme fa navigation jufqu'au détroit de Magellan, ou à la terre de quelques hordes fauvages qu'il appella Patagons, puifque les extremités de leurs corps couvertes de peaux reffembloient plutôt à des pattes d'ours qu'à des pieds et à des mains. Ce fait eft prouvé par des documens authentiques dépofés dans les archives de Nurenberg. Il y en a un furtout qui merite notre attention: "Martinus Behemus per oceanum Atlanticum huc illuc annos plufculos "oberrans, ante Chriftophorum Columbum Americæ infulas, ante Fernandum Magellanum, "fretum quod ab eo cognomentum habet perveftigavit, unde et in *tabula geographica longe prius* "*quam Magellanes* de expeditione fua cogitaffet, omnem circa infigne clariffimumque fretum "illud oræ habitudinem Lufitaniæ regi radio delineavit." Cette affertion fe trouve appuyée par des lettres de Behem ecrites en Allemand, et confervées dans les archives de Nurenberg, dans un volume contenant l'origine et les actions éclatantes des patriciens de cette ville. Ces lettres

ters are dated in 1486; that is, ſix years before the expedition of Columbus. This wonderful diſcovery has not eſcaped the notice of cotemporary writers. The following paſſage is extracted from the chronicle of Hartman Schedl: "In the year 1485, John the ſecond, king of
" Portugal, a man of a magnanimous ſpirit, furniſhed
" ſome gallies with proviſions, and ſent them to the ſouth-
" ward beyond the ſtraits of Gibraltar. He gave the com-
" mand of this ſquadron to James Canus, a Portugueſe,
" and Martin Behem a German of Nurenberg in Upper
" Germany, deſcended of the family of Bonna, a man
" very well acquainted with the ſituation of the globe,
" bleſſed with a conſtitution able to bear the fatigues of
" the ſea, and who by actual experiments and long ſail-
" ing, had made himſelf perfectly maſter with regard to
" the longitudes and latitudes of Ptolemy, in the weſt.
" Theſe two, by the bounty of Heaven, coaſting along the
" ſouthern ocean, and having croſſed the æquator, got
" into the other hemiſphere, where facing to the eaſt-
" ward, their ſhadows projected towards the ſouth and
" right-hand. Thus, by their induſtry, they may be ſaid
" to have opened to us another world hitherto unknown,
" and for many years attempted by none but the Genoeſe,
" and by them in vain. Having finiſhed this cruiſe in the
" ſpace of twenty-ſix months, they returned to Portugal,
" with the loſs of many of their ſeamen, by the violence
" of the climate." This

lettres ſont datées de 1486, c'eſt à dire ſix années avant l'expédition de Chriſtophe Colomb. Quelques auteurs contemporains n'ont pas manqué de faire mention d'une decouverte auſſi étonnante. Je trouve entre autres dans la chronique de Hartman Schedl le paſſage ſuivant :
" Anno Domini 1485, Johannes ſecundus Portugalliæ rex, altiſſimi vir cordis, certas galeas
" omnibus ad victum neceſſariis inſtruxit, eaſque ultra columnas Herculis ad meridiem miſit.
" Præfecit autem his patronos duos Jacobum Canum Portugallenſem et Martinum Behemum,
" hominem Germanum ex Nurimberga ſuperioris Germaniæ de Bonna familia natum, hominem
" inquam in cognoſcendo ſitu terræ peritiſſimum, mariſque patientiſſimum, qui Ptolomæi lon-
" gitudines et latitudines in occidente ad unguem experimento longævaque navigatione novit.
" Hi duo, bono Deorum auſpicio mare meriodionale ſulcantes a littore non longe evagantes
" ſuperato circulo æquinoctiali in alterum orbem excepti ſunt, ubi ipſis ſtantibus orientem
" verſus umbra ad meridiem et dexteram projiciebatur. Aperuere igitur *ſua induſtria alium*
" *orbem hactenus nobis incognitum*, et multis annis a nullis quam a *Januenſibus licet fruſtra* tentatum;
" peracta autem hujuſmodi navigatione viciſſimo ſexto menſe reverſi ſunt in Portugalliam,
" pluribus ob äeris impatientiam mortuis."

Cequi

This paſſage becomes more intereſting, from being quoted in a book on the ſtate of Europe during the reign of the emperor Frederick III. by the learned hiſtorian Æneas Sylvius, afterwards pope Pius II. This hiſtorian died before the diſcoveries of Behem were made, but the publiſhers of his works, thought the paſſage in Hartman Schedl ſo important, that they inſerted it in the hiſtory. We alſo find the following particulars, in the remarks made by Petrus Matæus, on the canon law, two years before the expedition of Columbus: "The firſt chriſtian "voyages to the newly diſcovered iſlands became fre-"quent, under the reign of Henry ſon of John king of "Luſitania. After his death, Alphonſus the fifth proſecut-"ed the deſign, and John who ſucceeded him followed "the plan of Alphonſus by the aſſiſtance of Martin Bœhm, "a very experienced navigator, ſo that, in a ſhort time, "the name of Luſitania become famous over the whole "world." Cellarius, one of the moſt learned men of his age, ſays expreſsly: "Bœhm did not think it enough to "ſurvey the iſland of Fayal, which he firſt diſcovered, or "the other adjacent iſlands which the Luſitanians call "Azores, *and we after the example of Bœhm*'s *companions*, "*call Flemiſh iſlands;* but advanced ſtill farther and far-"ther ſouth, until he arrived at the remoteſt ſtrait, be-"yond which, Ferdinand Magellan, following his tract, "afterwards ſailed and called it after his own name."

 All

Ce qui rend ce paſſage plus intereſſant encore, c'eſt qu'il eſt cité dans l'ouvrage du ſavant hiſtorien *Æneas Sylvius*, depuis pape ſous le nom de Pie II. ſur l'état de l'Europe du tems de l'empereur Frederic III. Cet hiſtorien eſt mort avant les decouvertes de Behem, mais les copiſtes de l'ouvrage d'Æneas Sylvius ont trouvé le paſſage de Hartman Schedl ſi intéreſſant qu'ils l'ont inſeré dans le corps de cette hiſtoire. Nous trouvons d'ailleurs dans les notes que Petrus Matæi a faites ſur le droit canon, deux ans avant l'expedition de Colomb, les détails ſuivans: "Primæ navigationes chriſtianæ ad novas inſulas eluceſcere cæperunt ſub Henrico "Johannis Luſitaniæ regis filio, &c. illo mortuo Alphonſus V. cæpta proſecutus eſt; Al-"phonſum Johannes imitatus opera *Martini Bohemi*, hominis in curſu navium peritiſſimi, ut "brevi tempore nihil celebrius per totum orbem audiretur ipſo Luſitaniæ nomine." Cellarius, un des plus grands ſavans de ſon ſiecle, dit expreſſement: "Behaimius non modo "Fagalem inſulam, quam primus invenit, aut alias circumjectas quas Azores Luſitani, nos "*Flandrias a Behaimi comitibus*, nominant, perluſtrandas ſibi cenſuit, verum etiam in auſtrum "magis et magis progreſſus uſque ad ultimum fretum, quod Ferdinandus Magellanus hujus "ductum ſecutus, pertranſiit et de ſuo id nomine appellavit."

Toute.

All theſe quotations, which cannot be thought tedious, ſince they ſerve to prove a fact almoſt unknown, ſeem to demonſtrate, that the firſt diſcovery of America is due to the Portugueſe, and not to the Spaniards; and that the chief merit belongs to a German aſtronomer. The expedition of Ferdinand Magellan, which did not take place before the year 1519, aroſe from the following fortunate circumſtance. This perſon, being in the apartment of the king of Portugal, ſaw there a chart of the coaſt of America, drawn by Behem, and at once conceived the bold project of following the ſteps of this great navigator. Jerome Benzon, who publiſhed a deſcription of America in 1550, ſpeaks of this chart, a copy of which, ſent by Behem himſelf, is preſerved in the archives of Nurenberg. The celebrated aſtronomer Riccioli, though an Italian, yet does not ſeem willing to give his countryman the honor of this important diſcovery. In his geography reformed, book III. page 90, he ſays: "Chriſtopher Columbus never thought "of an expedition to the Weſt Indies, until ſome time "before, while in the iſland of Madeira, where amuſing "himſelf in forming and delineating geographical charts, "he obtained information from Martin Bœhm, or as the "Spaniards ſay, from Alphonſus Sanchez de Huelva, a "pilot, who by meer chance had fallen in with the iſland "afterwards called Dominica." And in another place, "let Bœhm and Columbus have each their praiſe, they "were

Toutes ces citations qui ne ſauroient être trop longues, parcequ'elles ſervent à prouver un fait preſqu' inconnu, paroiſſent demontrer que la premiere decouverte de l'Amerique eſt due aux Portugais, et non aux Eſpagnols; et que c'eſt un aſtronome Allemand qui étoit à leur tête. L'expedition de Ferdinand Magellan, qui n'a eu lieu qu'en 1519, eſt due à un heureux hazard. Ce navigateur ſe trouvant dans l'appartement du roi de Portugal, y vit une carte des cotes de l'Amerique tracée par Behem, et conçut dès lors le projet hardi de ſuivre la route de ce grand navigateur. Jerome Benzon, qui a donnée en 1550, une deſcription de l'Amerique, fait mention de cette carte, dont on a conſervé une copie dans les archives de Nurenberg, où Behem l'avoit envoyée lui-même; le celebre aſtronome Riccioli, qui étoit lui-même Italien, ne paroit pas cependant vouloir attribuer à ſon compatriote cette importante decouverte; il dit dans ſa geographie reformée, livre 3. p. 90. "Chriſtophorus Columbus, cum "prius in Madera inſula, ubi conficiendis et delineandis chartis geographicis vacabat, *indicio habito a Martino Bohemo*, aut *ut Hiſpani dictitant* ab Alphonſo Sanchez de Huelva, nauclero "qui forte inciderat in inſulam, poſtea *Dominicam* dictam, cogitavit de navigatione in Indiam "occidentalem." Et dans un autre endroit, "Sit ſua laus Bohemo, ſit ſua laus Columbo, "ambo "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 ſo "much celebrated as that of Columbus, Americus or Ma- "gellan, although he is ſuperior to them all."

But the moſt poſitive proof of the great ſervices rendered to the crown of Portugal by Behem, is the recompence beſtowed on him by king John, who in 1485 knighted him in the moſt ſolemn manner, in the preſence of all his court. I have before me a German paper extracted from the archives of Nurenberg to the following purpoſe. "In the year 1485, on the 18th of February, in "Portugal, in the city of Allaſavas, and in the church of "St. Salvador, after the maſs, Martin Behem of Nurenberg, "was made a knight by the hands of the moſt puiſſant "lord, John the ſecond king of Portugal, Algarve, Afri- "ca and Guinea; and his chief ſquire was the king him- "ſelf, who put the ſword in his belt; and the duke of Begia "was his ſecond ſquire, who put on his right ſpur; and "his third ſquire was count Chriſtopher de Mela, the "king's couſin, who put on his left ſpur; and his fourth "ſquire was count Martini Marbarinis who put on his "iron helmet; and the king himſelf gave him the blow "on the ſhoulder, which was done in the preſence of all "the princes, lords and knights of the kingdom: and he "eſpouſed

"ambo fuerunt magni navarchi, ſed nunquam de ſua in Americam expeditione cogitaſſet "Columbus niſi Bohemum habuiſſet prædeceſſorem. Hujus nomen non tantopere celebra- "tur quanto Columbi, Americi et Magellani, quamvis his tribus ſit præferendus."

Mais ce qui prouve plus que toute autre choſe les grands ſervices rendus par Behem à la couronne de Portugal, c'eſt la reconnoiſſance du roi Jean, qui en 1485, le fit lui-même chevalier, de la maniere la plus ſolemnelle, et en preſence de toute ſa cour. J'ai ſous mes yeux un document Allemand, tiré des archives de Nurenberg, de la tenur ſuivante: "En 1485, "le 18 Fevrier, en Portugal dans la ville d'Allaſavas, et dans l'égliſe de Santo Salvador, "après la meſſe, a été fait chevalier Martino Behem, de Nurenberg, par la main du très "puiſſant ſeigneur roi Jean ſecond de Portugal, roi d'Algarve, roi d'Afrique et roi de "Guinée; et ſon premier ecuyer etoit le roi lui-même, qui mit ſon épée à ſon ceinturon; et "le duc de Begia étoit ſon ſecond ecuyer qui lui mit ſon éperon droit; et ſon troiſieme "ecuyer étoit le comte Chriſtophe de Mela couſin du roi, qui lui mit ſon éperon gauche; et "ſon quatrieme ecuyer étoit le comte Martini Marbarinis, qui lui mit ſon caſque de fer; et "le roi lui donna lui-même un coup ſur l'epaule; ce qui ſe paſſa en preſence de tous les princes "et de tous les ſeigneurs et chevaliers du royaume; et il a eſpouſé la fille d'un grand ſeigneur "en

" espoused the daughter of a great lord, in consideration " of the important services he had performed, and he was " made governor of the island of Fayal." These marks of distinction conferred on a stranger, could not be meant as a recompence for the discovery of the Azores, which was made 20 years before; but as a reward for the discovery of Congo, from whence the chevalier Behem had brought gold and different kinds of precious wares. This discovery made much greater impression than that of a western world, made at the same time, but it neither increased the wealth of the royal treasury, nor satisfied the avarice of the merchants.

In 1492 the chevalier Behem, crowned with honors and riches, undertook a journey to Nurenberg, to visit his native country and his family. He there made a terrestrial globe, which is looked on as a master-piece for that time, and which is still preserved in the library of that city. The tract of his discoveries may there be seen under the name of western lands, and from their situation it cannot be doubted, that they are the present coasts of Brazil and the environs of the straits of Magellan. This globe was made in the same year that Columbus sat out on his expedition, from whence it is not possible that Behem could have profited by the works of this navigator, who besides, went a much more northerly course.

After having performed several other interesting voyages, the chevelier Behem died at Lisbon in July 1506, regretted

" en considération des services qu'il a rendus, et il a été fait gouverneur de l'isle de Fayal." Cette grande distinction accordée à un étranger, ne pouvoit être la recompense de la decouverte des Açores, qui avoit eu lieu plus de 20 ans auparavant; mais elle étoit le pris de la decouverte du Congo, d'ou le chevalier Behem avoit apporté de l'or et plusieurs marchandises précieuses. Cette decouverte fit beaucoup plus d'impression que celle d'une terre occidentale faite dans le meme tems, mais qui n'offroit aucun benefice au trésor royal ni à la cupidité des marchands.

En 1492, le chevalier Behem, comblé d'honneur et de richesses, entreprit un voyage à Nurenberg, pour revoir sa patrie et sa famille. Il y composa un globe terrestre, qui est regardé comme un chef d'œuvre de son tems, et qui est encore conservé dans la bibiliotheque de cette ville. On y voit la trace de ses decouvertes sous le nom de terres occidentales, et par leur situation on ne peut disconvenir qu'elles ne soient les cotes actuelles du Bresil et les environs du détroit de Magellan. Ce globe est, fait dans la même année où Colomb a commençé son expedition; il est donc impossible que Behem ait profité du travail de ce navigateur, qui d'ailleurs a dirigé sa course beaucoup plus au nord.

Après avoir achevé plusieurs autres voyages intéressans, le chevalier Behem mourut à Lisbonne regretted by every body, 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 XIIIth century, and of John Mandeville, an Engliſhman, who, about the middle of the XIVth century, publiſhed an account of a journey of 33 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 honor 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 Behemira, who was certainly a man of too much importance not to have had a diſtinguiſhed place in hiſtory. Beſides, the Spaniſh pride:

bonne en Juillet 1506, generalement regretté, mais ne laiſſant à la poſterité d'autre ouvrage que le globe dont nous venons de parler. Il eſt fait d'après les ecrits de Ptolomée, de Pline, de Strabon, et ſurtout d'après les relations du Venitien Marc Paul, voyageur celebre du XIII ſiecle, et de Jean Mandeville, Anglois, qui au milieu du XIV ſiecle a publié les détails d'un voyage de 33 années en Afrique et en Aſie; il y a ajouté, les grandes decouvertes qu'il a faites lui-même ſus les cotes d'Afrique et d'Amérique.

D'après ces détails peu connus des écrivains modernes, nous devons conclure que *Martin Behenira*, dont Garcilaſſo fait mention, eſt ce même chevalier Behem que la ville de Nurenberg ſe glorifie d'avoir vu naitre dans ſes murailles. Il eſt vraiſemblable qu'au moment où il fut creé chevalier en Portugal, il a cru devoir donner une terminaiſon Portugaiſe à ſon nom, pour le rendre plus ſonore et plus conforme à l'idiome du pays. Garcilaſſo trompé par cette reſemblance de ſon, en a fait un Eſpagnol pour enlever à Chriſtophe Colomb la gloire d'avoir procuré à ſa métropole un auſſi grande acquiſition. Ce qui doit nous confirmer dans cette opinion, c'eſt que nous ne trouvons ni dans Mariana ni dans aucun autre hiſtorien Eſpagnol le nom de ce *Martin Behemira* qui aureit du être un homme trop important pour ne pas occuper:

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 had undoubtedly engaged king John to give into 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 promiſed himſelf 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 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,

cuper une place diſtinguée dans l'hiſtoire. La fierté Eſpagnole auroit d'ailleurs été flattée d'accorder à un national les lauriers dont elle a couronné Chriſtophe Colomb.

Il eſt donc peu vraiſemblable que ce navigateur ait été traité comme un extravagant quand il offrit à la cour de Portugal de faire des découvertes dans l'oueſt. La recherche des pays incunnus étoit alors la paſſion dominante de cette cour, et quand même le chevalier Behem n'auroit pas donné les notions importantes qu'il s'étoit procurées, la nouveauté du projet eut indubitablement engagé le roi Jean à ſe preter aux vues de Colomb; mais il paroit que ce prince s'y eſt refuſé, puiſque toutes ſes vues portoient alors ſur la cote d'Afrique et le nouveau paſſage dans l'Inde, d'où il ſe promettoit de tirer de grandes richeſſes, tandiſque les cotes meridionales du Breſil et la terre des Patagons, vues par Behem, ne lui offroient que des terres ſteriles, habitées par des ſauvages indomptables. Le refus de Jean II. bien loin d'affoiblir l'evidence des decouvertes de Behem, eſt donc plutot une preuve des connoiſſances que ce prince habile s'étoit déjà procurées ſur l'exiſtence d'un nouveau continent; et ce n'eſt qu'en 1501, c'eſt à dire trois ans aprés l'expedition de Vaſco de Gama dans l'Inde, qu'Emanuel jugea à propos de tirer parti des decouvertes de Behem, en envoyant au Breſil Albarez Cabral; meſure qui étoit peut être plutôt une ſuite de cette jalouſie qui a toujours exiſté entre le

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 neverthelеſs, with 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 favor 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, which Dr. Forſter procured him a copy of, and which, in his opinion, partakes of the imperfection of the coſmographical knowledge of the fifteenth 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

le Portugal et l'Eſpagne, que du deſir de faire des établiſſemens avantageux aux quels l'Inde etoit beaucoup plus propre que cette partie de l'Amerique.

S'il nous eſt permis de douter encore de l'importante decouverte faite par le chevalier Behem, c'eſt ſurtout l'autorité du Dr. Robertſon, qui doit porter atteinte aux témoignages des differens auteurs que nous avons tranſcrits. Ce ſavant écrivain traite l'hiſtoire de Behem comme une fiction de quelques auteurs Allemans, qui deſiroient d'attribuer à un de leurs compatriotes une decouverte qui a produit une ſi grande revolution dans le commerce de l'Europe. Mais il avoue cependant, d'après Herera, que Behem étoit établi à l'iſle de Fayal, qu'il étoit *l'ami intime de Chriſtophe Colomb*, et que Magellan avoit eu un globe composé par Behem, d'après lequel il avoit entrepris ſon expedition dans la mer du Sud; circonſtance qui prouve beaucoup en faveur de notre hypotheſe. Il rapporte de plus qu'en 1492, cet aſtronome a été voir ſa famille à Nurenberg, et qu'il y a laiſſé une carte deſſinée par lui-même; que le Dr. Forſter lui a procuré une copie de cette carte, qui ſuivant lui ſe reſſent de l'imperfection des connoiſſances coſmographiques du XV ſiecle; qu'il y a trouvé à la verité ſous le nom de l'iſle de St. Brandon, une terre qui paroit être la cote actuelle de la Guyane, et qui eſt placée dans la latitude 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 honor 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 Muller 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 Fernambouc, 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

titude du cap Verd; mais qu'il y a lieu de croire, que cette iſle fabuleuſe, qui ſe trouve ſur pluſieurs anciennes cartes, ne merite pas plus d'attention que la legende puerile de St. Brandon lui-même. Quoique le Dr. Robertſon ne paroiſſe pas diſpoſé d'accorder à Behem la gloire d'avoir decouvert le nouveau continent, nous trouvons dans ſon hiſtoire méme des armes pour le combattre. Il convient que Behem étoit très lié avec Chriſtophe Colomb, qu'il étoit le plus grand geographe de ſon tems, et diſciple du celebre Jean Muller ou Regiomontanus; qu'il a decouvert en 1483, le royaume de Congo, ſur la cote d'Afrique; qu'il a compoſé un globe dont s'eſt ſervi Magellan; qu'il a deſſiné á Nurenberg, une carte contenant des détails ſur ſes decouvertes, et qu'il a marqué ſur cette carte une terre qui ſe trouve dans la latitude de la Guyane actuelle. Le docteur Robertſon admet ſans aucune preuve, que cette terre n'étoit qu'une iſle fabuleuſe; nous pouvons ſuppoſer avec autant de ſondement, que le chevalier Behem faiſant ſon expedition dans le royaume de Congo, ait été pouſſé par les vents vers Fernambouc, et de là par des courans très communs dans ces parages vers les cotes de la Guyane, et qu'il ait pris pour une iſle la premiere terre qui s'eſt offerte à ſes yeux. La route qu'a pris dans la ſuite Chriſtophe Colomb rend cette ſuppoſition encore plus vraiſemblable, car s'il n'avoit eu connoiſſance que des cotes du Breſil, que l'on croit avoir été reconnues par Behem, il auroit dirigé ſa navigation plutôt vers le ſud-oueſt. L'expedition au Congo a eu lieu 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 German pieces in the original, which we have quoted, we may be allowed to form a different opinion without being too preſumptuous.

But ſhould it be aſked, why we take from Chriſtopher Columbus the reputation which all Europe has to this day allowed him? Why we are ſearching in 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 chevr. Behem, to leſſen his conſequence at the Spaniſh court? Why Portugal, jealous of the diſcovery of the new world, had 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

lieu en 1483, il eſt donc poſſible qu'à ſon retour Behem ait projetté une expedition vers les cotes du Breſil et des Patagons, et qu'il ait demandé à ſon ſouverain les ſecours dont nous avons parlé plus haut. Il eſt ſur qu'on ne ſauroit avoir trop de déference pour l'opinion d'un écrivain tel que Robertſon, mais ce ſavant n'ayant pu avoir connoiſſance des pieces Allemandes originales que nous avons citées, nous pouvons avoir un avis different du ſien ſans nous rendre coupable de preſomption.

Mais, dira-ton, pourquoi enlever à Chriſtophe Colomb une gloire que toute l'Europe lui a accordée juſqu'ici? Pourquoi chercher dans les archives d'une ville imperiale les cauſes d'un évenement qui a eu lieu à l'extremité la plus occidentale de l'Europe? Pourquoi les ennemis de Chriſtophe Colomb, qui étoient en grand nombre, n'ont 'ils pas tiré parti des prétendues decouvertes du Chevalier Behem pour diminuer ſon importance à la cour d'Eſpagne? Pourquoi le Portugal, jaloux de la decouverte du nouveau monde, n'at'il pas proteſté contre les aſſertions des Eſpagnols? Pourquoi Behem qui n'eſt mort qu'en 1506, n'at'il pas laiſſé lui même à la poſterité un écrit pour s'attribuer une decouverte auſſi importante?

Pour repondre à toutes ces queſtions je ſoumettrai au lecteur impartial les remarques ſuivantes:

1. Avant

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 idea of what passed in foreign countries. Gazettes and journals were unknown, and the learned obliged to travel to inform themselves of the progress of their neighbours. Italy was the center of the arts and what are called science at that time. The frequent journies 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

1. Avant Colomb le grand merite d'un navigateur consistoit plutôt à conçevoir la possibilité de l'existence d'un nouveau continent, qu'à chercher des terres dans une region où il etoit sur d'en trouver. S'il est donc certain que Behem a eu cette idée hardie avant Colomb, la gloire de ce dernier en est singulierement diminuée.

2. Toutes les preuves historiques que nous avous donnés ci dessus ne nous laissant aucun doute sur le fait, il s'agit seulement d'expliquer les causes morales du silence des auteurs Espagnols et Portugais, des énnemis de Colomb, et de Behem lui-même.

3. On sait qu'avant Charles quint il y avoit très peu de communication entre les savans des differentes nations. Les ecrivains étoient fort rares, à l'exception de quelques moines qui ont rapporté bien ou mal les evenemens qui étoient à leur portée dans des chroniques qu'on ne lit plus, ou n'avoit que peu de notions sur ce qui se passoit en pays étranger ; les gazettes, les journaux étoint inconnus, et les savans étoient obligés de voyager pour voir de leurs propres yeux les progrés de leurs voisins. L'Italie étoit le centre des arts et de ce qu'on appelloit science dans ce tems-là. Les frequens voyages des empereurs d'Allemagne à Rome leur donna la facilité de connoitre des gens de merite, et de les placer dans les differentes universités de

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 body was either ſhopkeeper, 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

de l'empire. C'eſt à cette circonſtance que l'on doit attribuer les grands progrés que les Allemans ont faits ſurtout dans les mathematiques, depuis le XIV. juſqu'au XVI. ſiecle; ils avoient les meilleurs géographes, les meilleurs hiſtoriens et les politiques les plus éclairés. Ils étoient attentifs ſur tout ce qui ſe paſſoit en Europe, et les liaiſons multipliées des differens princes avec les puiſſances étrangeres, leur donnoient une grande facilité de raſſembler, dans leurs archives, les pieces originales des evenemens les plus importans de l'Europe. C'eſt a cet eſprit de critique et de recherche qu'eſt due en grande partie la reformation de Luther, et on ne peut ſe diſſimuler, que, ſurtout dans le XV. ſiecle, il n'y ait eu plus de connoiſſances hiſtoriques et politiques en Allemagne que dans tout le reſte de l'Europe, à l'exception de l'Italie. Il n'eſt donc pas étonnant que nous trouvions dans les archives d'une des plus anciennes villes imperiales des détails ſur une expedition, projetté ſur les bords du Tage par un Allemand, par un homme très conſidéré dans ſon pays, et dont par conſequent toutes les demarches devenoient interéſſantes.

4. Il n'en étoit pas de même du Portugal, où toute la nation à l'exception du ſouverain, étoit plongée dans la plus profonde ignorance. Tout le monde y étoit ou marchand, ou matelot, ou ſoldat; et ſi ce peuple a fait les decouvertes les plus importantes, il faut en chercher le motif dans ſa cupidité, et non dans ſon deſir de s'inſtruire. Il ſe contenta d'amaſſer de l'or dans

ing 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 much more informed before Charles V. introduced at Madrid the learned men of Flanders and Germany. It is then very poſſible, that the chevr. Behem made very intereſting diſcoveries in geography, in 1485, without the public's being acquainted with them. If he had brought back from his expedition, gold or diamonds, the noiſe 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 which he had formed. The mariners who accompanied the chevr. 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 26. 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

dans toutes les parties du monde connu, tandiſque l'Allemand et l'Italien tenoient la plume pour tranſmettre à la poſterité le ſouvenir de ſes richeſſes et de ſes cruautés. Les Eſpagnols n'étoient par beaucoup plus inſtruits, avant que Charlesquint eut amené à Madrid des ſavans de Flandres et d'Allemagne. Il eſt donc très poſſible que le chevalier Behem ait fait en 1485, des decouvertes trés intéreſſantes pour la géographie, ſans que le public en ait été inſtruit. S'il eut rapporté de ſon expedition de l'or ou des diamans, le bruit s'en ſeroit repandu en peu de ſemaines, mais de ſimples notions géographiques n'étoient pas de nature à intéreſſer des hommes de cette trempe.

5. Le long ſéjour qu'a fait Chriſtophe Colomb à Madere, rend ſon entrevue avec Behem plus que vraiſemblable. Il eſt impoſſible qu'il ait négligé de voir un homme auſſi intéreſſant, et qui pouvoit lui donner toutes ſortes de conſeils ſur l'execution du plan qu'il avoit formé. Les marins qui ont accompagné le chevalier Behem pouvoient d'ailleurs repandre à Madere, et aux Açores, des bruits concernant les decouvertes dont ils avoient été temoins. Ce qui doit nous confirmer dans cette opinion, c'eſt que *Mariana* dit lui-même (livre 26. chap. 3.) qu'un *certain* batiment allant en Afrique, avoit été jetté par un coup de vent ſur de certaines terres inconnues; et que les matelots après leur retour à Madere, avoient communiqué à Chriſtophe

ſumbus 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 chevr. 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 the names of 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

Chriſtophe Colomb les circonſtances de leur navigation. Tous les auteurs s'accordent en géneral, que ce ſavant avoit eu quelques renſeignemens ſur des terres occidentales, mais ils ne nous en parlent que d'une maniere très vague. L'expedition du chevalier Behem nous explique ce myſtere.

6. Cet aſtronome ne pouvoit être jaloux des decouvertes de Colomb, puiſque celui-ci a été beaucoup plus nord; et que dans un tems où l'on ne connoiſſoit par toute l'étendue du nouveau monde, et où les connoiſſances géographiques étoient extrêmement bornées, on pouvoit croire que le pays trouvé par Colomb n'avoit aucun rapport avec celui de Behem.

Quoiqu'il en ſoit, il paroit certain que Behem a decouvert ce continent avant Colomb, et que cette queſtion qui n'eſt que de pure curioſité en Europe, devient intéreſſante pour le patriote Américain. Les Grecs ont conſervé ſoigneuſement l'hiſtoire fabuleuſe de leurs premieres ſondateurs, ils leur ont élevé des autels; pourquoi Behem, Chriſtophe Colomb et Veſpuce ne meriteroient'-ils pas des ſtatues ſur les places publiques des villes Americaines? Ces monumens precieux tranſmettroient à la poſterité la reconnoiſſance que doivent inſpirer les noms de ces bienfaiteurs du genre humain. Sans le ſavoir ils ont jetté les fondemens du bonheur de pluſieurs millions d'habitans; et Seſoſtris, Phul, Cyrus, Theſée, Romulus, les fondateurs greateſt empires, will be forgotten, before the ſervices rendered by theſe illuſtrious navigators can be effaced from the memory of man.

fondateurs des plus grands empires, ſeront oubliés, avant que les ſervices rendus par ces navigateurs illuſtres, puiſſent s'effacer de la memoire des hommes.

N° XXXVI.

The antiſeptic Virtues of Vegetable Acid and Marine Salt combined, in various Diſorders accompanied with Putridity; communicated in a Letter to JOHN MORGAN, *M.D. F.R.S. and Profeſſor of the Theory and Practice of Phyſic at Philadelphia, by* WILLIAM WRIGHT, *M. D. of Trelawney in Jamaica.*

HAVING experienced the virtues of vegetable acid and marine ſalt, when combined; I beg leave to lay before you a few obſervations on the uſe of this ſimple medicine in ſeveral diſeaſes. It is my ſincere wiſh, that it may prove as beneficial to mankind in general, as it has been to many of my patients in this part of the country.

Take of lime-juice or lemon-juice three ounces, of marine ſalt as much as the acid will diſſolve; of any ſimple diſtilled cordial water one pint; and of loaf ſugar a ſufficient quantity to ſweeten it. The doſe of this mixture muſt be proportioned to the age, ſex, and violence of the diſeaſe. A wine glaſs-ful may be given to adults every two, four or ſix hours.

By Geoffroy's table it appears, that the foſſil alkali has a greater affinity with the marine, than with the vegetable acid. However, marine ſalt diſſolves readily in the lime-juice, throws up a white ſcum to the ſurface, and on applying the ear near the veſſel where the experiment is made, a ſmall hiſſing may be heard, ſimilar to that when acids

acids and alkalies are mixed. It would ſeem probable that part of the marine ſalt is hereby decompoſed.

That vegetable acids and marine ſalt are antiſeptics, has long been known, but their effects when mixed I apprehend to be but lately diſcovered.

Without farther preface, I ſhall proceed to the particular diſeaſes in which they have been adminiſtered, prepared as above.

Of the DYSENTERY.

The dyſentery is a very frequent diſorder in this and other Weſt-India iſlands; and ſometimes is epidemic, particularly in the rainy ſeaſons, or when proviſions are ſcarce. Amongſt other cauſes of dyſenteries, I have often known the eating of yams not arrived at maturity, as alſo unripe alligator pears, produce a bloody flux.

Dyſenteries commonly begin with frequent looſe ſtools for a day or two, attended with gripings: by degrees, the gripes grow more ſevere, nothing is voided by ſtool but a ſmall quantity of mucus, mixed with blood; a teneſmus comes on and is exceedingly troubleſome.

The appetite fails, the patients are low ſpirited, and ſuffer a great proſtration of ſtrength. The mouth and tongue are much furred and ſlimy, and the taſte is like that of rotten butchers meat. The deſire of drink is ſometimes exceſſive, but for the moſt part very moderate. The pulſe is very low, feeble and undulating; and rarely riſes ſo high, as to indicate the uſe of a lancet. Such was the dyſentery in 1771. It proved fatal to many people, both old and young, though treated according to the moſt approved methods of cure, and the loſs of ſeveral patients of mine, convinced me of the neceſſity of uſing antiſeptics early in this diſeaſe.

A vomit ſeemed neceſſary to clear the ſtomach, and ſome gentle purge, to carry off part of the offending matter by ſtool. But the action of theſe, however mild, often increaſed the proſtration of ſtrength, and rendered the ſtools ſooner bloody. Nor was opium of any real uſe. A tea made of Simarouba and given to ſome, had a very ſalutary effect, whilſt, if given to others, it would by no means lye on their ſtomachs.

From a conſideration of the antiſeptic quality of both the ſal: marin: and of the vegetable acid, I was induced to make trial of their effects united in the manner above mentioned. It acted like a charm, and I find that from the uſe of it, the frequency of ſtools, gripes and teneſmus, have ſoon worn off; the ſtools gradually become of a natural conſiſtence and quantity; the ſpirits, ſtrength and appetite returned, and the patient has been reſtored to perfect health in a very few days.

When the dyſentery was of long ſtanding, ſtarch clyſters, with a ſmall portion of opium, abated the teneſmus.

This medicine was equally ſerviceable in diarrhœas.

DIABETES.

As I had ſucceeded ſo well in the cure of dyſenteries, I was determined to try its effects in the diabetes: ſeveral opportunities ſoon offered; but as theſe caſes were accompanied with other complaints, eſpecially with fevers of the remitting kind, it will be proper firſt to ſpeak of

The REMITTENT FEVER.

This by far the moſt common fever within the tropics, is the leaſt underſtood, and conſequently for the moſt part badly treated. Strangers, who walk much, or work hard in the heat of the ſun, are more ſubject to it than ſeaſoned Europeans or natives of the country.

Dr. Cleghorn's deſcription of this fever is accurate and juſt. His method of cure, ſimple and eaſy. Every phyſician

ſician, who would wiſh to practice with ſucceſs, ſhould be well acquainted with that valuable performance, as alſo with what Dr. Lind has ſaid on the ſubject.

It is then ſufficient here, to obſerve that remittent fevers are often attended with diarrhœas, the diabetes, and ſometimes with a copious diſcharge of ſaliva, as if mercury had been previouſly given. In ſuch circumſtances I never found the bark of ſervice; a few glaſſes of the above mixture fully anſwered the intention, not only by removing theſe ſymptoms, but the fever at the ſame time.

The Peruvian bark afterwards, taken out of ſome of the ſame mixture, effectually ſecured the patient from a return of this dangerous malady.

The mixture rarely acted as an aſtringent in this or any other diſorder. But when this effect took place, the interpoſition of ſome lenient purge was deemed neceſſary.

BELLY-ACH.

The belly-ach with inflammatory ſymptoms has frequently occured in the courſe of my practice; they yielded with difficulty to bleeding, ſmall doſes of emetic tartar, a mercurial pill, repeated doſes of caſtor oil, diluting drinks, with nitre, fomentations and glyſters. A copious diſcharge of fœtid excrement for the moſt part gives immediate relief.

I have obſerved in many caſes, after moſt excruciating belly-achs, that the ſtools were liquid, white, ſmall in quantity, and very fœtid. The patients being worn out with pain, grew diſpondent, did not care to ſpeak, fell into cold clammy ſweats and were very reſtleſs. They complained of an ill taſte in their mouths. Their tongues were much furred. Their breath offenſive, and they had a great propenſity to vomit.

Formerly I attempted the relief of thoſe threatening ſymptoms with the bark, in various forms, as well as claret, and often ſaved my patient; ſometimes however I failed of ſucceſs. When ſuch caſes fall now under my care, I have immediate recourſe to the antiſeptic mixture; nor have I been hitherto diſappointed: the ſtools becoming leſs frequent on the uſe of it, and of a better conſiſtence; the cold ſweats alſo diſappear, and the ſpirits ſoon return, together with an appetite for food.

The PUTRID SORE THROAT.

In June 1770, the putrid ſore throat made conſiderable havock amongſt adults and children. It attacked thoſe of a lax habit, who for a few days had ſlight head-achs, chillineſs and heats alternately, and an uneaſineſs about their throats, but not ſo much as to hinder their ſwallowing.

On examination, the mouth, tongue and gums were foul and ſlimy; the tonſils and uvula covered with white ſpecks or ſloughs; the breath was hot and offenſive; the ſkin felt hot and pungent to the touch; the pulſe low and quick; a diarrhœa often attended, and the patients were in general much dejected.

Antimonial wine with cordials and nouriſhing diet ſucceeded beſt, till the ſloughs or ſpots were removed and ſeparated; then the bark completed the cure. When a diarrhœa accompanied this diſorder, I gave the mixture with ſucceſs.

In all diſorders where a gargle is neceſſary, I make uſe of the above mixture in preference to any other; and I find it ſpeedily cleanſes the tongue, gums and fauces, and ſweetens the breath.

Where lemons or limes cannot be had, vinegar or cremor tartar may be ſubſtituted in their room.

From what has been ſaid, it is evident, that the medicine is poſſeſſed of conſiderable antiſeptic powers, and its virtue

virtue conſiſts in correcting the peccant matter in the ſtomach and inteſtinal canal.

All the diſeaſes in which I have given it, had a putrid tendency. I ſhall be happy to hear of its ſucceſs in your weſtern hemiſphere.

And am, with eſteem,
SIR,
Your moſt humble ſervant,
WILLIAM WRIGHT.

N° XXXVII.

Medical Hiſtory of the Cortex Ruber, or Red Bark; communicated to JOHN MORGAN, *M. D. Profeſſor of the Theory and Practice of Phyſic at Philadelphia, and F. R. S. London, &c.*

Read Feb. 20, 1784. I HAVE lately received the following communications upon the Cortex Ruber, which I have found ſo efficacious in the cure of obſtinate remittent and bilious fevers, that I think it my duty to lay them before this ſociety, in hopes of ſo valuable a medicine being thereby better known, and introduced more generally into practice..

Extract of a Letter from Thomas S. Duché, *dated London, Auguſt* 9, 1783.

" I was lately at a lecture delivered at Guy's hoſpital, by Dr. Saunders, upon the cure of intermittent fevers, and obſerving the doctor ſpoke very much in favour of a new ſpecies of bark which he had introduced into the practice of phyſic, I procured a ſpecimen of it for you, thinking

thinking it might be agreeable to you to hear of any new improvements in the healing art. It is called *Red Bark*. According to his account it poſſeſſes ſo much virtue, and is of ſuch certain efficacy, that, compared with it, the common bark is an inert maſs. It contains a much larger portion of reſin, has a much ſtronger aromatic taſte than the common bark, and does not require half the quantity for a doſe. Amongſt other particulars, he mentioned the following proof of its ſuperior virtue, namely, that of this medicine, when adminiſtered in a ſimple cold infuſion, any given quantity is much ſtronger and effectual to remove the fever than a chemical extract from the ſame quantity of the other. I now ſend you a ſpecimen, by which you will be able to make a trial and form ſome judgment of its virtues."

T. S. Duché.

Soon after the receipt of the foregoing letter, I received the following valuable communication from Dr. George Davidſon of St. Lucia, which it affords me great pleaſure to lay before this ſociety.

St. Lucia, Auguſt 29, 1783.

To Doctor John Morgan, at Philadelphia.

Sir,

IF the ſubject upon which I have the honour to write you, ſhould be found to merit attention, and prove in any reſpect uſeful and advantageous to mankind, I ſhall eaſily ſtand excuſed in addreſſing you, perſonally unacquainted as I am.

I have by this opportunity ſent a ſmall ſpecimen of the Cinchona of this iſland, reſembling the Peruvian bark in its botanical character, and from the trial made here ſurpaſſing it in medical virtues. It is now nearly four years ſince the Caribæan bark was diſcovered upon the heights adjoining

adjoining Morne Fortuné, and introduced into practice by Dr. Young, physician to his Britannic majesty's troops. The freshness of the bark, the little attention bestowed in drying it, and the large doses in which it was exhibited, produced alarming fits of vomiting and purging, and deterred us, at that time, from the further prosecution of the subject, until the other day that a treatise upon the red bark, by Dr. Saunders of London, and a belief which we entertained that this was the same bark which he describes, induced us again to make a trial of it. Having properly dried it, and given it in the cold infusion with greater caution and in less doses than at the first essay, we are now happy in assuring the public, that in most instances it has not disappointed us. Still, however, notwithstanding the utmost care in drying it, in some cases it still seems to retain its emetic and purgative qualities, as the stomach and first passages, in complaints here, are loaded with a quantity of putrid bile. These are not its least valuable properties. It will, however, be necessary when these effects are produced, to check them afterwards by opiates.

With regard to its preparations: I have generally given it in the cold infusion either made with lime or cinnamon water. An extract made with spirits and water sits easily on the stomach and can be given in larger quantities.

In some late cases of tertians, where I have been called to the patient during the second fit; without watching for its going off, I have begun with this bark, which effectually cleansed the stomach and bowels, and paved the way for its future administration.

In putrid dysenteries, and in a remarkable species of dysentery, conjoined with an intermittent fever, which I have met with here, the bark has done more than all the remedies which I have seen employed. The purgative effects which it produced enabled us to throw it in earlier; the hardened scybula, the support of the disease, were removed

removed, the ftomach and bowels braced up, and, by the interpofition of opiates, the fpafms were removed.

Having fent feveral fpecimens of the bark for a trial to different parts of the continent of America, and particularly to my worthy friend Doctor Hall of Peterfburgh Virginia, I impatiently wait the refult of your trials, and will efteem myfelf particularly obliged by your communication. If you chufe, I fhall fend you fome of the young trees planted in tubs, with fome of the feeds.

Should it be found to anfwer my expectation, the pleafure refulting from the thoughts of having communicated fomething ufeful, will be to me ample enough recompenfe. I have the honor to be,

With the utmoft refpect,
Your moft obedient humble fervant,
GEORGE DAVIDSON.

P. S. Dr. Wright of Jamaica (in fifth vol. of medical commentaries,) defcribes a fpecies of Cinchona, with only one flower on a footftalk; the fame was likewife found at the Havanna. It differs in that particular from the old bark, which refembles the St. Lucia bark, in having feveral flowers on each footftalk.

The following is a Defcription of the Cinchona Caribæa Sanctæ Luciæ.

The tree is commonly found in ravines, near fprings, under the fhade of a larger tree. It delights in places well fhaded, and defended from the north-eaft trade wind. The foil is commonly a ftiff red earth with a clayey fubftratum; quantities of fmall beautiful chryftals, of a regular angular form, are found intermixed.

The tree is about the fize of the cherry tree; feldom exceeding the thicknefs of the thigh, and twenty-five feet in height.

The

The flowers begin to appear at the commencement of the rainy ſeaſon in beautiful tufts, upon pannicles branched out in threes and fours. I have never ſeen that ſpecies deſcribed by Jacquin and found at the Havannah, *pedunculis unifloris*.

Before the corolla is fully expanded, and the ſtamina make their appearance without the tube of the corolla, the flower is white, but it afterwards turns to a beautiful purple. Then dropping off, the germen enlarges to the ſize of an hazle-nut, oblong and round. It gradually dries, burſts in two, and ſcatters the ſeeds, which fall to the ground and again take root.

The wood of the tree is light, ſpongy, and fit for no uſeful purpoſe. It has not the bitter taſte of the bark. The leaves are very bitter, and the flowers, ſeeds, &c. ſeem to poſſeſs the bitterneſs and aſtringency in a more eminent degree.

An ounce of the bark in fine powder infuſed in a quart of cold water for twenty-four hours, and the infuſion afterwards filtered, appears higher coloured than a decoction made with double the quantity of the old bark. The colour which it ſtrikes with the *tinct. flor. martial.* and *ſal martis*, is likewiſe of a deeper black. The ſpirituous tincture is of a deep red colour, and ſtrikes a deep black by the addition of the preparations of iron.

The taſte of the Cinchona Caribæa is manifeſtly more aſtringent than the taſte of the old bark; an inference may therefore, *à priori*, be made, that its tonic powers are greater.

The quantity of reſin which it yields is much more conſiderable, and an extract made with both ſpirits and water, ſeems to poſſeſs the whole virtues of the bark.

N° XXXVIII.

A Letter from Dr. BENJAMIN FRANKLIN, *to* Mr. ALPHONSUS le ROY, *Member of ſeveral Academies, at Paris. Containing ſundry Maritime Obſervations.*

At Sea, on board the London Packet, Capt. Truxton, Auguſt 1785.

SIR,

Read Dec. 2, 1785.

YOUR learned writings on the navigation of the antients, which contain a great deal of curious information; and your very ingenious contrivances for improving the modern ſails (*voilure*) of which I ſaw with great pleaſure a ſucceſsful trial on the river Seine, have induced me to ſubmit to your conſideration and judgment, ſome thoughts I have had on the latter ſubject.

Thoſe mathematicians who have endeavoured to improve the ſwiftneſs of veſſels, by calculating to find the form of leaſt reſiſtance, ſeem to have conſidered a ſhip as a body moving through one fluid only, the water; and to have given little attention to the circumſtance of her moving through another fluid, the air. It is true that when a veſſel ſails right before the wind, this circumſtance is of no importance, becauſe the wind goes with her; but in every deviation from that courſe, the reſiſtance of the air is ſomething, and becomes greater in proportion as that deviation increaſes. I wave at preſent the conſideration of thoſe different degrees of reſiſtance given by the air to that part of the hull which is above water, and confine myſelf to that given to the ſails; for their motion through the air is reſiſted by the air, as the motion of the hull through the water is reſiſted by the water, though with leſs force as the air is a lighter fluid. And to ſimplify the diſcuſſion as much as poſſible, I would ſtate one ſituation only, to wit, that of the wind upon the beam, the ſhip's courſe being directly acroſs the wind; and I would ſuppoſe

ſuppoſe the ſail ſet in an angle of 45 degrees with the keel, as in the following figure; wherein

A B repreſents the body of the veſſel, CD the poſition of the ſail, EEE the direction of the wind, MM the line of motion. In obſerving this figure it will appear, that ſo much of the body of the veſſel as is immerſed in the water, muſt, to go forward, remove out of its way what water it meets with between the pricked lines FF. And the ſail, to go forward, muſt move out of its way all the air its whole dimenſion meets with between the pricked lines CG and DG. Thus both the fluids give reſiſtance to the motion, each in proportion to the quantity of matter contained in the dimenſion to be removed. And though the air is vaſtly lighter than the water, and therefore more eaſily removed, yet the dimenſion being much greater its effect is very conſiderable. Plate IV. Figure 1.

It is true that in the caſe ſtated, the reſiſtance given by the air between thoſe lines to the motion of the ſail is not apparent to the eye, becauſe the greater force of the wind which ſtrikes it in the direction EE, overpowers its effect, and keeps the ſail full in the curve a, a, a, a, a. But ſuppoſe the wind to ceaſe, and the veſſel in a calm to be impelled with the ſame ſwiftneſs by oars, the ſail would then appear filled in the contrary curve b, b, b, b, b, when prudent men would immediately perceive that the air reſiſted its motion, and would order it to be taken in.

Is there any poſſible means of diminiſhing this reſiſtance, while the ſame quantity of ſail is expoſed to the action of the wind, and therefore the ſame force obtained from it? I think there is, and that it may be done by dividing the ſail into a number of parts, and placing thoſe parts in a line one behind the other; thus inſtead of one ſail extending from C to D, figure 2, if four ſails containing together the ſame quantity of canvas, were placed as in figure 3, each having one quarter of the di-

 menſions

menſions of the great ſail, and expoſing a quarter of its ſurface to the wind, would give a quarter of the force; ſo that the whole force obtained from the wind would be the ſame, while the reſiſtance from the air would be nearly reduced to the ſpace between the pricked lines *a b* and *c d*, before the foremoſt ſail.

It may perhaps be doubted whether the reſiſtance from the air would be ſo diminiſhed; ſince poſſibly each of the following ſmall ſails having alſo air before it, which muſt be removed, the reſiſtance on the whole would be the ſame.

This is then a matter to be determined by experiment. I will mention one that I many years ſince made with ſucceſs for another purpoſe; and I will propoſe another ſmall one eaſily made. If that too ſucceeds, I ſhould think it worth while to make a larger, though at ſome expence, on a river boat; and perhaps time and the improvements experience will afford, may make it applicable with advantage to larger veſſels.

Having near my kitchen chimney a round hole of eight inches diameter, through which was a conſtant ſteady current of air, increaſing or diminiſhing only as the fire increaſed or diminiſhed, I contrived to place my jack ſo as to receive that current; and taking off the flyers, I fixed in their ſtead on the ſame pivot a round tin plate of near the ſame diameter with the hole; and having cut it in radial lines almoſt to the centre, ſo as to have ſix equal vanes, I gave to each of them the obliquity of forty-five degrees. They moved round, without the weight, by the impreſſion only of the current of air, but too ſlowly for the purpoſe of roaſting. I ſuſpected that the air ſtruck by the back of each vane might poſſibly by its reſiſtance retard the motion; and to try this, I cut each of them into two, and I placed the twelve, each having the ſame obliquity, in a line behind each other, when I perceived a great augmentation in its velocity, which encouraged me to di-

vide

vide them once more, and, continuing the ſame obliquity, I placed the twenty-four behind each other in a line, when the force of the wind being the ſame, and the ſurface of vane the ſame, they moved round with much greater rapidity, and perfectly anſwered my purpoſe.

The ſecond experiment that I propoſe, is, to take two playing cards of the ſame dimenſions, and cut one of them tranſverſely into eight equal pieces; then with a needle ſtring them upon two threads one near each end, and place them ſo upon the threads that, when hung up, they may be one exactly over the other, at a diſtance equal to their breadth, each in a horizontal poſition; and let a ſmall weight, ſuch as a bird-ſhot, be hung under them, to make them fall in a ſtraight line when let looſe. Suſpend alſo the whole card by threads from its four corners, and hang to it an equal weight, ſo as to draw it downwards when let fall, its whole breadth preſſing againſt the air. Let thoſe two bodies be attached, one of them to one end of a thread a yard long, the other to the other end. Extend a twine under the ceiling of a room, and put through it at thirty inches diſtance two pins bent in the form of fiſh-hooks. On theſe two hooks hang the two bodies, the thread that connects them extending parallel to the twine, which thread being cut, they muſt begin to fall at the ſame inſtant. If they take equal time in falling to the floor, it is a proof that the reſiſtance of the air is in both caſes equal. If the whole card requires a longer time, it ſhows that the ſum of the reſiſtances to the pieces of the cut card is not equal to the reſiſtance of the whole one*.

This principle ſo far confirmed, I would proceed to make a larger experiment, with a ſhallop, which I would rig in this manner.

A B is

* The motion of the veſſel made it inconvenient to try this ſimple experiment, at ſea, when the propoſal of it was written. But it has been tried ſince we came on ſhore, and ſucceeded as the other.

Plate IV. Figure 4. A B is a long boom, from which are hoifted feven jibs, a, b, c, d, e, f, g, each a feventh part of the whole dimenfions, and as much more as will fill the whole fpace when fet in an angle of forty-five degrees, fo that they may lap when going before the wind, and hold more wind when going large. Thus rigged, when going right before the wind, the boom fhould be brought at right angles with the keel, by means of the fheet ropes C D, and all the fails hauled flat to the boom.

Thefe pofitions of boom and fails to be varied as the wind quarters. But when the wind is on the beam, or when you would turn to windward, the boom is to be hauled right fore and aft, and the fails trimmed according as the wind is more or lefs againft your courfe.

It feems to me that the management of a fhallop fo rigged would be very eafy, the fails being run up and down feparately, fo that more or lefs fail may be made at pleafure; and I imagine, that there being full as much fail expofed to the force of the wind which impells the veffel in its courfe, as if the whole were in one piece, and the refiftance of the dead air againft the forefide of the fail being diminifhed, the advantage of fwiftnefs would be very confiderable; befides that the veffel would lie nearer the wind.

Since we are on the fubject of improvements in navigation, permit me to detain you a little longer with a fmall relative obfervation. Being, in one of my voyages, with ten merchant-fhips under convoy of a frigate at anchor in Torbay, waiting for a wind to go to the weftward; it came fair, but brought in with it a confiderable fwell. A fignal was given for weighing, and we put to fea all together; but three of the fhips left their anchors, their cables parting juft as the anchors came a-peak. Our cable held, and we got up our anchor; but the fhocks the fhip felt before the anchor got loofe from the ground, made me reflect on what might poffibly have caufed the breaking

ing of the other cables; and I imagined it might be the ſhort bending of the cable juſt without the hauſe-hole, from a horizontal to an almoſt vertical poſition, and the ſudden violent jerk it receives by the riſing of the head of the ſhip on the ſwell of a wave while in that poſition. For example, ſuppoſe a veſſel hove up ſo as to have her head nearly over her anchor, which ſtill keeps its hold, perhaps in a tough bottom; if it were calm, the cable ſtill out would form nearly a perpendicular line, meaſuring the diſtance between the hauſe-hole and the anchor; but if there is a ſwell, her head in the trough of the ſea will fall below the level, and when lifted on the wave will be as much above it. In the firſt caſe the cable will hang looſe and bend perhaps as in figure 5. In the ſecond caſe figure 6, the cable will be drawn ſtraight with a jerk, muſt ſuſtain the whole force of the riſing ſhip, and muſt either looſen the anchor, reſiſt the riſing force of the ſhip, or break. But why does it break at the hauſe-hole?

Let us ſuppoſe it a cable of three inches diameter, and repreſented by figure 7. If this cable is to be bent round the corner A, it is evident that either the part of the triangle contained between the letters a, b, c, muſt ſtretch conſiderably, and thoſe moſt that are neareſt the ſurface; or that the parts between d, e, f, muſt be compreſſed; or both, which moſt probably happens. In this caſe the lower half of the thickneſs affords no ſtrength againſt the jerk, it not being ſtrained, the upper half bears the whole, and the yarns near the upper ſurface being firſt and moſt ſtrained, break firſt, and the next yarns follow; for in this bent ſituation they cannot bear the ſtrain all together, and each contribute its ſtrength to the whole, as they do when the cable is ſtrained in a ſtraight line.

To remedy this, methinks it would be well to have a kind of large pulley wheel, fixed in the hauſe-hole, ſuppoſe of two feet diameter, over which the cable might paſs; and being there bent gradually to the round of the wheel,

wheel, would thereby be more equally ſtrained, and better able to bear the jerk, which may ſave the anchor, and by that means in the courſe of the voyage may happen to ſave the ſhip.

One maritime obſervation more ſhall finiſh this letter. I have been a reader of news-papers now near ſeventy years, and I think few years paſs without an account of ſome veſſel met with at ſea, with no ſoul living on board, and ſo many feet of water in her hold, which veſſel has nevertheleſs been ſaved and brought into port: and when not met with at ſea, ſuch forſaken veſſels have often come aſhore on ſome coaſt. The crews who have taken to their boats and thus abandoned ſuch veſſels, are ſometimes met with and taken up at ſea by other ſhips, ſometimes reach a coaſt, and are ſometimes never heard of. Thoſe that give an account of quitting their veſſels, generally ſay, that ſhe ſprung a leak, that they pumped for ſome time, that the water continued to riſe upon them, and that deſpairing to ſave her, they had quitted her leſt they ſhould go down with her. It ſeems by the event that this fear was not always well founded, and I have endeavoured to gueſs at the reaſon of the people's too haſty diſcouragement.

When a veſſel ſprings a leak near her bottom, the water enters with all the force given by the weight of the column of water, without, which force is in proportion to the difference of level between the water without and that within. It enters therefore with more force at firſt, and in greater quantity, than it can afterwards when the water within is higher. The bottom of the veſſel too is narrower, ſo that the ſame quantity of water coming into that narrow part, riſes faſter than when the ſpace for it to flow in is larger. This helps to terrify. But as the quantity entering is leſs and leſs as the ſurfaces without and within become more nearly equal in height, the pumps that could not keep the water from riſing at firſt, might afterwards be able to prevent its riſing higher, and the people might

might have remained on board in ſafety, without hazarding themſelves in an open boat on the wide ocean. (Fig. 8.)

Beſides the greater equality in the height of the two ſurfaces, there may ſometimes be other cauſes that retard the farther ſinking of a leaky veſſel. The riſing water within may arrive at quantities of light wooden work, empty cheſts, and particularly empty water caſks, which if fixed ſo as not to float themſelves may help to ſuſtain her. Many bodies which compoſe a ſhip's cargo may be ſpecifically lighter than water, all theſe when out of water are an additional weight to that of the ſhip, and ſhe is in proportion preſſed deeper into the water; but as ſoon as theſe bodies are immerſed, they weigh no longer on the ſhip, but on the contrary, if fixed, they help to ſupport her, in proportion as they are ſpecifically lighter than the water. And it ſhould be remembered, that the largeſt body of a ſhip may be ſo balanced in the water, that an ounce leſs or more of weight may leave her at the ſurface or ſink her to the bottom. There are alſo certain heavy cargoes, that when the water gets at them are continually diſſolving, and thereby lightening the veſſel, ſuch as ſalt and ſugar. And as to water caſks mentioned above, ſince the quantity of them muſt be great in ſhips of war where the number of men conſume a great deal of water every day, if it had been made a conſtant rule to bung them up as faſt as they were emptied, and to diſpoſe the empty caſks in proper ſituations, I am perſuaded that many ſhips which have been ſunk in engagements, or have gone down afterwards, might with the unhappy people have been ſaved; as well as many of thoſe which in the laſt war foundered, and were never heard of. While on this topic of ſinking, one cannot help recollecting the well known practice of the Chineſe, to divide the hold of a great ſhip into a number of ſeparate chambers by partitions tight caulked, (of which you gave a model in your boat upon the Seine) ſo that if a leak ſhould ſpring in one of them the others are not affected by it; and though that chamber ſhould fill to a level with the ſea, it would not be ſufficient to ſink the veſſel. We have not imitated this practice. Some little diſadvantage it might occaſion in the ſtowage is perhaps one reaſon, though that I think might be more than compenſated by an abatement in the inſurance that would be reaſonable, and by a higher price taken of paſſengers, who would rather prefer going in ſuch a veſſel. But our ſeafaring people are brave, deſpiſe danger, and reject ſuch precautions of ſafety, being cowards only in one ſenſe, that of *fearing* to be *thought afraid.*

I promiſed to finiſh my letter with the laſt obſervation, but the garrulity of the old man has got hold of me, and as I may never have another occaſion of writing on this ſubject, I think I may as well now, once for all, empty my nautical budget, and give you all the thoughts that have in my various long voyages occurred to me relating to navigation. I am ſure that in you they will meet with a candid judge, who will excuſe my miſtakes on account of my good intention.

There are ſix accidents that may occaſion the loſs of ſhips at ſea. We have conſidered one of them, that of foundering by a leak. The other five are, 1. Overſetting by ſudden flaws of wind, or by carrying ſail beyond the bearing. 2. Fire by accident or careleſſneſs. 3. A heavy ſtroke of lightning, making a breach in the ſhip, or firing the powder. 4. Meeting and ſhocking with other ſhips in the night. 5. Meeting in the night with iſlands of ice.

To that of overſetting, privateers in their firſt cruize have, as far as has fallen within my knowledge or information, been more ſubject than any other kind of veſſels. The double deſire of being able to overtake a weaker flying enemy, or to eſcape when purſued by a ſtronger, has induced the owners to overmaſt their cruizers, and to ſpread too much canvas; and the great number of men, many of them

them not ſeamen, who being upon deck when a ſhip heels ſuddenly are huddled down to leeward, and increaſe by their weight the effect of the wind. This therefore ſhould be more attended to and guarded againſt, eſpecially as the advantage of lofty maſts is problematical. For the upper ſails have greater power to lay a veſſel more on her ſide, which is not the moſt advantageous poſition for going ſwiftly through the water. And hence it is that veſſels which have loſt their lofty maſts, and been able to make little more ſail afterwards than permitted the ſhip to ſail upon an even keel, have made ſo much way, even under jury maſts, as to ſurprize the mariners themſelves. But there is beſides, ſomething in the modern form of our ſhips that ſeems as if calculated expreſsly to allow their overſetting more eaſily. The ſides of a ſhip inſtead of ſpreading out as they formerly did in the upper works, are of late years turned in, ſo as to make the body nearly round, and more reſembling a caſk. I do not know what the advantages of this conſtruction are, except that ſuch ſhips are not ſo eaſily boarded. To me it ſeems a contrivance to have leſs room in a ſhip at nearly the ſame expence. For it is evident that the ſame timber and plank conſumed in raiſing the ſides from a to b, and from d to c, would have raiſed them from a to e, and from d to f, fig. 9. In this form all the ſpaces between e, a, b, and c, d, f, would have been gained, the deck would have been larger, the men would have had more room to act, and not have ſtood ſo thick in the way of the enemy's ſhot; and the veſſel the more ſhe was laid down on her ſide, the more bearing ſhe would meet with, and more effectual to ſupport her, as being farther from the center. Whereas in the preſent form, her ballaſt makes the chief part of her bearing, without which ſhe would turn in the ſea almoſt as eaſily as a barrel. More ballaſt by this means becomes neceſſary, and that ſinking a veſſel deeper in the water occaſions more reſiſtance to her going through it. The

Bermudian ſloops ſtill keep with advantage to the old ſpreading form. The iſlanders in the great Pacific ocean, though they have no large ſhips, are the moſt expert boat-ſailors in the world, navigating that ſea ſafely with their proas, which they prevent overſetting by various means. Their ſailing proas for this purpoſe have outriggers generally to windward, above the water, on which one or more men are placed to move occaſionally further from or nearer to the veſſel as the wind freſhens or ſlackens. But ſome have their outriggers to leeward, which reſting on the water ſupport the boat ſo as to keep her upright when preſſed down by the wind. Their boats moved by oars or rather by paddles, are, for long voyages, fixed two together by croſs bars of wood that keep them at ſome diſtance from each other, and ſo render their overſetting next to impoſſible. How far this may be practicable in larger veſſels, we have not yet ſufficient experience. I know of but one trial made in Europe, which was about one hundred years ſince by, Sir William Petty. He built a double veſſel, to ſerve as a pacquet boat between England and Ireland. Her model ſtill exiſts in the muſeum of the Royal Society, where I have ſeen it. By the accounts we have of her, ſhe anſwered well the purpoſe of her conſtruction, making ſeveral voyages; and though wrecked at laſt by a ſtorm, the misfortune did not appear owing to her particular conſtruction, ſince many other veſſels of the common form were wrecked at the ſame time. The advantage of ſuch a veſſel is: That ſhe needs no ballaſt, therefore ſwims either lighter or will carry more goods; and that paſſengers are not ſo much incommoded by her rolling: to which may be added, that if ſhe is to defend herſelf by her cannon, they will probably have more effect, being kept more generally in a horizontal poſition, than thoſe in common veſſels. I think however that it would be an improvement of that model, to make the ſides

which

which are oppofed to each other perfectly parallel, though the other fides are formed as in common thus, figure 10.

The building of a double fhip would indeed be more expenfive in proportion to her burthen; and that perhaps is fufficient to difcourage the method.

The accident of fire is generally well guarded againft by the prudent captain's ftrict orders againft fmoking between decks, or carrying a candle there out of a lanthorn. But there is one dangerous practice which frequent terrible accidents have not yet been fufficient to abolifh; that of carrying ftore-fpirits to fea in cafks. Two large fhips, the Seraphis and the Duke of Athol, one an Eaft-Indiaman, the other a frigate, have been burnt within thefe two laft years, and many lives miferably deftroyed, by drawing fpirits out of a cafk near a candle. It is high time to make it a general rule, that all the fhip's ftore of fpirits fhould be carried in bottles.

The misfortune by a ftroke of lightning I have in my former writings endeavoured to fhow a method of guarding againft, by a chain and pointed rod, extending, when run up, from above the top of the maft to the fea. Thefe inftruments are now made and fold at a reafonable price by *Nairne and Co.* in London, and there are feveral inftances of fuccefs attending the ufe of them. They are kept in a box, and may be run up and fixed in about five minutes, on the apparent approach of a thunder guft.

Of the meeting and fhocking with other fhips in the night, I have known two inftances in voyages between London and America. In one both fhips arrived though much damaged, each reporting their belief that the other muft have gone to the bottom. In the other, only one got to port; the other was never afterwards heard of. Thefe inftances happened many years ago, when the commerce between Europe and America was not a tenth part of what it is at prefent, fhips of courfe thinner fcattered, and the chance of meeting proportionably lefs. It has long been

been the practice to keep a *look-out before* in the channel, but at sea it has been neglected. If it is not at present thought worth while to take that precaution, it will in time become of more consequence; since the number of ships at sea is continually augmenting. A drum frequently beat or a bell rung in a dark night, might help to prevent such accidents.

Islands of ice are frequently seen off the banks of Newfoundland, by ships going between North-America and Europe. In the day-time they are easily avoided, unless in a very thick fog. I remember two instances of ships running against them in the night. The first lost her bowsprit, but received little other damage. The other struck where the warmth of the sea had wasted the ice next to it, and a part hung over above. This perhaps saved her, for she was under great way; but the upper part of the cliff taking her foretopmast, broke the shock, though it carried away the mast. She disengaged herself with some difficulty, and got safe into port; but the accident shows the possibility of other ships being wrecked and sunk by striking those vast masses of ice, of which I have seen one that we judged to be seventy feet high above the water, consequently eight times as much under water; and it is another reason for keeping a good *look-out before*, though far from any coast that may threaten danger.

It is remarkable that the people we consider as savages, have improved the art of sailing- and rowing-boats in several points beyond what we can pretend to. We have no sailing boats equal to the flying proas of the south seas, no rowing or paddling boat equal to that of the Greenlanders for swiftness and safety. The birch canoes of the North-American Indians have also some advantageous properties. They are so light that two men may carry one of them over land, which is capable of carrying a dozen upon the water; and in heeling they are not so subject to take in water as our boats, the sides of which are

lowest

loweſt in the middle where it is moſt likely to enter, this being higheſt in that part, as in figure 11.

The Chineſe are an enlightened people, the moſt antiently civilized of any exiſting, and their arts are antient, a preſumption in their favour: their method of rowing their boats differs from ours, the oars being worked either two a-ſtern as we ſcull, or on the ſides with the ſame kind of motion, being hung parallel to the keel on a rail and always acting in the water, not perpendicular to the ſide as ours are, nor lifted out at every ſtroke, which is a loſs of time, and the boat in the interval loſes motion. They ſee our manner, and we theirs, but neither are diſpoſed to learn of or copy the other.

To the ſeveral means of moving boats mentioned above, may be added the ſingular one lately exhibited at Javelle, on the Seine below Paris, where a clumſy boat was moved acroſs that river in three minutes by rowing, not in the water, but in the air, that is, by whirling round a ſet of windmill vanes fixed to a horizontal axis, parallel to the keel, and placed at the head of the boat. The axis was bent into an elbow at the end, by the help of which it was turned by one man at a time. I ſaw the operation at a diſtance. The four vanes appeared to be about five feet long, and perhaps two and a half wide. The weather was calm. The labour appeared to be great for one man, as the two ſeveral times relieved each other. But the action upon the air by the oblique ſurfaces of the vanes muſt have been conſiderable, as the motion of the boat appeared tolerably quick going and returning; and ſhe returned to the ſame place from whence ſhe firſt ſet out, notwithſtanding the current. This machine is ſince applied to the moving of air balloons: An inſtrument ſimilar may be contrived to move a boat by turning under water.

Several mechanical projectors have at different times propoſed to give motion to boats, and even to ſhips, by means of circular rowing, or paddles placed on the circum-

ference

ference of wheels to be turned conſtantly on each ſide of the veſſel; but this method, though frequently tried, has never been found ſo effectual as to encourage a continuance of the practice. I do not know that the reaſon has hitherto been given. Perhaps it may be this, that great part of the force employed contributes little to the motion. For inſtance, (fig. 12.) of the four paddles a, b, c, d, all under water, and turning to move a boat from X to Y, c has the moſt power, b nearly though not quite as much, their motion being nearly horizontal; but the force employed in moving a, is conſumed in preſſing almoſt downright upon the water till it comes to the place of b; and the force employed in moving d is conſumed in lifting the water till d arrives at the ſurface; by which means much of the labour is loſt. It is true, that by placing the wheels higher out of the water, this waſte labour will be diminiſhed in a calm, but where a ſea runs, the wheels muſt unavoidably be often dipt deep in the waves, and the turning of them thereby rendered very laborious to little purpoſe.

Among the various means of giving motion to a boat, that of M. Bernoulli appears one of the moſt ſingular, which was to have fixed in the boat a tube in the form of an L, the upright part to have a funnel-kind of opening at top, convenient for filling the tube with water; which deſcending and paſſing through the lower horizontal part, and iſſuing in the middle of the ſtern, but under the ſurface of the river, ſhould puſh the boat forward. There is no doubt that the force of the deſcending water would have a conſiderable effect, greater in proportion to the height from which it deſcended; but then it is to be conſidered, that every bucket-full pumped or dipped up into the boat, from its ſide or through its bottom, muſt have its *vis inertiæ* overcome ſo as to receive the motion of the boat, before it can come to give motion by its deſcent; and that will be a deduction from the moving power.

To

To remedy this, I would propofe the addition of another fuch L pipe, and that they fhould ftand back to back in the boat thus, figure 13. the forward one being worked as a pump, and fucking in the water at the head of the boat, would draw it forward while pufhed in the fame direction by the force at the ftern. And after all it fhould be calculated whether the labour of pumping would be lefs than that of rowing. A fire-engine might poffibly in fome cafes be applied in this operation with advantage.

Perhaps this labour of raifing water might be fpared, and the whole force of a man applied to the moving of a boat by the ufe of air inftead of water; fuppofe the boat conftructed in this form, figure 14. A, a tube round or fquare of two feet diameter, in which a pifton may move up and down. The pifton to have valves in it, opening inwards to admit air when the pifton rifes; and fhutting, when it is forced down by means of the lever B turning on the center C. The tube to have a valve D, to open when the pifton is forced down, and let the air pafs out at E, which ftriking forcibly againft the water abaft muft pufh the boat forward. If there is added an air-veffel F properly valved and placed, the force would continue to act while a frefh ftroke is taken with the lever. The boatman might ftand with his back to the ftern, and putting his hands behind him, work the motion by taking hold of the crofs bar at B, while another fhould fteer; or if he had two fuch pumps, one on each fide of the ftern, with a lever for each hand, he might fteer himfelf by working occafionally more or harder with either hand, as watermen now do with a pair of fculls. There is no pofition in which the body of a man can exert more ftrength than in pulling right upwards.

To obtain more fwiftnefs, greafing the bottom of a veffel is fometimes ufed, and with good effect. I do not know that any writer has hitherto attempted to explain this. At firft fight one would imagine, that though the friction of a hard body fliding on another hard body, and the refiftance occafioned by that friction, might be diminifhed by putting greafe between them, yet that a body fliding on a fluid, fuch as water, fhould have no need of nor receive any advantage from fuch greafing. But the fact is not difputed. And the reafon perhaps may be this. The particles of water have a mutual attraction, called the attraction of adhefion. Water alfo adheres to wood, and to many other fubftances, but not to greafe: On the contrary they have a mutual repulfion, fo that it is a queftion whether when oil is poured on water, they ever actually touch each other; for a drop of oil upon water, inftead of fticking to the fpot where it falls, as it would if it fell on a looking-glafs, fpreads inftantly to an immenfe diftance in a film extremely thin, which it could not eafily do if it touched and rubbed or adhered even in a fmall degree to the furface of the water. Now the adhefive force of water to itfelf, and to other fubftances, may be eftimated from the weight of it neceffary to feparate a drop, which adheres, while growing, till it has weight enough to force the feparation and break the drop off. Let us fuppofe the drop to be the fize of a pea, then there will be as many of thefe adhefions as there are drops of that fize touching the bottom of a veffel, and thefe muft be broken by the moving power, every ftep of her motion that amounts to a drop's breadth: And there being no fuch adhefions to break between the water and a greafed bottom, may occafion the difference.

So much refpecting the motion of veffels. But we have fometimes occafion to ftop their motion; and if a bottom is near enough we can caft anchor: Where there are no foundings, we have as yet no means to prevent driving in a ftorm, but by lying-to, which ftill permits driving at the rate of about two miles an hour; fo that in a ftorm continuing fifty hours, which is not an uncommon cafe, the fhip may drive one hundred miles out of her courfe; and fhould fhe in that diftance meet with a lee fhore, fhe may be loft.

To

To prevent this driving to leeward in deep water, a ſwimming anchor is wanting, which ought to have theſe properties.

1. It ſhould have a ſurface ſo large as being at the end of a hauſer in the water, and placed perpendicularly, ſhould hold ſo much of it, as to bring the ſhip's head to the wind, in which ſituation the wind has leaſt power to drive her.

2. It ſhould be able by its reſiſtance to prevent the ſhip's receiving way.

3. It ſhould be capable of being ſituated below the heave of the ſea, but not below the undertow.

4. It ſhould not take up much room in the ſhip.

5. It ſhould be eaſily thrown out, and put into its proper ſituation.

6. It ſhould be eaſy to take in again, and ſtow away.

An ingenious old mariner whom I formerly knew, propoſed as a ſwimming anchor for a large ſhip to have a ſtem of wood twenty-five feet long and four inches ſquare, with four boards of 18, 16, 14, and 12, feet long, and one foot wide, the boards to have their ſubſtance thickened ſeveral inches in the middle by additional wood, and to have each a four inch ſquare hole through its middle, to permit its being ſlipt on occaſionally upon the ſtem, and at right angles with it; where all being placed and fixed at four feet diſtance from each other, it would have the appearance of the old mathematical inſtrument called a foreſtaff. This thrown into the ſea, and held by a hauſer veered out to ſome length, he conceived would bring a veſſel up, and prevent her driving, and when taken in might be ſtowed away by ſeparating the boards from the ſtem. Figure 15. Probably ſuch a ſwimming anchor would have ſome good effect, but it is ſubject to this objection, that lying on the ſurface of the ſea, it is liable to be hove forward by every wave, and thereby give ſo much leave for the ſhip to drive.

Two machines for this purpofe have occurred to me; which though not fo fimple as the above, I imagine would be more effectual, and more eafily manageable. I will endeavour to defcribe them, that they may be fubmitted to your judgment, whether either would be ferviceable; and if they would, to which we fhould give the preference.

The firft is to be formed, and to be ufed in the water on almoft the fame principles with thofe of a paper kite ufed in the air. Only as the paper kite rifes in the air, this is to defcend in the water. Its dimenfions will be different for fhips of different fize.

To make one of fuppofe fifteen feet high; take a fmall fpar of that length for the back-bone, AB, figure 16, a fmaller of half that length C D, for the crofs piece. Let thefe be united by a bolt at E, yet fo as that by turning on the bolt they may be laid parallel to each other. Then make a fail of ftrong canvas, in the fhape of figure 17. To form this, without wafte of fail-cloth, few together pieces of the proper length, and for half the breadth, as in figure 18, then cut the whole in the diagonal lines a, b, c, and turn the piece F fo as to place its broad part oppofite to that of the piece G, and the piece H in like manner oppofite to I, which when all fewed together will appear as in figure 17. This fail is to be extended on the crofs of figure 16, the top and bottom points well fecured to the ends of the long fpar; the two fide points d, e, faftened to the ends of two cords, which coming from the angle of the loop (which muft be fimilar to the loop of a kite) pafs through two rings at the ends of the fhort fpar, fo as that on pulling upon the loop the fail will be drawn to its extent. The whole may, when aboard, be furled up, as in figure 19, having a rope from its broad end, to which is tied a bag of ballaft for keeping that end downwards when in the water, and at the other end another rope with an empty keg at its end to float on the furface; this

this rope long enough to permit the kite's defcending into the undertow, or if you pleafe lower into ftill water. It fhould be held by a haufer. To get it home eafily, a fmall loofe rope may be veered out with it, fixed to the keg. Hauling on that rope will bring the kite home with fmall force, the refiftance being fmall as it will then come endways.

It feems probable that fuch a kite at the end of a long haufer would keep a fhip with her head to the wind, and refifting every tug, would prevent her driving fo faft as when her fide is expofed to it, and nothing to hold her back. If only half the driving is prevented, fo as that fhe moves but fifty miles inftead of the hundred during a ftorm, it may be fome advantage, both in holding fo much diftance as is faved, and in keeping from a lee fhore. If fingle canvas fhould not be found ftrong enough to bear the tug without fplitting, it may be doubled, or ftrengthened by a netting behind it, reprefented by figure 20.

The other machine for the fame purpofe, is to be made more in the form of an umbrella, as reprefented, figure 21. The ftem of the umbrella a fquare fpar of proper length, with four moveable arms, of which two are reprefented C, C, figure 22. Thefe arms to be fixed in four joint cleats, as D, D, &c. one on each fide of the fpar, but fo as that the four arms may open by turning on a pin in the joint. When open they form a crofs, on which a four fquare canvas fail is to be extended, its corners faftened to the ends of the four arms. Thofe ends are alfo to be ftayed by ropes faftened to the ftem or fpar, fo as to keep them fhort of being at right angles with it: And to the end of one of the arms fhould be hung the fmall bag of ballaft, and to the end of the oppofite arm the empty keg. This on being thrown into the fea, would immediately open; and when it had performed its function, and the ftorm over, a fmall rope from its other end being pulled on, would turn it, clofe it, and draw it eafily home to the fhip. This machine feems more fimple in its operation,

ration, and more eaſily manageable than the firſt, and perhaps may be as effectual.*

Veſſels are ſometimes retarded, and ſometimes forwarded in their voyages, by currents at ſea, which are often not perceived. About the year 1769 or 70, there was an application made by the board of cuſtoms at Boſton, to the lords of the treaſury in London, complaining that the packets between Falmouth and New-York, were generally a fortnight longer in their paſſages, than merchant ſhips from London to Rhode-Iſland, and propoſing that for the future they ſhould be ordered to Rhode-Iſland inſtead of New-York. Being then concerned in the management of the American poſt-office, I happened to be conſulted on the occaſion; and it appearing ſtrange to me that there ſhould be ſuch a difference between two places, ſcarce a day's run aſunder, eſpecially when the merchant ſhips are generally deeper laden, and more weakly manned than the packets, and had from London the whole length of the river and channel to run before they left the land of England, while the packets had only to go from Falmouth, I could not but think the fact miſunderſtood or miſrepreſented. There happened then to be in London, a Nantucket ſea-captain of my acquaintance, to whom I communicated the affair. He told me he believed the fact might be true; but the difference was owing to this, that the Rhode-Iſland captains were acquainted with the gulf ſtream, which thoſe of the Engliſh packets were not. We are well acquainted with that ſtream, ſays he, becauſe in our purſuit of whales, which keep near the ſides of it, but are not to be met with in it, we run down along the ſides, and frequently croſs it to change our ſide: and in croſſing it have ſometimes met and ſpoke with thoſe packets, who were in the middle of it, and ſtemming it. We have informed them that they were

* Captain Truxton, on board whoſe ſhip this was written, has executed this propoſed machine; he has given ſix arms to the umbrella, they are joined to the ſtem by iron hinges, and the canvas is double. He has taken it with him to China. February 1786.

were ſtemming a current, that was againſt them to the value of three miles an hour; and adviſed them to croſs it and get out of it; but they were too wiſe to be counſelled by ſimple American fiſhermen. When the winds are but light, he added, they are carried back by the current more than they are forwarded by the wind: and if the wind be good, the ſubtraction of 70 miles a day from their courſe is of ſome importance. I then obſerved that it was a pity no notice was taken of this current upon the charts, and requeſted him to mark it out for me, which he readily complied with, adding directions for avoiding it in ſailing from Europe to North-America. I procured it to be engraved by order from the general poſt-office, on the old chart of the Atlantic, at Mount and Page's, Tower-hill; and copies were ſent down to Falmouth for the captains of the packets, who ſlighted it however; but it is ſince printed in France, of which edition I hereto annex a copy.

This ſtream is probably generated by the great accumulation of water on the eaſtern coaſt of America between the tropics, by the trade winds which conſtantly blow there. It is known that a large piece of water ten miles broad and generally only three feet deep, has by a ſtrong wind had its waters driven to one ſide and ſuſtained ſo as to become ſix feet deep, while the windward ſide was laid dry. This may give ſome idea of the quantity heaped up on the American coaſt, and the reaſon of its running down in a ſtrong current through the iſlands into the bay of Mexico, and from thence iſſuing through the gulph of Florida, and proceeding along the coaſt to the banks of Newfoundland, where it turns off towards and runs down through the Weſtern iſlands. Having ſince croſſed this ſtream ſeveral times in paſſing between America and Europe, I have been attentive to ſundry circumſtances relating to it, by which to know when one is in it; and beſides the gulph weed with which it is interſperſed, I find that

that it is always warmer than the ſea on each ſide of it, and that it does not ſparkle in the night: I annex hereto the obſervations made with the thermometer in two voyages, and poſſibly may add a third. It will appear from them, that the thermometer may be an uſeful inſtrument to a navigator, ſince currents coming from the northward into ſouthern ſeas, will probably be found colder than the water of thoſe ſeas, as the currents from ſouthern ſeas into northern are found warmer. And it is not to be wondered that ſo vaſt a body of deep warm water, ſeveral leagues wide, coming from between the tropics and iſſuing out of the gulph into the northern ſeas, ſhould retain its warmth longer than the twenty or thirty days required to its paſſing the banks of Newfoundland. The quantity is too great, and it is too deep to be ſuddenly cooled by paſſing under a cooler air. The air immediately over it, however, may receive ſo much warmth from it as to be rarified and riſe, being rendered lighter than the air on each ſide of the ſtream; hence thoſe airs muſt flow in to ſupply the place of the riſing warm air, and meeting with each other, form thoſe tornados and water-ſpouts frequently met with, and ſeen near and over the ſtream; and as the vapour from a cup of tea in a warm room, and the breath of an animal in the ſame room, are hardly viſible, but become ſenſible immediately when out in the cold air, ſo the vapour from the gulph ſtream, in warm latitudes is ſcarcely viſible, but when it comes into the cool air from Newfoundland, it is condenſed into the fogs, for which thoſe parts are ſo remarkable.

The power of wind to raiſe water above its common level in the ſea, is known to us in America, by the high tides occaſioned in all our ſea-ports when a ſtrong northeaſter blows againſt the gulph ſtream.

The concluſion from theſe remarks is, that a veſſel from Europe to North-America may ſhorten her paſſage by avoiding to ſtem the ſtream, in which the thermometer will be be very uſeful; and a veſſel from America to Europe may do the ſame by the ſame means of keeping in it. It may have often happened accidentally, that voyages have been ſhortened by theſe circumſtances. It is well to have the command of them.

But may there not be another cauſe, independent of winds and currents, why paſſages are generally ſhorter from America to Europe than from Europe to America? This queſtion I formerly conſidered in the following ſhort paper.

On board the Pennſylvania Packet, Capt. Oſborne,
At ſea, April 5, 1775.

"Suppoſe a ſhip to make a voyage eaſtward from a place in lat. 40° north, to a place in lat. 50° north, diſtance in longitude 75 degrees.

"In ſailing from 40 to 50, ſhe goes from a place where a degree of longitude is about eight miles greater than in the place ſhe is going to. A degree is equal to four minutes of time; conſequently the ſhip in the harbour ſhe leaves, partaking of the diurnal motion of the earth, moves two miles in a minute faſter, than when in the port ſhe is going to; which is 120 miles in an hour.

"This motion in a ſhip and cargo is of great force; and if ſhe could be lifted up ſuddenly from the harbour in which ſhe lay quiet, and ſet down inſtantly in the latitude of the port ſhe was bound to, though in a calm, that force contained in her would make her run a great way at a prodigious rate. This force muſt be loſt gradually in her voyage, by gradual impulſe againſt the water, and probably thence ſhorten the voyage. Query, In returning does the contrary happen, and is her voyage thereby retarded and lengthened?"*

Would it not be a more ſecure method of planking ſhips, if inſtead of thick ſingle planks laid horizontally, we were

 to

* Since this paper was read at the Society, an ingenious member, Mr. Patterſon, has convinced the writer that the returning voyage would not, from this cauſe, be retarded.

to uſe planks of half the thickneſs, and lay them double and acroſs each other as in figure 23? To me it ſeems that the difference of expence would not be conſiderable, and that the ſhip would be both tighter and ſtronger.

The ſecuring of the ſhip is not the only neceſſary thing; ſecuring the health of the ſailors, a brave and valuable order of men, is likewiſe of great importance. With this view the methods ſo ſucceſsfully practiſed by Captain Cook in his long voyages, cannot be too cloſely ſtudied or carefully imitated. A full account of thoſe methods is found in Sir John Pringle's ſpeech, when the medal of the Royal Society was given to that illuſtrious navigator. I am glad to ſee in his laſt voyage that he found the means effectual which I had propoſed for preſerving flour, bread, &c. from moiſture and damage. They were found dry and good after being at ſea four years. The method is deſcribed in my printed works, page 452, fifth edition. In the ſame, page 469, 470, is propoſed a means of allaying thirſt in caſe of want of freſh water. This has ſince been practiſed in two inſtances with ſucceſs. Happy if their hunger, when the other proviſions are conſumed, could be relieved as commodiouſly; and perhaps in time this may be found not impoſſible. An addition might be made to their preſent vegetable proviſion, by drying various roots in ſlices by the means of an oven. The ſweet potatoe of America and Spain, is excellent for this purpoſe. Other potatoes, with carrots, parſnips and turnips, might be prepared and preſerved in the ſame manner.

With regard to make-ſhifts in caſes of neceſſity, ſeamen are generally very ingenious themſelves. They will excuſe however the mention of two or three. If they happen in any circumſtance, ſuch as after ſhipwreck, taking to their boat, or the like, to want a compaſs, a fine ſewing-needle laid on clear water in a cup will generally point to the north, moſt of them being a little magnetical, or may be made ſo by being ſtrongly rubbed or hammer-

ed,

ed, lying in a north and ſouth direction. If their needle is too heavy to float by itſelf, it may be ſupported by little pieces of cork or wood. A man who can ſwim, may be aided in a long traverſe by his handkerchief formed into a kite, by two croſs ſticks extending to the four corners; which being raiſed in the air, when the wind is fair and freſh, will tow him along while lying on his back. Where force is wanted to move a heavy body, and there are but few hands and no machines, a long and ſtrong rope may make a powerful inſtrument. Suppoſe a boat is to be drawn up on a beach, that ſhe may be out of the ſurf, a ſtake drove into the beach where you would have the boat drawn; and another to faſten the end of the rope to, which comes from the boat, and then applying what force you have to pull upon the middle of the rope at right angles with it, the power will be augmented in proportion to the length of rope between the poſts. The rope being faſtened to the ſtake A, and drawn upon in the direction C D, will ſlide over the ſtake B; and when the rope is bent to the angle A D B, repreſented by the pricked line in figure 24, the boat will be at B.

Some ſailors may think the writer has given himſelf unneceſſary trouble in pretending to adviſe them; for they have a little repugnance to the advice of landmen, whom they eſteem ignorant and incapable of giving any worth notice; though it is certain that moſt of their inſtruments were the invention of landmen. At leaſt the firſt veſſel ever made to go on the water was certainly ſuch. I will therefore add only a few words more, and they ſhall be addreſſed to paſſengers.

When you intend a long voyage, you may do well to keep your intention as much as poſſible a ſecret, or at leaſt the time of your departure; otherwiſe you will be continually interrupted in your preparations by the viſits of friends and acquaintance, who will not only rob you of the time you want, but put things out of your mind, ſo that when you come to ſea, you have the mortification to recollect points of buſineſs that ought to have been done, accounts you had intended to ſettle, and conveniencies you had propoſed to bring with you, &c. &c. all which have been omitted through the effect of theſe officious friendly viſits. Would it not be well if this cuſtom could be changed; if the voyager after having, without interruption, made all his preparations, ſhould uſe ſome of the time he has left, in going himſelf to take leave of his friends at their own houſes, and let them come to congratulate him on his happy return.

It is not always in your power to make a choice in your captain, though much of your comfort in the paſſage may depend on his perſonal character, as you muſt for ſo long a time be confined to his company, and under his direction; if he be a ſenſible, ſociable, good natured, obliging man, you will be ſo much the happier. Such there are; but if he happens to be otherwiſe, and is only ſkilful, careful, watchful and active in the conduct of his ſhip, excuſe the reſt, for theſe are the eſſentials.

Whatever right you may have by agreement in the maſs of ſtores laid in by him for the paſſengers, it is good to have ſome particular things in your own poſſeſſion, ſo as to be always at your own command.

1. Good water, that of the ſhip being often bad. You can be ſure of having it good only by bottling it from a clear ſpring or well and in clean bottles. 2. Good tea. 3. Coffee ground. 4. Chocolate. 5. Wine of the ſort you particularly like, and cyder. 6. Raiſins. 7. Almonds 8. Sugar. 9. Capillaire. 10. Lemons. 11. Jamaica ſpirits. 12. Eggs greas'd. 13. Diet bread. 14. Portable ſoup. 15. Ruſks. As to fowls, it is not worth while to have any called yours, unleſs you could have the feeding and managing of them according to your own judgment under your own eye. As they are generally treated at preſent in ſhips, they are for the moſt part ſick, and their fleſh tough

tough and hard as whitleather. All ſeamen have an opinion, broached I ſuppoſed at firſt prudently, for ſaving of water when ſhort, that fowls do not know when they have drank enough, and will kill themſelves if you give them too much, ſo they are ſerved with a little only once in two days. This is poured into troughs that lie ſlopeing, and therefore immediately runs down to the lower end. There the fowls ride upon one another's backs to get at it, and ſome are not happy enough to reach and once dip their bills in it. Thus tantalized, and tormented with thirſt, they cannot digeſt their dry food, they fret, pine, ſicken and die. Some are found dead, and thrown overboard every morning, and thoſe killed for the table are not eatable. Their troughs ſhould be in little diviſions like cups to hold the water ſeparately, figure 25. But this is never done. The ſheep and hogs are therefore your beſt dependance for freſh meat at ſea, the mutton being generally tolerable and the pork excellent.

It is poſſible your captain may have provided ſo well in the general ſtores, as to render ſome of the particulars above recommended of little or no uſe to you. But there are frequently in the ſhip poorer paſſengers, who are taken at a lower price, lodge in the ſteerage, and have no claim to any of the cabbin proviſions, or to any but thoſe kinds that are allowed the ſailors. Theſe people are ſometimes dejected, ſometimes ſick, there may be women and children among them. In a ſituation where there is no going to market, to purchaſe ſuch neceſſaries, a few of theſe your ſuperfluities diſtributed occaſionally may be of great ſervice, reſtore health, ſave life, make the miſerable happy, and thereby afford you infinite pleaſure.

The worſt thing in ordinary merchant ſhips is the cookery. They have no profeſſed cook, and the worſt hand as a ſeaman is appointed to that office, in which he is not only very ignorant but very dirty. The ſailors have

have therefore a ſaying, that *God ſends meat and the devil cooks.* Paſſengers more piouſly diſpoſed, and willing to believe heaven orders all things for the beſt, may ſuppoſe that knowing the ſea-air and conſtant exerciſe by the motion of the veſſel would give us extraordinary appetites, bad cooks were kindly ſent to prevent our eating too much; or, that foreſeeing we ſhould have bad cooks, good appetites were furniſhed to prevent our ſtarving. If you cannot truſt to theſe circumſtances, a ſpirit-lamp, with a blaze-pan, may enable you to cook ſome little things for yourſelf; ſuch as a haſh, a ſoup, &c. And it might be well alſo to have among your ſtores ſome potted meats, which if well put up will keep long good. A ſmall tin-oven to place with the open ſide before the fire, may be another good utenſil, in which your own ſervant may roaſt for you a bit of pork or mutton. You will ſometimes be induced to eat of the ſhip's ſalt beef, as it is often good. You will find cyder the beſt quencher of that thirſt which ſalt meat or fiſh occaſions. The ſhip biſcuit is too hard for ſome ſets of teeth. It may be ſoftened by toaſting. But ruſk is better; for being made of good fermented bread, ſliced and baked a ſecond time, the pieces imbibe the water eaſily, ſoften immediately, digeſt more kindly and are therefore more wholſome than the unfermented biſcuit. By the way, ruſk is the true original biſcuit, ſo prepared to keep for ſea, biſcuit in French ſignifying twice baked. If your dry peas boil hard, a two-pound iron ſhot put with them into the pot, will by the motion of the ſhip grind them as fine as muſtard.

The accidents I have ſeen at ſea with large diſhes of ſoup upon a table, from the motion of the ſhip, have made me wiſh that our potters or pewterers would make ſoup-diſhes in diviſions, like a ſet of ſmall bowls united together, each containing about ſufficient for one perſon, in ſome ſuch form as fig. 26; for then when the ſhip ſhould make a ſudden heel, the ſoup would not in a body flow over

over one ſide, and fall into people's laps and ſcald them, as is ſometimes the caſe, but would be retained in the ſeparate diviſions, as in figure 27.

After theſe trifles, permit the addition of a few general reflections. Navigation when employed in ſupplying neceſſary proviſions to a country in want, and thereby preventing famines, which were more frequent and deſtructive before the invention of that art, is undoubtedly a bleſſing to mankind. When employed merely in tranſporting ſuperfluities, it is a queſtion whether the advantage of the employment it affords is equal to the miſchief of hazarding ſo many lives on the ocean. But when employed in pillaging merchants and tranſporting ſlaves, it is clearly the means of augmenting the maſs of human miſery. It is amazing to think of the ſhips and lives riſqued in fetching tea from China, coffee from Arabia, ſugar and tobacco from America, all which our anceſtors did well without. Sugar employs near one thouſand ſhips, tobacco almoſt as many. For the utility of tobacco there is little to be ſaid; and for that of ſugar, how much more commendable would it be if we could give up the few minutes gratification afforded once or twice a day by the taſte of ſugar in our tea, rather than encourage the cruelties exerciſed in producing it. An eminent French moraliſt ſays, that when he conſiders the wars we excite in Africa to obtain ſlaves, the numbers neceſſarily ſlain in thoſe wars, the many priſoners who periſh at ſea by ſickneſs, bad proviſions, foul air, &c. &c. in the tranſportation, and how many afterwards die from the hardſhips of ſlavery, he cannot look on a piece of ſugar without conceiving it ſtained with ſpots of human blood! Had he added the conſideration of the wars we make to take and retake the ſugar iſlands from one another, and the fleets and armies that periſh in thoſe expeditions, he might have ſeen his ſugar not merely ſpotted, but thoroughly dyed ſcarlet in grain. It is theſe wars that make the maritime powers of Europe, the

the inhabitants of London and Paris, pay dearer for ſugar than thoſe of Vienna, a thouſand miles from the ſea; becauſe their ſugar coſts not only the price they pay for it by the pound, but all they pay in taxes to maintain the fleets and armies that fight for it.

With great eſteem, I am, Sir,

Your moſt obedient humble ſervant,

B. FRANKLIN.

Obſervations of the warmth of the ſea-water, &c. by Fahrenheit's thermometer, in croſſing the Gulph ſtream; with other remarks made on board the Pennſylvania packet, Capt. Oſborne, bound from London to Philadelphia, in April and May 1775.

Date.		Hour.	Temp. of Air.	Temp. of Wat.	Wind.	Courſe.	Diſtance.	Latitude N. °	′	Longitude W. °	′	Remarks.
April	10			62								
	11			61								
	12			64								
	13			65								
	14			65								
	26		60	70				37	39	60	38	Much gulph weed; ſaw a whale.
	27		60	70	S S E	W b S		37	13	62	29	Colour of water changed.
	28	8A.M.	70	64	S W	W N W		37	48	64	35	No gulph weed.
	—	6 P.M.	67	60			34					Sounded, no bottom.
	29	8A.M.	63	71	N	W	44	37	26	66	0	Much light in the water laſt nig.
	—	5 P.M.	65	72	N E							Water again of the uſual deep ſea colour, little or no light in it at night.
	—	11 dit.	66	66	N WbN	W b S	57					
	30	8A.M.	64	70	N E	W b N	69					
	—	12	62	70		E b S	24	37	20	68	53	Frequent gulph weed, water continues of ſea colour, little light.
	—	6 P.M.	64	72	E S E	W b N	43					
	—	10 dit.	65	65	S		25					Much light.
May	1	7A.M.	68	63			60					Much light all laſt night.
	—	12	65	56	S S W	W N W	44	38	13	72	23	Colour of water changed.
	—	4 P.M.	64	56		W b N	21					
	—	10 dit.	64	57	S W	W N W	31					Much light.
	2	8A.M.	62	53			18	38	43	74	3	Much light. Thunder-guſt.
	—	12	60	53	W S W	N W	18					
	—	6 P.M.	64	55	N W	W S W	15					
	—	10 dit.	65	55	N b W	W b N	10					
	3	7 A.M.	62	54			30	38	30	75	0	

Obfervations of the warmth of the fea-water, &c. by Fahrenheit's thermometer; with other remarks made on board the Reprifal, Capt. Wycks, bound from Philadelphia to France, in October and November 1776.

Date.		Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Courfe.	Diftance.	Latit. N.	Long. W.	Remarks.
Octo.	31	10		76	70	S S E	E b S	135	38 12	70 30	Left the capes Thurfday night, October 29, 1776.
	—		4		71						
Nov.	1	10			78	W S W	E ½ N	109	No ob.	68 12	
	—		4	71	81						
	2	8		71	75	N					
	—	12			78			141	ditto.	65 23	
	—		4	67	76						Some fparks in the water thefe two laft nights.
	3	8			76	N W	E SE ½ E				
	—	12			76		E b S	160	37 0	62 7	
	—		4	70	76						
	4	9		68	76		N b E				Ditto.
	—		1		76			194	36 26	58 8	
	—		4	68	76						
	—		8		78						
	5	8		68	76		N E				Ditto.
	—	12		70	75			163	35 21	55 3	
	—		4		75						
	—		8		75						
	6	8			76	E b N	S 50 E				
	—	12			77			75	35 33	53 52	
	7	8			78	S E b E	N 30 W				
	—	12			77			108	36 6	52 46	
	—		4		77						
	8	9		75	77	S b E	N 49 E				
	—	12			77			175	38 2	50 1	
	—		4		77						
	9	9		75	77						
	—	12		75	70	S W	N 33 E	175	39 39	46 55	

Obfervations

Obſervations made on board the Repriſal, continued.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Courſer.	Diſtance.	Latit. N.	Long. W.	Remarks.
Novem. 9		4		71						
10	8		70	68						
—	12			64	E	N 17 E	64	40 39	46 27	
11	8			63						
—	12			61	S E	N 8 E	41	41 19	46 19	
12	8		56	59						
—		4		69	N N W	N 80 E	120	41 39	43 42	
13	all day			68	E	S 82 E	69	41 29	42 10	
14	8		70	70		N 74 E	111	42 0	39 57	
—		Noon		72	E S E					
—		4		71						
15	8		61	69						
—		Noon		68	W S W	N 70 E	186	43 3	35 51	
—		4		67						
16		Noon	65	67	S W	N 67 W	48	43 22	34 50	
—		4		63						
17	8			63	E S E	N 19 E	56	44 15	34 25	
18	all day			65	S b W	N 75 E	210	45 6	29 43	Some gulph weed.
19		Noon	65	64	S W	N 80 E	238	45 46	24 2	
20	8			62	N	S 80 E	155	45 19	20 30	
—		4		60						
21	9			62	S	N 88 E	94	45 22	18 17	
22	10		60	62	S S W	S 89 E	133	45 19	15 19	
23		Noon		61	W S W	S 86 E	194	45 6	10 35	
24		do.		60	N N E	N 78 E	191	45 46	6 10	
25		do.		60	N E	S 76 E	125	45 4	3 23	
26		do.	56	60	E	N 73 E	31	45 13	2 20	
27		do.		58						Soundings off Belliſle.
28		do.	54	56						

1785. A Journal of a voyage from the Channel between France and England towards America.

N. B. Longitude is reckoned from London, and the Thermometer is according to Fahrenheit.

Dates.	Latit. N.	Long. W.	Therm. A.M. Air.	Therm. A.M. Water.	Therm. P.M. Air.	Therm. P.M. Water.	Winds.	Courfe.	Diftance.	Variation of the Needle.	Therm. Noon. A.	Therm. Noon. W.
July 29			62	57			These are ta-	ken on an				
30			62	58	63	58	average of	24 hours.				
31			60	58	62	62			Miles.	Weft.		
Auguft 1	49 15	4 15	63	62	60	64	Eaft	S W ½ W	60	22° 0		
2	48 28	8 58	64	64	64	63	E S E	W b S ½ S	174			
3	47 0	12 13	60	67	omit	ted.	N E	S W b W	160			
4	45 0	15 43	66	66	do.	66	N W b W	S W ½ W	190			
5	43 5	17 25	67	65	65	68	N E	S W b S	131	20 0		
6	41 3	19 44	70	68	71	69	N E	S W ½ S	166	16 30		
7	38 45	21 34	70	70	68	70	N E	S S W ¼ W	165	11 30		
8	36 42	23 10	72	71	73	72	N E	S S W ¾ W	149	11 15		
9	35 40	25 40	73	73	73	74	N E	W S W ¼ S	137			
10	35 0	27 0	71	73	77	75	N W	W S W ¾ S	76			
11	33 51	28 42	74	74	76	77	North	S W ¾ W	112			
12	33 30	31 30	76	75	76	76	North	W ¾ S	143			
13	33 17	33 32	76	76	78	77	N E	W ½ S	103		77	78
14	33 22	34 31	76	76	81	79	S S E	W ½ N	50		81	79
15	33 45	35 0	78	79	79	78	W N W	S W ¼ W	35		79	79
16	34 14	35 30	79	78	81	80	Weft	N W ½ N	38		81	80
17	35 37	36 4	80	79	80	78	W S W	N N W	75		80	78
18	36 7	37 16	80	78	omit	ted.	N W b W	WNW ½ N	65		80	79
19	36 38	38 0	78	77	78	77	W S W	N W ½ W	49		79	77
20	37 38	38 6	78	76	omit	ted.	Weft	N ¼ W	62		77	75
21	36 15	38 26	73	74	78	76	W N W	S b W	82		77	75
22	35 40	38 44	77	76	80	77	W b S	S S W	38		80	77
23	35 35	40 52	79	77	78	75	North	W ¼ S	100		omit	ted.
24	35 12	41 31	75	73	75	74	W N W	S W b W	41		75	74
25	35 40	42 33	79	76	79	76	W b N	WNW ¾ N	60		80	76
26	35 30	42 44	79	76	80	76	S W b W	S W ½ S	14		80	76
27	35 14	43 23	79	77	81	79	Weft	W S W ¼ S	38		81	78
28	34 23	44 0	78	76	78	78	N N E	S W b S	60		78	78
29	34 12	45 52	77	78	78	78	N E	W ¼ S	94	8 0	79	78
30	34 5	48 31	78	78	78	78	Eaft	W ½ S	134		78	78
31	34 20	51 4	80	79	81	79	Eaft	W ¾ S	129		80	80
Septem. 1	34 20	52 47	81	78	omit	ted.	S S W	W ¼ N	86		83	80
2	34 55	55 12	81	80	83	80	S W	W b N ½ W	125		83	80
3	35 30	57 24	83	80	83	80	S W b S	W b N ½ N	114	6 0	84	81
4	35 50	59 1	82	80	83	80	S W ½ W	W b N ¼ N	82		83	81
5	35 55	61 0	81	80	82	81	S S W	W ¼ N	96		82	81
6	36 20	62 30	80	81	79	80	N W b N	W b N	75		78	80
7	34 50	63 10	87	80	78	81	N W b W	S. S W	86		78	81
8	34 45	64 40	75	79	75	79	North	W ¼ S	74		75	79
9	35 43	66 42	75	79	77	73	N E	W N W	108		78	80
10	37 20	68 40	77	73	77	70	E N E	N W	126		78	72

OBSERVATIONS.

July 31. At one P. M. the Start bore W N W. diſtant ſix leagues.
Auguſt 1. The water appears luminous in the ſhip's wake.
—2. The temperature of the water is taken at eight in the morning and at eight in the evening.
—6. The water appears leſs luminous.
—7. Formegas S W. diſt. 32½ deg. St. Mary's S W ½ S 33 leagues.
—8. From this date the temperature of the water is taken at eight in the morning and at ſix in the evening.
—10. Moonlight, which prevents the luminous appearance of the water.
—11. A ſtrong ſoutherly current.
—12. Ditto. From this date the temperature of the air and water was taken at noon, as well as morning and evening.
—16. Northerly current.
—19. Firſt ſaw gulph weed.
—21. Southerly current.
—22. Again ſaw gulph weed.
—24. The water appeared luminous in a ſmall degree before the moon roſe.
—29. No moon, yet very little light in the water.
—30. Much gulph weed to-day.
—31. Ditto.
Sept. 1. Ditto.
—2. A little more light in the water.
—4. No gulph weed to-day. More light in the water.
—5. Some gulph weed again.
—6. Little light in the water. A very hard thunder-guſt in the night.
—7. Little gulph weed.
—8. More light in the water. Little gulph weed.
—9. Little gulph weed. Little light in the water laſt evening.
—10. Saw ſome heds of rock-weed; and we were ſurpriſed to obſerve the water ſix degrees colder by the thermometer than the preceding noon.

This day (10th) the thermometer ſtill kept deſcending, and at five in the morning of the 11th, it was in water as low as 70, when we ſtruck foundings. The ſame evening the pilot came on board, and we found our ſhip about five degrees of longitude a-head of the reckoning, which our captain accounted for by ſuppoſing our courſe to have been near the edge of the gulph ſtream, and thus an eddy-current always in our favour. By the diſtance we ran from Sept. 9, in the evening, till we ſtruck ſoundings, we muſt have then been at the weſtern edge of the gulph ſtream, and the change in the temperature of the water was probably owing to our ſuddenly paſſing from that current, into the waters of our own climate.

On the 14th of Auguſt the following experiment was made. The weather being perfectly calm, an empty bottle, corked very tight, was ſent down 20 fathoms, and it was drawn up ſtill empty. It was then ſent down again 35 fathoms, when the weight of the water having forced in the cork, it was drawn up full; the water it contained was immediately tried by the thermometer, and found to be 70, which was ſix degrees colder than at the ſurface: The lead and bottle were viſible, but not very diſtinctly ſo, at the depth of 12 fathoms but when only 7 fathoms deep, they were perfectly ſeen from the ſhip. This experiment was thus repeated Sept. 11, when we were in ſoundings of 18 fathoms. A keg was previouſly prepared with a valve at each end, one opening inward the other outward; this was ſent to the bottom in expectation that by the valves being both open when going down, and both ſhut when coming up, it would keep within it the water received at bottom. The upper valve performed its office well, but the under one did not ſhut quite cloſe, ſo that much of the water was loſt in hauling it up the ſhip's ſide. As the water in the keg's paſſage upwards could not enter at the top, it was concluded that what water remained in it was of that near the ground, and on trying this by the thermometer, it was found to be at 58, which was 12 degrees colder than at the ſurface.

This laſt Journal was obligingly kept for me by Mr. J. Williams, my fellow-paſſenger in the London Packet, who made all the experiments with great exactneſs.

Two

N° XXXIX.

Two Hearts found in one Partridge.

ALL the works of nature are linked the one to the other and form a whole, in the immenſity of which we only perceive ſome points which appear to us detached, becauſe thoſe which unite them are concealed from us.

The reſult of this connection is, that no work of nature ought to be neglected, that there is not one which may not derive ſome direct or indirect utility to man.

That which appears futile, ſhould be graſped like the others, and in poſſeſſing ourſelves of it we ſhould be aſſured that we have hold of a chain, the precious links of which will be diſcovered by time[a]. If thoſe links which are wanting leave vacancies, the intermediate links are every day preſented to us by the hand of chance; and it is the buſineſs of the naturaliſt to arrange them. Let us then offer

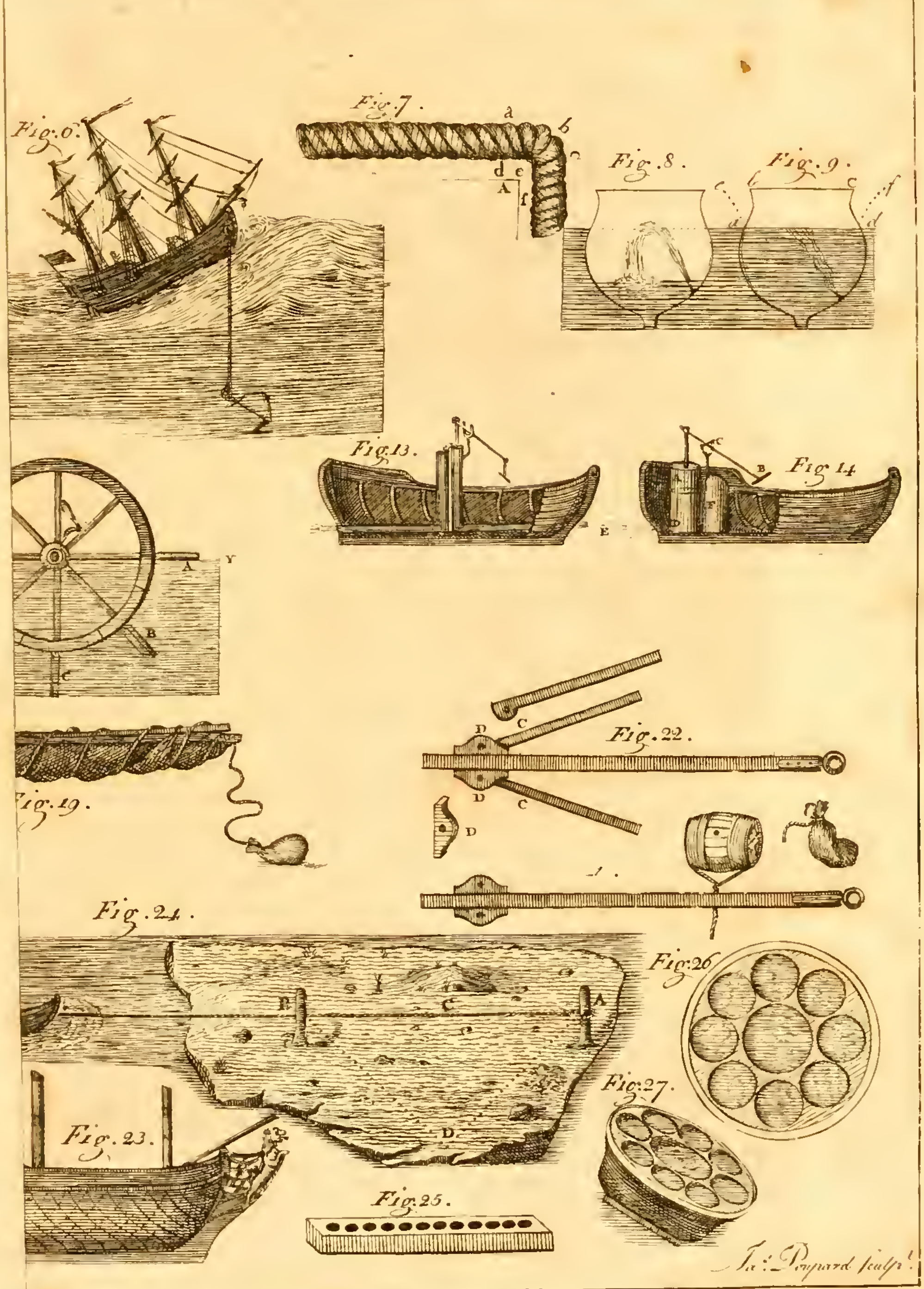
DEUX COEURS TROUVÉS DANS UNE PERDRIX.

TOUS les faits de la nature ſont liés les uns aux autres, et forment un tout, dans l'immenſité duquel nous n'appercevons que quelques points, qui nous paroiſſent iſolés parce-que ceux qui les uniſſent nous ſont cachés. Il reſulte de cet enchainement qu'aucun fait de la nature ne doit être négligé, qu'il en eſt pas un qui ne puiſſe devenir de quelque utilité pour l'homme, ſoit directe ou indirecte. Celui qui paroit le plus ſutile doit être recueilli comme les autres; en le ſaiſiſſant on doit être aſſuré qu'on tient une chaine ou le temps découvrira des chainons precieux*. Si ceux qui manquent y laiſſent des lacunes, ces chainons intermediaires nous ſont tous les jours preſentés par la main du hazard, et celle du naturaliſte les met à leurs places. Offrons

(a) We here ſee women ſit quiet in their houſes whilſt thunder is rumbling over their heads; would they, at this day, enjoy this happy ſecurity, if a man had not obſerved, ſome thouſand years ago, that a piece of amber when rubbed attracted light bodies which are near it. It is he who put into the hands of modern philoſophers the chain in which Franklin was to find the link, from which his imagination took the hint of his conductor.

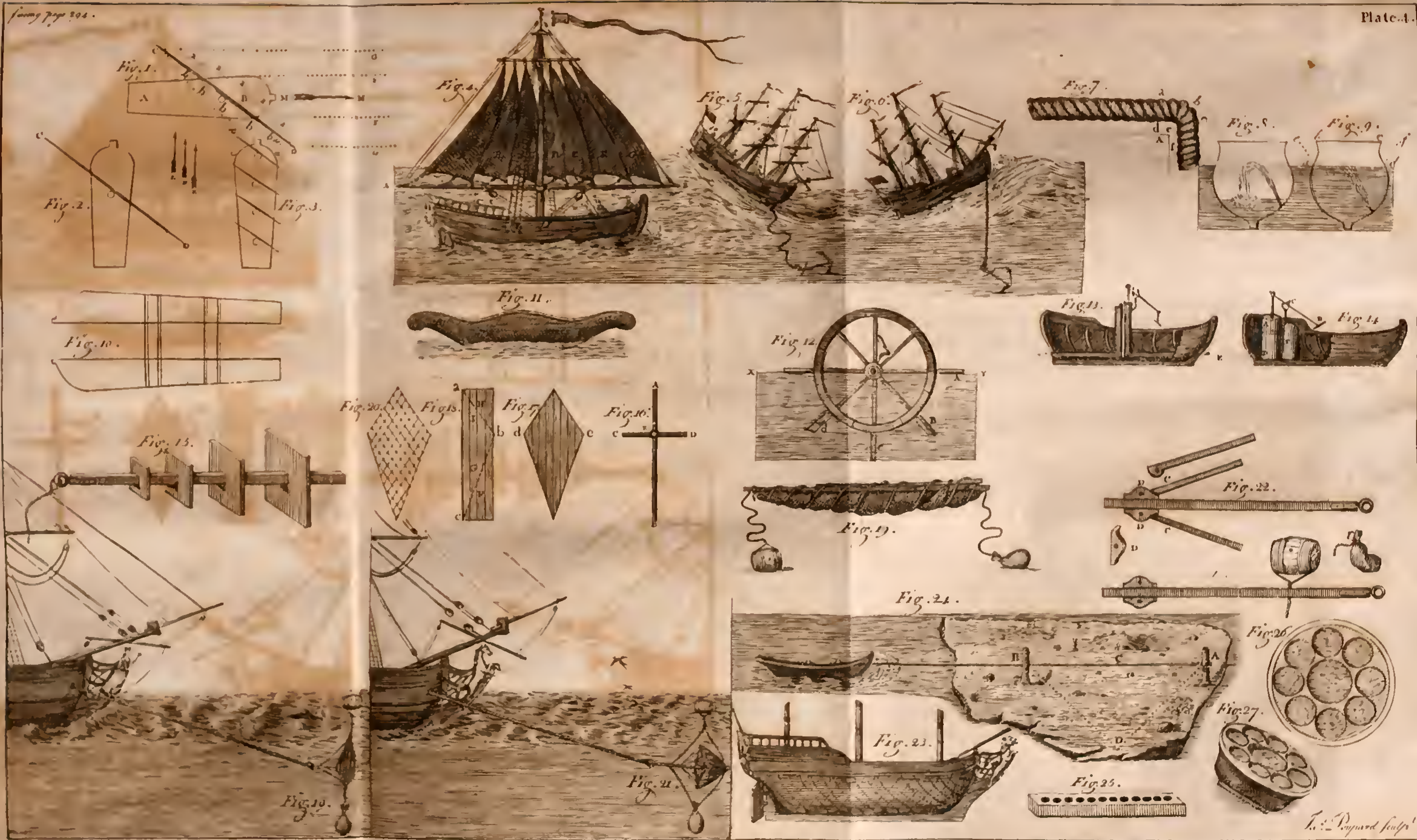
* *Nous voyons ici les femmes tranquilles dans leurs maiſons lorſque la tonnérre gronde ſur leur tête. Jouiroient elles aujourdhui de cette heureuſe ſécurité, ſi un homme n'avoit obſervé il y a quelques milliers d'années, qu'un morceau d'ambre frotté attire les corps legers qui l'avoiſinent? C'eſt lui qui a mis dans les mains des phyſiciens modernes la chaine ou Franklin devoit trouver le chainon dont il eſt parti pour imaginer ſon conducteur.*

Plate.4.



Ja.s Poynard sculp.t

facing page 294.



J. & Poynard sculp.

Plate 5.

facing page 315.

REMARKS

Upon the Navigation from

NEWFOUNDLAND to NEW-YORK,

In order to avoid the

GULPH STREAM

On one hand, and on the other the SHOALS *that lie to the Southward of Nantucket and of St. George's Banks.*

AFTER you have paſſed the Banks of Newfoundland in about the 44th degree of latitude, you will meet with nothing, till you draw near the Iſle of Sables, which we commonly paſs in latitude 43. Southward of this iſle, the current is found to extend itſelf as far North as 41° 20′ or 30′, then it turns towards the E. S. E. or S. E. ¼ E.

Having paſſed the Iſle of Sables, ſhape your courſe for the St. George's Banks, ſo as to paſs them in about latitude 40°, becauſe the current ſouthward of thoſe banks reaches as far North as 39°. The ſhoals of thoſe banks lie in 41° 35′.

After having paſſed St. George's Banks, you muſt, to clear Nantucket, form your courſe ſo as to paſs between the latitudes 38° 30′ and 40° 45′.

The moſt ſouthern part of the ſhoals of Nantucket lie in about 40° 45′. The northern part of the current directly to the ſouth of Nantucket is felt in about latitude 38° 30′.

By obſerving theſe directions and keeping between the ſtream and the ſhoals, the paſſage from the Banks of Newfoundland to New-York, Delaware, or Virginia, may be conſiderably ſhortened; for ſo you will have the advantage of the eddy current, which moves contrary to the Gulph Stream. Whereas if to avoid the ſhoals you keep too far to the ſouthward, and get into that ſtream, you will be retarded by it at the rate of 60 or 70 miles a day.

The Nantucket whale-men being extremely well acquainted with the Gulph Stream, its courſe, ſtrength and extent, by their conſtant practice of whaling on the edges of it, from their iſland quite down to the Bahamas, this draft of that ſtream was obtained from one of them, Capt. Folger, and cauſed to be engraved on the old chart in London, for the benefit of navigators, by

B. FRANKLIN.

Note, The Nantucket captains who are acquainted with this ſtream, make their voyages from England to Boſton in as ſhort a time generally as others take in going from Boſton to England, viz. from 20 to 30 days.

A ſtranger may know when he is in the Gulph Stream, by the warmth of the water, which is much greater than that of the water on each ſide of it. If then he is bound to the weſtward, he ſhould croſs the ſtream to get out of it as ſoon as poſſible.

B. F.

fer him all thoſe which we meet with. It increaſes the maſs of human knowledge, and enriches a ſtore-houſe which is very precious to man; a ware-houſe belonging to all nations and to all ages. Not to lodge every new diſcovery in this common ſtore, is to ſquander away riches which we held only in truſt, and in which the moſt diſtant generations have an intereſt. This is criminally depriving humanity of a bleſſing which is intended for the good of ſociety.

The foregoing reflections induce me to publiſh the following fact. Monſieur Vergé, ſenior ſurgeon of the artillery, lodged next door to me, and came into my room with the entrails of a partridge which he had juſt opened, and ſhewed me two hearts attached to one lung by blood veſſels. I requeſted him to go immediately to the chev. de Chaſtellux, knowing that the phenomenon would be intereſting to him. But wiſhing firſt to ſtep home, and not being able with one hand to open the padlock which faſtened his door, he put down the ſaucer which contained the entrails; his dog who had followed, ſuppoſing it was a meſs for himſelf, ſoon emptied the ſaucer.

The chevalier de Chaſtellux called on me an hour afterwards to ſee a male opoſſum, of which I ſpoke to him on account

Offrons lui donc tous ceux que nous rencontrons, c'eſt groſſir la maſſe des connoiſſances humaines, et enrichir le magazin le plus précieux à l'homme, magazin qui appartient à toutes les nations et à tous les ſiecles. Ne pas rapporter à ce depòt commun les faits nouveaux que l'on découvre, c'eſt perdre des richeſſes dont on étoit dépoſitaire, et ſur leſquelles les générations les plus reculées avoient des droits. C'eſt ſe rendre coupable envers l'humanité, en la fruſtrant d'un bien dont on lui étoit comptable.

Ce ſont ces réflexions qui m'engagent à rendre publique le fait ſuivant.

Le Sieur Vergé, chirurgien major de l'artillerie, logé à coté de chèz moi, entra il y a quelques jours dans ma chambre, tenant dans ſes mains les entrailles d'une perdrix qu'il venoit d'ouvrir, et m'y fit remarquer deux coeurs qui tenoient à un même poumon par des vaiſſeaux ſanguins. Je le priai d'aller ſur le champ les faire voir à Mr. le Chevr. de Chaſtellux, que je ſavois que ce phénomene intereſſeroit. Mais ayant voulu auparavant rentrer chez lui, et ne pouvant d'une main ouvrir le cadenat qui ferme ſa porte, il poſa a terre la ſoucoupe ſur la quelle etoient ces entrailles. Son chien, qui l'avoit ſuivi, crut que c'etoit pour lui, et eut bientôt vidé la ſoucoupe. Mr. le Chevr. de Chaſtellux vint une heure aprés chez moi, voir un opoſſum mâle, dont je lui avois parlé à cauſe de la ſingularité des organes de la génération de cet account of the ſingularity of his organs of generation[b]. I mentioned to him the two hearts, he could not comprehend me. I ſent to aſk of the ſenior ſurgeon why he did not do what I requeſted of him; he came and related the accident of the ſaucer. The chevalier de Chaſtellux judged there was no other way of repairing the loſs but by a certificate, ſtating what we had ſeen; but every thing conſpired againſt the deſire the chevalier de Chaſtellux and I had to perpetuate the phenomenon; the dog had deprived us of the monument; the maſter refuſed us his certificate, ſaying, one cannot be too circumſpect in affirming a fact which we ought not to judge of from external appearances; but to be convinced that that which appeared to us as two hearts were really ſo, they ought to have been diſſected, &c. One cannot too much applaud the ſcruples of Mr. Vergé; but regarding as much my veracity as Mr. Vergé his, I am not afraid to hazard an exact relation of what I have ſeen, without alteration or exaggeration. I ſaw, then, two fleſhy ſubſtances of a brown violet colour, of an oblong form, thicker at one end than the other, nearly round taken tranſverſely. Theſe two ſubſtances reſembled each

cet animal†. Je lui parlai des deux coeurs; il ne ſcut ce que je lui voulois dire: J'énvoyai demander au chirurgèon major pourquoi il n'avoit pas exécuté ce que je lui avoit dit; il vint nous raconter l'accident de la ſoucoupe. Mr. le Chevr. de Chaſtellux jugea qu'l n'y avoit pas d'autre moyen de réparer cette perte, que par un proces verbal, qui conſtata ce que nous avions vu. Mais tout conſpiroit contre le deſir que Mr. le Chevr. de Chaſtellux et moi avions de perpétuer le ſouvenir de ce phenomene: Le chien nous a privé du monument; le maitre nous refuſe ſon atteſtation, diſant, *qu'on ne peut être trop circonſpect à affirmer un fait; qu'il ne faut pas juger ſur des apparences exterieurs; que pour etre aſſuré que ce qui nous a paru des coeurs fuſſent reellement des coeurs, qu'il eut fallu les avoir ouverts, &c.* On ne peut qu'aplaudir aux ſentiments qui rendent M. Vergé ſi ſcrupuleux. Mais en me pîquant d'autant de véracité que M. Vergé, je ne crains point de la compromettre en rendant exactement ce que j'ai vu, ſans altération et ſans exagération. Or j'ai vû deux corps charnus d'un violet brun, de ſorme un peu oblongue, plus gros par un bout que par l'autre, un peu plus convexe d'un coté que de l'autre, a peu près ronds conſidérés tranſverſalement. Ces deux corps etoient abſolument ſemblables l'un a l'autre

(b) It has nothing external except the two teſticles placed under the belly; the male and the female have but one orifice placed under the tail, which ſerves them to void their excrements, their urine, and probably for generation. It is not known whether a penis comes out of this orifice, or whether there is nothing but an opening of the two orifices in the act of copulation.

† *Il n'a d'exterieur que deux teſticules placées ſous le ventre, le male et la femelle n'ont qu'un orifice placé ſous la quéue qui leur ſert a vider les excréments, les urines, et probablement à la génération. On ignore ſi par ce cloaque le mâle ſort une verge, ou s'il ne ſe fait qu'un abouchement des deux orifices lors de la copulation.*

each other exactly in ſize, form and colour. From their thicker ends came out ſeveral veſſels, among them I obſerved one to each of the bodies ,pretty large and paler than the reſt; a part of this veſſel was faſtened to the lungs in ſuch a manner, that by taking hold of only the lungs, theſe two bodies in queſtion were both ſuſpended at the ſame diſtance from the lungs. In placing theſe viſcera on the hand in ſuch a manner that the reſembling ſides of theſe two bodies faced each other, the veſſels which were ſuſpended appeared uniformly diſpoſed, which makes me imagine that the two bodies were uniformly diſpoſed in the body of the animal[c]. However, the above obſervations being freſh in my memory, I ordered the entrails of four dozen of partridges of the ſame ſpecies[d], juſt taken out, to be brought me; and I found in each a body like the double body which had been found in the bird of Mr. Vergé, having ſimilar veſſels, ſome of which were paler than others, adhering in the ſame manner to the lungs, and this body was certainly a heart or my partridges had none. I nevertheleſs leave every one at liberty to judge

U u which

l'autre, en volume, en forme, et en couleur; de leur gros bout ſortoient pluſieurs vaiſſeaux, parmi leſquels j'en remarquai un à chacun de ces corps, aſsèz gros et de couleur plus pale que les autres; une partie de ces vaiſſeaux tenoient au poumon, de maniére que ſaiſiſſant ſeulement le poumon, les deux corps dont il s'agit y reſtoient ſuſpendus tous deux a la même diſtance du poumon. En arrangeant ces viſceres ſur la main, de maniére que les cotes ſemblables de ces deux corps ſe regardaſſent, les vaiſſeaux qui y tenoient ſe trouvoient ſimetriquement diſpoſés; ce qui me fait juger que ces deux corps devoient ſe trouver dans l'animal ſimetriquement placés*. Enfin ayant encore la memoire fraiche des obſervations ſuſdites, je me fis apporter les entrailles de quatre douzaines de perdrix de la même eſpece† que l'on venoit de vider, et j'ai vû qu'il ſe trouvoit dans chacune un corps en tout ſemblable à celui qui s'etoit trouvé douhle dans la perdrix de M. Vergé, ayant de pareils vaiſſeaux, dont un plus pâle que les autres, tenant de la même maniére au poumon, et ce corps etoit certainement un cœur, ou mes perdrix n'en avoient pas. Je laiſſe maintenant chacun libre de juger lequel paroit le plus vraiſemblable, qu'une perdrix eut deux cœurs, ou qu'il ne s'en trouva pas un ſeul dans

quarante

* *Je preſume qu'ils occupoient les deux cotés du thorax, et que les veines de l'un s'anaſtomoſoient aux art res de l'autre et reciproquement.*

(c) I preſume that they occupied the two ſides of the thorax, and that the veins of the one anaſtamoſed with the arteries of the other, and vice verſa.

† *Elle eſt un peu plus petite que celle qu'on appelle en France perdrix de paſſage ou raquette, elle ſe perche de meme.*

(d) It appears rather ſmaller than that which, in France, is called perdrix de paſſage, or raquette; they ſit on a tree in the ſame manner.

which was the moſt probable, that one partridge had two hearts, or that forty-eight had none at all. It may indeed be objected, that the firſt might have a true and a falſe one. Mr. Voltaire, who believes that nature amuſes herſelf in making concha veneris, might alſo believe that ſhe diverted herſelf in imitating two hearts. I ſhould therefore be as circumſpect as Mr. Vergé, and not affirm ſooner than him that his partridge had two hearts, for I may be miſtaken. I am, however, infinitely more certain of it than that it had but one.

D'ABOVILLE.

Williamſburgh, Feb. 15, 1782.

I the ſubſcriber, ſenior ſurgeon of the regiment of Auxonne, and of the artillerymen of the king's army in America, do declare, that the 10th of this month, having taken out the entrails of the body of a partridge, there appeared to me two hearts. However, during the ſhort time that I had to inſpect them, I was ſo aſtoniſhed with ſo ſurprizing a phenomenon, that I fear my eyes may have deceived me, and I dare not to affirm what I believe I ſaw. I went immediately to a merchant, who lodged next door to me, to ſhow him this miracle, and alſo to Mr. Aboville. The latter has juſt read to me the account he has written of this phenomenon as it appeared to him. I hereby certify that I ſaw

quarente huit perdrix. Il reſtera à dire que la premiére pouvoit en avoir un vraï et un faux. M. de Voltaire, qui croyoit que la nature s'amuſe a imiter des conqua véneris, auroit pu croire qu'elle s'amuſe auſſi a contrefaire des cœurs; je ſerai donc auſſi circonſpect que M. Vergé, et n'affirmerai pas plus que lui que ſa perdrix avoit deux cœurs, car je pourrois me tromper; j'en ſuis cependant infiniment plus certain que je ne le ſuis de n'en avoir qu'un.

A Williamſburg en Virginie, le 15 Fev. 1782.

(Signé) d'Aboville.

JE ſouſſigné, chirurgien major du régiment d'Auxonne, et de l'equipage d'artillerie de l'armée du roi en Amerique; déclare, que le dix de ce mois ayant rétiré les entrailles du corps d'une perdrix, il me parut qu'il s'y trouvoit deux coeurs. Mais pendant le peu de temps que j'ai pu les conſidérer, j'étois tellement ebloui d'un phénomene auſſi ſurprenant, que je crains que mes yeux ne m'ayent trompé, et n'oſe affirmer ce que je crois avoir vû. Je fus ſur le champ montrer cette merveille a un marchand qui loge a coté de chez moi, et à M. d'Aboville; ce dernier vient de me lire l'expoſé qu'il a écrit de ce phénomene, tel qu'il s'eſt preſenté

I ſaw nothing contrary to the obſervations contained in this account.

Williamſburgh, Feb. 15, 1782. VERGÉ.

I the ſubſcriber, merchant at Williamſburgh, reſiding next door to Mr. Vergé, ſenior ſurgeon of artillery, do certify that the 10th of this month, this ſurgeon came to ſhew me the entrails of a partridge, and pointed out to me two hearts very diſtinct, adhering by their blood-veſſels to the ſame lung. I moreover certify that Mr. Aboville has read me the account he has given thereof, and that I find nothing in it contrary to what I ſaw.

THOMAS BENTLEY.

Williamſburgh, Feb. 18, 1782.

ſenté a ſes yeux. Je certifie n'avoir rien obſervé qui ne ſoit conforme aux obſervations contenues dans cet expoſé.

A Williamſburg, le 18 Fev. 1782.
(Signé) Vergé.

Je ſouſſigné marchand demeurant à Williamſburg à coté de chez M. Vergé, chirurgien major de l'artillerie, certifie, que le dix de ce mois ce chirurgien vint me montrer les entrailles d'une perdrix, et qu'il m'y fit remarquer deux cœurs, bien diſtincts, tenans tous les deux par leurs vaiſſeaux ſanguins, à un même poumon. Je certifie en outre que M. d'Aboville vient de me lire l'expoſé qu'il en a fait, et que je n'y ai rien trouvé que de conforme à ce que j'avois vû.

A Williamſburg, le 18 Fev. 1782.
(Signé) Thomas Bentley.

N° XL.

Conjectures concerning Wind and Water-Spouts, Tornados and Hurricanes. Communicated by Dr. JOHN PERKINS, *of Boſton, to* JOHN MORGAN, *M. D. of Philadelphia, Profeſſor of the Theory and Practice of Phyſic; and F. R. S. London, &c.*

WITH reſpect to water-ſpouts what I am about to conſider is whether water aſcends or deſcends in

 theſe

thefe bodies? A queftion which it is reafonable to think fhould be determined by facts, and the nature of things; and concerning which, if we wifh to attain to any certainty, we muft be careful not to be mifled by fuch appearances and imaginations, as have hitherto commanded the general belief.

Agreeable to this method of inquiry, I fhall in the firft place produce the obfervations of three or four perfons, in whom I can confide for fimplicity and honefty of intention.

The firft is that of captain Melling, formerly of Bofton, who informed me that in a voyage from our Weft-India iflands, in the month of Auguft, in a warm day juft at evening, a fpout fell clofe by the veffel, and in two or three feconds of time came acrofs the ftern where he then was. A flood of water, as he expreffed it, poured upon him and almoft beat him down, fo that he was obliged to lay hold of what was neareft to him, to prevent being wafhed overboard, which in his fright he was apprehenfive of. But the fpout immediately paffed off with a roaring noife into the fea. I afked him if he tafted the water? Tafte it faid he! I could not help tafting it, it ran into my mouth, nofe, eyes and ears. Was it then frefh or falt? as frefh, faid he, as ever I tafted fpring water in my life.

The next account I had was from captain John Wakefield, alfo of Bofton, which was, that being juft within the ftraits of Gibraltar, a fpout fell clofe by his fhip with a great roaring which he heard as he was fetting in the cabbin, the men upon deck immediately crying out for him to come up, which he inftantly did, and faw it travelling away before the fhip, fo near that he plainly faw the water defcend. His men affured him that it did fo from the beginning. He told me the wind was very fmall during the operation of it.

Captain John Howland, of the fame town, told me that in paffing the calm latitudes, a fpout fell fo near that he he evidently ſaw the water deſcend, very contrary to his former opinion concerning theſe bodies.

Mr. Samuel Spring, of the ſame town, told me that in a voyage from India, in paſſing the ſtraits of Malacca, a ſpout fell by eſtimation about fifty yards from their ſhip; the appearance of which was that of a column of water; or rather a ſtream of almoſt contiguous drops, from the cloud down into the ſea, making a great froth in the place like water falling among rocks, as he expreſſed it. He ſaid it was extremely plain that the water deſcended. One of the ſhip's crew was with him when he gave me this account, and confirmed it.

Many other accounts I have had from thoſe who have ſeen ſpouts, but ſo indeterminate as not to be worth much notice; I therefore content myſelf with the above, which ſpeak for themſelves.

In the next place I ſhall make a few remarks on Mr. Stuart's figures of ſpouts, which he took in the Mediterranean, as they are to be ſeen in the philoſophical tranſactions of London, Le Motte's abridgement; particularly on the pointing to the place of ſpattering in the water, and the great roar that attends the operation of a large ſpout; the buſh about the foot or baſe of a great ſpout; the break or partition in the trunk of it at the top of the buſh; and the pillar-like appearance within the buſh.

Firſt I ſhall endeavour to give ſome idea of the nature and cauſe of the pointing by the external and apparent means that nature uſes in the production of a ſpout; for as to the intimate operations of nature our faculties cannot reach them. Two or three obſervations I ſuppoſe will readily be granted, and ſhorten my work.

One is that thoſe places where the lower region of air is drawn away on one or both ſides, either by the heat of neighbouring continents, or in the calm latitudes, from which it paſſes away into, and for the ſupply of the equatorial

torial expence, are likely to be the places moſt liable to ſpouts.

In the next place I expect it will be granted that the air is much colder in the upper regions, and of conſequence ſpecifically heavier than that near the ſurface, by which when there are little or no differing motions of the air, (i. e. winds) in or about the region of the clouds, particular ſpots of air and vapour in the cloud, may be diſpoſed to deſcend, and, when ſo, will take very aptly a particular channel downwards. Theſe things being granted what is of a like kind will readily be ſo diſpoſed too, as when the atmoſphere is full of vapours condenſing into clouds, this condenſation may be quicker in one place than in another, which by the acquired cold will become more weighty and preſs moſt in a particular point. Thus it may deſcend through the more rarified and yielding ſubjacent region, the firſt drops piercing and making a channel may facilitate the deſcent of the vapour, till it puts on what Stuart calls a ſword-like appearance. The agitation cauſed by deſcending will accelerate condenſation, which together with the drops paſſing through the vapour in this channel, may at every ſtop in the paſſage be waſting the vapour, by taking it up into leſſer maſſes of water till it ends in a point, which it will in this caſe naturally do, becauſe the ſwifteſt motion down is in the center of the pointing body.

Such a ſpout may increaſe ſo as to form maſſes of water, the ſubſtance of the cloud, all obſtacles removed, paſſing down in greater abundance, and ſtill more ſwiftly condenſing; or it may preſently ceaſe when it has but juſt appeared, or inſtead of this, make, as it were, ſeveral attempts for completing a ſpout, the vapour teat advancing and retiring alternately, but which finally fail, without producing effect. Thus it has done, as it ſeems, when the cloud has not had ſufficient ſupplies for it to ſucceed in a complete and opaque ſpout. Such are the appearances of Mr. Stuart's figures, &c. The obliquity of the pointing

pointing is owing to the courfe of the air, as the bend is to two different ones at different heights.

The next thing propofed to be confidered, was the great roar that attends a complete fpout while it lafts; and it is the fame as that in cataracts or falls of water from great eminencies. This kind of roar could not exift in any way of afcent, being very different from that of a whirlwind, which is no other than that of any other ftrong wind.

Mr. Stuart's figures of the great fpouts are drawn with the appearance of a bufh round their bafes: The cafe is fuch, that great falls of waters muft make a proportionable fpray; fo that the appearance is natural, and indeed a neceffary confequence. It rifes up from the foot of the fpout and falls back in a parabolic manner into the fea. As was faid of the roar juft now, fo it may be faid of this, that it could not have exifted in any conceivable way of afcent; while on the contrary it is perfectly agreeable to nature on the principle of defcent. It continues the whole time of a large fpout, increafing and diminifhing as that does.

The appearance of a break or partition in the trunk of the fpout, at the top of the bufh, is a very curious phenomenon: It is not real but apparent, and could not have happened without the bufh; it being caufed by a refraction of rays from the drops that conftitute the top of the bufh; whence a divergency and fo much lofs of vifion.

In great fpouts there is alfo a pillar-like appearance, being a part of the trunk within the bufh, and by another refraction through the fide of the bufh; by which it appears much bigger than it is, and limited in altitude by the break. The three laft are agreeable to the laws of optics; and all the five particulars being attendants on the greater or the fmaller fpouts, are to me undeniable evidences of the univerfal defcent of waters in thefe bodies. I pafs from Mr. Stuart's figures to that of Mr. Maine, which is not lefs curious.

Mr.

Mr. Maine, in the ſame philoſophical tranſactions, has given us the figure of a ſpout that fell at Topſham, near Exeter. He has depicted it in the act of ſtriking a boat as it paſſed a creek; from the bottom of which he has drawn a rebound of the whole body of the ſpout projected from it to a large diſtance; evidently proving the deſcent: And which, while he is arguing for the aſcent, it would have much become him to have accounted for, and to have ſhown how it agreed with the doctrine of aſcent. The ſpout proceeding paſſed on to the land, and brake off the limbs of a tree, beat the thatch off of a houſe, and did perhaps various other damage; but we hear nothing of its carrying up any of the light ſubſtances and dropping them at great diſtances, far from any environs of the place, which it would moſt certainly have done had there been a whirlwind, or any ſupernal ſuction employed in the operation.

The ſupernal ſuction which ſome have mentioned I ſuppoſe I may paſs over without more than the bare mention of it, but whirlwinds we know there are frequently, and ſome of conſiderable ſtrength; ſo that it being the general opinion that ſpouts are formed by them, it may not be amiſs to examine a little what force they may reaſonably be allowed to have, and the limits of it.

Their genuine cauſe, ſuppoſing them to be natural productions, is no other than the aſcent of the heated and conſequently lighter air, at the ſurface, into, or through the colder and conſequently heavier regions of the atmoſphere above: And in proportion to the different degrees of heat in one of theſe, and cold in the other, may the ſtrength of theſe be, but no more.

Dr. Arbuthnot, in his treatiſe on the air, tells us, that the rarification of the air in the hotteſt day in ſummer renders it but one-tenth lighter than that of the coldeſt in winter, or in words to this purpoſe, if I remember right, for I have not his book by me. Suppoſing then the upper region the ſame at all times as the lower one in winter

ter when a whirlwind happens, it cannot have any greater force than the weight of one-tenth of the atmosphere, and considering the resistance to its rising which it must encounter, and the friction by the way, not so much; by which the strength may not be equal to three feet of water. It is undoubtedly nine parts in ten too weak to make a vacuum, and having a column of water two miles high to support, besides the additional necessity of still more force to drive it swiftly up, would require an atmosphere two thousand times more weighty than ours to raise water to the clouds.

Mr. Stuart says he saw the water ascend in the heart of a spout; which seems to have been an unlucky expression. The bodies of large spouts are too gross and opaque for any one to see to the center of them; and no one has ever pretended to have seen water ascend in the small ones. His imagination therefore must have been too strong for any one to confide in, so far as was he prejudiced; and at least one of his views was to prove the ascent; which, had he understood nature in a tolerable degree, he would have renounced.

That there is a gyrating appearance in the great spouts, seems to have been matter of observation; nor is there any improbability in the thing. As air passing up in whirlwinds, so water, or air, passing down may gyrate; and no doubt it does. The case is, that some have imagined the gyration to have been upwards: but the appearance of gyration up or down may easily deceive, as any one may be convinced by observing the swift turning of artificial screws, in which the direction will appear as the person is disposed to fancy it.

We are told the Chinese sailors answer to the question, what are you afraid of in spouts, is, that they may break in their decks. Which shows they take them to be descents; and their knowledge is from observation and experience.

I conclude with one short remark, viz. That to believe water ascends in these bodies, to the region of the

 clouds,

clouds, is virtually to admit of real and effential miracle, without fufficient proof; and contrary to every idea we can form, of a divinely wife intention.

Tornados and hurricanes I take to be of the fame general nature, although differing in fome circumftances and appearances.

By the term tornado, or wind-fpout, I mean a violent wind which has been obferved in thefe northern colonies a few times fince they were difcovered and fettled by our people. But perhaps no part of the terraqeous globe is entirely free from fomething of the like kind, as the atmofphere is every where liable to fimilar commotions.

The Spanifh term of tornado, feems to have been chiefly ufed for a violent ftorm at fea, of larger extent than what I am about to explain, which is of a more contracted nature, and confined to a narrow fphere of action; fo that it requires a particular and fignificant name, fuch as wind-fpout, till a more fuitable one is found for it.

Defcription of one. It begins of a fudden; more or lefs of clouds having been drawn together, a fpout of wind coming from it ftrikes the ground in a round fpot of a few rods or perches diameter, with a prone direction, in the courfe of the wind of the day, and proceeds thus half a mile or a mile. The pronenefs of its defcent makes it rebound from the earth, throwing fuch things as are moveable before it, but fome fideways from it. A vapour, mift or rain defcends with it, by which the path of it is marked and wet.

I fhall produce the inftance of that at Leicefter, a town about fifty miles from Bofton, a few years fince, which being more violent than ufual, may give fome idea of the thing.

It happened in the month of July, on a hot day about four o'clock P. M. A few clouds having gathered weftward and coming over head, a fudden motion of their running together in a point being obferved, immediately a fpout of wind ftruck the ground at the weftern end of a houfe houſe, and inſtantly carried it away, with a negro fellow in it, who was afterwards found dead in the path of it. Two men and a woman, by the breach of the floor, fell into the cellar; one man was driven forceably up into the chimney-corner. Theſe were preſerved, though much bruiſed; they were wet with a vapour or miſt, as were the remains of the floor and the whole path of the ſpout.

This wind raiſed boards, timbers, &c. and carried them before it. A joiſt was found on one end driven near three feet into the ground. I imagine the ſpout took it in its elevated ſtate and drove it forceably down. By what I can learn of its procedure, it continued but three or four ſeconds of time in a place, paſſing along with the celerity of a middling wind, conſtantly declining in ſtrength till it ceaſed.

There ſeems to have been ſuch a guſt as this at cape Cod, about forty years ago, of which I received an account from two men who lived in the neighbourhood of the place. It came on of a ſudden, and was ſo violent that it threw down a young woman who happened to be in the way of it; ſhe was forced to lay hold on the buſhes which happened to be within her reach, to prevent her being carried away by it. It paſſed a pond of water, and the people wondered it did not ſuck up the water, as they conceived it to be a water-ſpout, but it did not. The young woman was however wet with the vapour that accompanied it.

Of Hurricanes, particularly thoſe of the Weſt-India Iſlands.

To account ſatisfactorily for theſe convulſions of our atmoſphere, requires a greater number and more circumſtantial obſervations than we are at preſent furniſhed with; ſo that all that can at preſent be ſaid of their origin and cauſes muſt be very conjectural. However, ſince an attempt to explain them may give occaſion to further and more exact obſervations, I ſhall proceed to offer my preſent thoughts concering them.

 I believe

I believe thoſe of the Weſt-India iſlands to be owing to ſome occaſional obſtruction in the uſual and natural procedure of the equatorial trade. This I conjecture from the more than uſual preceding calms. In the natural courſe of this trade the air riſes up in the line and paſſes off towards the poles, and, in the more contracted degrees of the greater latitudes, proves the courſe of their weſtern trades: So that could this aſcent be prevented through the whole circle of that zone, there would be no more weſterly winds in theſe latitudes than any others.

Over violent rains and cold naturally tend to check the aſcent of air out of this circle, rather making it deſcend. And as there are annual rains in the equator over againſt thoſe iſlands, and in ſome years more than others, it is eaſy to conceive ſuch an effect, and the conſequences. Great clouds and over-much vapour generate cold and weight, while at the ſame time the rains are beating down the air; and as theſe prevent the riſing of the air out of the line, ſo they hinder its uſual progreſs to it from the tropics on both ſides. Thus calms muſt take place; by which the natives uſed to predict approaching hurricanes, without underſtanding the reaſon of the thing.

Much of calms in the inter-tropical climates cauſe rarifactions, and aſcents of air into the upper regions, inſtead of its being carried to the line to be diſpoſed of in the grand circulation of the atmoſphere; this will be the caſe more eſpecially among the iſlands, which increaſe the heat of the atmoſphere. Then by theſe aſcents there will be accumulations of air above, which becoming cold in the higher regions will acquire a greater ſpecific weight, and be diſpoſed to deſcend on the firſt giving way of the more rarified and yielding ſubjacent region; and this will be the caſe when there happens not to be ſufficient motions of air in the middle region to keep ſmooth and even the ſtratums of the more and the leſs rarified regions; and ſo prevent particular portions and places from bending downwards;

wards; and it is this alone that does prevent it. By a failure in this, a descent once begun, the consequences cannot be prevented: The heavy quantity above will continue to descend till all the upper cold regions are exonerated to many hundreds of miles round; and all their contents shifted into the place of the rarified and lighter air below.

Such are my ideas of the causes and operations of a hurricane in those climates. I have only to add here, that the rains in these violent storms are, as I think, a strong confirmation of the doctrine of descent; as they are in that kind of hurricane called by sailors the Ox's Eye, on the coast of Guinea; and the like happens under various names in different parts of our globe. Even the wind in our thunder-gusts is from descent; the air in the cloud being rendered dense and weighty, descends, and flows in the direction of the wind of the time, and with the more violence, by the warm air at the surface giving way to it. These are sometimes strong, but seldom attended with danger or damage.

What objections may be raised against these opinions, shall be candidly attended to; in the mean time there is one objection that must be obviated, the argument being somewhat interested in it. It is as follows.

Having expressed my opinion that hurricanes and tornados or wind spouts have the same general nature, while we see a great disparity in their magnitude and procedure; some explanation seems necessary to prevent mistakes; I think a little consideration of the place, climate, and circumstances may remove the difficulty.

The earth is an oblate spheroid; its diameter many miles greater at the equator than at the poles, caused by its diurnal centrifugal force. If this then has so great an effect on terraqueous matter, it cannot have less on our air, but if any difference, rather more; especially if we consider, that the atmosphere makes a larger diameter, and yet revolves

volves in the ſame time, ſo that its centrifugal force muſt be proportionably greater. The diurnal motion of the earth tends to throw a vaſt ſurplus of air on the equator, by which there is probably more air between the tropics than on the reſt of the globe. But this is a matter of conjecture not to be perceived by any ſort of preſſure any more than by the barometer, for reaſons obvious to thoſe converſant in the nature and effect of the ſeveral principles. However it might not be amiſs to obſerve whether there be any difference in the height of the mercury before any of theſe ſtorms. But to return.

Although the air in the intert-ropical latitudes is in the general lighter than in the remote ones, yet when the upper air has obtained a paſſage downward, it being vaſt in quantity, and occupying great ſpace, it will be long in accelerating and paſſing down. The paſſage is long, ſo that it will gain a great deal of the force we find it has by the length of deſcent. Neither will the middle region be diſpoſed to ſhut up without a briſk wind in it, before the whole, even to remote regions, is diſcharged through the large hiatus, as before mentioned and now repeated, to account for the duration and extent of theſe otherwiſe wonderful winds, with ſuch unrelenting violence.

Far different is the caſe of the high latitude tornados in their circumſtances and their manner, although agreeing in their general nature. The centrifugal force here has extremely little effect, unleſs to caſt the atmoſphere toward the equator inſtead of raiſing or increaſing its quantity over any given place on either ſide. Beſides there is the attraction of the ſun, moon, and all the other planets for ever within the tropics attracting the atmoſphere that way and leſſening the height of the high latitude atmoſphere, which therefore may be ſuppoſed not a fourth ſo high from the ſurface as that.

Since then the atmoſphere is vaſtly leſs in height, and alſo much leſs in quantity than toward the line, the deſcents muſt

muſt naturally be very different. Here are no accumulations aloft. The quantity ready for a diſcharge downward is vaſtly leſs, and the paſſage narrow and contracted; and by the almoſt conſtant motions of air, were there more ſupplies it would ſoon ſhut up. Beſides there is little aptneſs to flow from ſurrounding regions by reaſon of the ſmallneſs of their depth, &c. And yet ſo great is the ſpecific weight of what deſcends, that the firſt aſſault has been known to equal the greateſt violence of the proper hurricanes in their moſt powerful moments.

N° XLI.

The whole Proceſs of the Silk-Worm, from the Egg to the Cocon; communicated to Dr. JOHN MORGAN, *Phyſician at Philadelphia, in two Letters from Meſſrs* HARE *and* SKINNER, *Silk Merchants in London, July* 27, 1774, *and February* 24, 1775.

Read July 8, 1775. IT is ſome time ſince we were honored with your eſteemed letter of 27th September laſt. We ſhould not have delayed ſo long acknowledging its receipt, if it had been in our power to have ſent you before this time the manuſcript you will receive herewith; but it is only lately we have been able to procure it from one of the firſt houſes in Italy. It contains an exact account of the Italian moſt improved method of making raw ſilk. We flatter ourſelves it may prove of ſome ſervice to your new eſtabliſhed manufactory, for whoſe uſe ſolely we ſent for it to Italy.

The large quantity of raw ſilk that continually arrives from China every year, being moſtly of a round or large ſize, will a good deal interfere with the ſale of yours, provided

provided you make it of the ſame; therefore we by all means recommend your reeling yours of the fineneſs of five to ſix cocons, no coarſer at any rate if avoidable. And we further beg leave to recommend your giving orders to your workmen to be extremely careful in aſſorting the ſilk, obſerving that all that is put into one parcel be exactly, if poſſible, of the ſame fineneſs; for if it is not, it will very much prejudice its ſale; a neglect in this particular is complained of in all the ſilk that has hitherto been received from America. If the ſilk, which was very good in itſelf that we received from Georgia, had been properly aſſorted, we certainly ſhould have ſold it 1/6 or 2*ſ*. per *lb*. better than we did. If you reel your ſilk fine the China ſilk rather promotes its ſale than otherwiſe, as it is neceſſary to have fine ſilk to work up with that of China.

We ſhall at all times be very ready to communicate to you any intelligence in our power.

We are, with reſpect,
Sir,
Your moſt obedient ſervants,
HARE & SKINNER.

Chap. I. *Of the Silk-Worm.*

THE perſon who purpoſes raiſing a quantity of ſilk-worms, and preſerving good eggs, muſt begin a year before hand. He muſt chooſe a certain number of good cocons, or ſilk cods, the ſuperficies of which, he ſlightly pierces with a needle and thread and ſtrings them by ſcores; which done, he hangs them up in a convenient room, this being the moſt proper poſition for them. After the moths or butterflies contained in the cocon, have eaten their way through their natural incloſure, (which is generally about four days after the cocon is finiſhed*) you may

* It happens ſometimes the butterfly is longer before its birth, i. e. from 15 to 30 days if the weather is chilly. They generally come out in the morning.

may place them on a linen cloth difpofed vertically, as againft a wall, or on a line, &c. where they couple and are joined during twenty-four hours. This over, the female lays her eggs during other twenty-four hours; after which fhe dies, as does the male; this their fecond life, if I may be permitted the term, is only of forty-eight hours duration. When the eggs are new laid, they are about the bignefs of a common pin's head, and of a ftraw colour; by degrees they become black, affume more folidity, lofing at the fame time part of their bulk.

When they are arrived at this point, you muft feparate them from the cloth; to effect which, you muft dip them into a large pan filled with one half water and the other half wine, rather more than lukewarm; when your cloth has foaked in this liquor a little while, you may feparate them from the cloth with a filver fpoon and dry them in a funny place, and take them away when they begin to be whitifh.

When you have thus detached your eggs, you muft keep them till the next year in a cool damp place to preferve them from hatching during the great heat, which would ruin the project.

On the arrival of the fpring, you muft obferve when the mulberry tree begins to put forth its leaves, which muft be your fignal to expofe your eggs in a very warm place, that they may all hatch at once, otherwife they would only hatch by little and little, and in proportion as each individual would be arrived at the point of its natural maturity. In which cafe the pains required to feparate their different claffes would be exceffive, not to fay impoffible. To hatch your eggs you muft carry them about you nine or ten days, keeping them in your bofom, or other parts near the body; in the night you may put them between the matraffes of the bed. You may likewife hatch them by the heat of an oven, but this method is dangerous, becaufe you may poffibly burn the worm contained in the egg, and thereby deftroy all your future hopes.

II. The worm is entirely black at its birth, and is about as long as an ant. He is rolled up in the egg, which otherwife could not contain him. He preferves this black colour eight or nine days. After your worms are hatched you muft put them on wicker fhelves, which are covered firft with paper and afterwards with a bed of the youngeft and moft tender mulberry leaves; you may place feveral ranges of them in the fame chamber, one above another, provided you leave at leaft a foot and a half between each range; that the fcaffolding be in the middle of the room, and that your wicker fhelves be not too broad, but juft fo as to reach on each fide conveniently to the middle. By degrees the worm grows and requires more room. It muft be your care to thin them, and keep thofe of the fame fize as near as you can on one row; for which reafon you muft always leave fome fhelves vacant for that purpofe.

The worm continues feeding during eight days after its birth, at the end of which he has three lines in length or the fourth part of an inch. He is then attacked with his firft ficknefs, which confifts in a kind of lethargic fleep for three days together, during which fpace he changes his fkin, ftill preferving the fame bulk.

This fleep being over, he begins to eat again during five days, at which term he is grown to the fize of feven lines in length, after which follows a fecond ficknefs*, in every refpect like the former. He then feeds during other five days, and is now about nine lines in length, when he is attacked with his third ficknefs; which over, he continues to eat again five days more, which are followed by his fourth ficknefs, at which time he is arrived at his full growth, i. e. about fourteen lines in length and two in diameter.

He

* You muft obferve that thefe fickneffes are much longer, and laft feven or eight days when the weather is cold.

He then feeds during five days with a moſt voracious appetite; after which he diſdains his food, becomes tranſparent a little on the yellow caſt, and leaves his ſilky traces on the leaves where he paſſes; theſe ſigns denote that he is ready to begin his cocon.

You muſt then furniſh him with little buſhes of heath, broom or other like twigs, ſticking the bundles upright in rows between the ſhelves, and forcing them a little that they may not fall; he remains ſtill two days to climb up the twigs, and ſettle himſelf on a good place, after which he begins to lay the foundation of his lodge, and is five days in ſpinning his cocon. He remains generally about the ſpace of forty-ſeven days.

III. You muſt keep your worms in a dry place, ſheltered and ſhut up cloſe, provided it be not too hot. If the weather be cold you muſt make a ſmall fire. When you furniſh them with leaves, take great care that they be thoroughly dry and ſtrew them lightly over your worms. You muſt obſerve to take away their dung very frequently. When the worms are ready to mount (in order to ſpin) if the weather be ſtifling hot attended with thunder, you will ſee them in a languiſhing condition; your care muſt then be to revive them, which is effected thus.

Take a few eggs and onions and fry them in a pan with ſome ſtale hog's lard, the ranker the better, and make pancake; which done, carry it ſmoaking hot into the room where they are kept, and go round the chamber with it. You will be ſurprized to ſee how the ſmell revives them, excites thoſe to eat who have not done feeding, and makes the others that are ready to ſpin, climb up the twigs.

Theſe little creatures require a great deal of care in the management; one or other muſt attend them day and night; you muſt be very dexterous and gentle in handling them; and, as I may ſay, the whole ſucceſs depends on the care you obſerve and pains you take in rearing them.

The worms cannot ſuffer ſtrong ſmells, ſuch as tobacco and the like, for which reafon you muft avoid offending their delicate organs.

In many parts of Italy, amongft others Romagna and La Marche of Ancona, they have two filk racoltas, or harvefts. They keep the eggs in very cool places, and when the mulberry tree begins to bud again (for during the racolta it is ftripped of its leaves for food for the worms) they expofe their eggs to hatch. Sometimes they give rofe leaves to the young worms, when there are no young mulberry leaves. The cocons of this fecond racolta are rather inferior to thofe of the firft. The filk worm is generally fourteen lines in length and two in diameter and fix and two-fevenths in circumference. He is either of a milk or pearl colour or blackifh: thefe laft are the beft. His body is divided into feven rings, to each of which are joined two very fhort feet. He has a fmall point like a thorn, exactly above the anus. The fubftance which forms the filk, is in their ftomach, which is very long; wound up as it were on two fpindles and furrounded with a gum, commonly yellow, fometimes white, not often greenifh. When the worm fpins his cocon, he winds off a thread from each of his fpindles, and joins them, afterwards, by means of two hooks which are placed in his mouth; fo that the cocon is compofed of a double thread. Having opened a filk worm you may take out the fpindles which are folded up in three plaits, and on ftretching them out and drawing each extremity, you may extend them to near two ells in length. If you then fcrape the thread fo ftretched out with your nail, you will fcratch off the gum, which is very much like bees-wax, and performs the fame office to the filk it covers, as a gold leaf does to the ingot of filver it furrounds, when drawn out by the wire-drawer; the filk then remains of a pearl colour. This thread which is extremely ftrong and even is about the thicknefs of a middling pin.

Three things very remarkable in this infect, are,

1. They

1. They defcribe a femicircle in eating.

2. Their excrement has perfectly the form of a mulberry.

3. They have no fex before their metamorphofis.

CHAP. II. *Of the Cocons.*

I. IT is almoft a general rule to wait fix or feven days after all the cocons feem to be formed, before you take them off the boughs in order to give the worms time to bring them to perfection. It is then proper from that time to give fome air to the room in which you have kept them, in order to diffipate a confiderable dampnefs which the worms exhale on their mounting, (when they have not been well fed and kept, for when they have been properly nurfed this dampnefs is not to be found) and which is of great detriment to the cocons, either by rotting them, rendering them foft, or covering them with fpots.

The cocons may be divided into two general claffes, the white and the yellow, in the yellow you meet with all the fhades from a bright yellow diminifhing at laft to white, fome few are of a pale green. We reckon nine forts of cocons, viz.

1. The good cocons are thofe which are brought to their perfection, ftrong and little, and not at all fpotted.

2. The pointed cocons are thofe, one of whofe extremities rifes up in a point. After having afforded a little filk, the point, which is the weaker part, breaks or tears, and it is impoffible to continue winding that cocon any longer, becaufe when the thread comes round to the hole it is of confequence broke.

3. The cocalons are a little bigger than the other, yet they do not contain more filk, becaufe the contexture is not fo ftrong. In winding they are to be feparated from the reft, becaufe they require to be wound in cooler water, otherwife they furze out in winding.

4. The

4. The dupions, or douple cocons, are ſo called becauſe they contain ſometimes two and ſometimes three worms, who have jointly formed one ſingle cocon. They interlace their threads, for which reaſon they are to be kept aſunder from the reſt; they make the ſilk we call dupions.

5. The ſoufflons are cocons very imperfect, whoſe contexture is looſe, ſometimes to that degree that they are tranſparent, and bear the ſame proportion to the others, as a gauze to a ſattin. Theſe cannot be wound.

6. The perforated cocons are ſo called, becauſe they have a hole at one end, for which reaſon they alſo cannot be wound.

7. The calcined cocons are thoſe whoſe worm, after the formation of the cocon, is attacked with a ſickneſs which ſometimes petrifies it, and at others reduces it to a fine white powder, without in the leaſt endamaging the ſilk; on the contrary, theſe cocons produce more ſilk than the others, becauſe the worm is conſiderably lighter. They are to be diſtinguiſhed by the noiſe the petrified worm makes when you ſhake the cocon. In Piedmont they ſell for half as much again as the others. It is very rare to ſee a parcel of 25 *lb.* of them at a time: 63 *lb.* of theſe cocons have produced 1 *lb.* 1 *oz.* of fine ſilk of five to ſix cocons.

8. The good choquette conſiſts in thoſe cocons whoſe worm dies, before he has brought it to its perfection. They are to be known by the worms ſticking to one ſide of the cocon, which is eaſily to be perceived when on ſhaking it you do not hear the chryſalis rattle. Theſe cocons are of as fine ſilk as the others, but they are to be wound ſeparately becauſe they are ſubject to furze out, and the ſilk has not ſo bright a colour, neither is it ſo ſtrong and nervous.

9. The bad choquette is compoſed of defective cocons, ſpotted or rotten. They wind many of theſe cocons together. It makes a very foul bad qualified ſilk of a blackiſh colour.

II. To

II. To know whether a cocon be good or not you muſt obſerve if it be firm and ſound, or not, if it has a fine grain, and if the two ends are round and ſtrong. The cocons of a bright yellow yield more ſilk than the others, becauſe they contain a greater quantity of gum; but the advantage accrues to the winder only, becauſe all this gum is loſt in the dying. For which reaſon, as well as for certain colours they take better, the pale ſilks are preferred, becauſe having leſs gum they loſe leſs in boiling.

In the number of cocons that are bought, there ought to be neither ſoufflons, nor perforated cocons; becauſe the ſeller is obliged to keep them apart and to ſell them as ſuch; notwithſtanding which, you may always reckon on half profit of theſe ſorts that remain with the others, and if to theſe you add the dupions and choquette, you may calculate them at ten per cent.

The cocons of the mountains are better than thoſe of the plain; there is a greater quantity of white amongſt them. 'Tis true they are not ſo large as thoſe of the plain, but the worm, at the ſame time, is proportionably leſs. The reaſon of which is, that the air of the mountains being ſharper, the worm labours with greater vigour. They ſucceed, likewiſe, better in the dry plains than in the damp and marſhy parts, becauſe the leaf is more nouriſhing. Five or ſix days after the cocon has been detached from the branches, it is your buſineſs to prevent the birth of the worm, who would, otherwiſe, pierce through the ſhell, and thereby render the cocon uſeleſs. To prevent which you muſt put your cocons in long ſhallow baſkets, and fill them up within an inch of the top. You then cover them with paper and a wrapper over that. Theſe baſkets are to be diſpoſed in an oven, whoſe heat is as near as can be that of an oven from which the bread is juſt drawn after being baked After your cocons have remained therein near an hour, you muſt draw them out, and to ſee whether all the worms are dead, draw out a dupion from the

the middle of your baſket and open it, if the worm be dead, you may conclude all the reſt are ſo; becauſe the contexture of the dupion being ſtronger than that of the other cocons, it is conſequently leſs eaſy to be penetrated by the heat. You muſt obſerve to take it from the middle of the baſket, becauſe in that part the heat is leaſt perceptible; after you have drawn your baſkets from the oven, you muſt firſt cover each of them with a woolen blanket or rug, leaving the wrapper beſides, and then you pile them one on the other. If your baking has ſucceeded, your woolen cover will be all over wet with a kind of dew, the thickneſs of your little finger. If there be leſs, it is a ſign your cocons have been too much or too little baked. If too much baked, the worm being over dried, cannot tranſpire a humour he no longer contains, and your cocon is then burnt. If not enough baked, the worm has not been ſufficiently penetrated by the heat to diſtil the liquor he contains, and in that caſe is not dead.

You muſt let your baſkets ſtand thus covered five or ſix hours if poſſible, in order to keep in the heat, as this makes an end of ſtifling thoſe worms, which might have avoided the firſt impreſſion of the fire.

You are likewiſe to take great care to let your cocons ſtand in the oven the time that is neceſſary; for if they do not ſtand long enough your worm is only ſtunned for a time and will afterwards be revived. If on the other hand, you leave them too long in the oven you burn them, many inſtances of theſe two caſes are frequently to be met with.

It is a good ſign when you ſee ſome of the butterflies ſpring out from among the cocons which have been baked, becauſe you may be certain they are not burnt. For if you would kill them all to the laſt worm you would burn many cocons, which might be more expoſed to the heat than that particular worm.

III. When

III. When you put your cocons into the oven, you muſt be very careful in picking out all the ſpotted ones, otherwiſe they communicate their ſpots by the great perſpiration occaſioned in them by the heat. If you have a parcel of ſtrong and another of weak cocons, and you can only wind a part of them freſh (i. e. without baking) give the preference to the weak cocons, and bake your ſtrong ones, becauſe the latter, containing more gum, ſupport the baking much better and ſuffer leſs than the weak ones.

As faſt as the cocons you buy are brought in, put them in baſkets and expoſe them to the ſun, if it ſhines, in caſe your oven be full, in order at leaſt to ſtun the worm and prevent his working to pierce his cocon during that time.

It is very proper likewiſe that they be a little in the air before you put them in the oven; becauſe the peaſants bring them in baſkets heaped one on the other, which heats them and renders them extremely ſoft, but the air brings them to their proper tone again.

Sometimes the peaſants ſell you the cocons ready baked when they have been obliged to keep them ſometime. It is eaſy to know them, becauſe the worms when baked, being dry, make a louder noiſe on rattling them than when they are freſh.

When your cocons are fully baked, and have ſtood long enough, you muſt ſpread them half a foot thick on broad ozier ſhelves, which are diſtributed into as many ſtories as the height of the room will admit of, two or three feet diſtant one from the other; taking care to turn them every day, and to change their places, for otherwiſe there are many inconveniencies that would ariſe from ſuch a neglect. They would become mouldy and the moths would eat them. Beſides this, it is abſolutely neceſſary in order to ſeparate the ſpotted cocons, or the bad choquette, which would ſpread to all the cocons that are near them, and muſt be wound immediately to prevent their damaging any further.

The building where you spread your cocons is called the Coconiere, and consists of one or more large rooms, in which are distributed as many ranges as you can conveniently place, taking care that the supporters touch neither the roof nor the wall, because if there were any rats in the Coconiere they would come down the poles, and destroy the cocons, they being very greedy of the worm contained in them.

A middling cocon has about thirteen lines in its greater diameter, by eight lines the lesser diameter, some are larger, some are smaller; but this is the general size. The dupion has generally fifteen lines great diameter by nine lesser diameter.

The cocon is composed of several strata or surfaces applied one on the other; notwithstanding they all communicate, otherwise it would be impossible to wind them off. It is an easy matter to take off one or more of these surfaces, the uppermost of which is coarser, less gummed, and higher coloured than the undermost. Finally, these surfaces are composed of a fine sort of saliva, whose texture has a tolerable resemblance to the thin skin you find joined to the inside of a hen's egg.

The cocons produce a thread of a very unequal length, you may meet some that yield twelve hundred ells, whilst others will scarcely afford two hundred ells. In general you may calculate the production of a cocon, from five hundred to six hundred ells in length.

IV. The worm or chrysalis, as he is inclosed in his cocon is shrunk up into himself, so that it is but half as long in his primitive state, but it is on the contrary as thick again.

He is of a cinnamon colour, and full of liquor, rather clear, which forms the seed in the males, and the eggs in the females. Though he seems to be insensible in that state, yet you may perceive he is not wholly so, for on piercing him with a pin slightly, you will see him move,

move, and we make uſe of theſe experiments to ſee if they have been killed in the oven.

The worm dries the older it grows, ſo that the ſame quantity, or the ſame number of cocons decreaſes daily in weight. The cocons which encloſe the male butterfly have more ſilk at the extremities, than thoſe which contain the females; but it is very difficult to perceive this difference, the moſt ſkilful connoiſſeurs will miſtake at leaſt twenty in a hundred.

When the worm wants to break his way through, he pierces the cocon, firſt wetting it a little in order to gnaw it the more eaſily; he has then only to ſtrip off his upper coat, under which he has another quite white, with wings.

When he comes out, his wings, which at firſt appear very ſmall, open and diſplay themſelves by little and little, and are entirely at liberty in an hour or two. As ſoon as born he ſeeks a female, and one would ſay he is born again merely to propagate his ſpecies, for he expires a very little time after having performed his function.

CHAP. III. *Of Cocons Royal, Perforated Cocons, and Soufflons.*

THE royal cocons are thoſe which you have kept for ſeed. The worm makes a hole in them for his paſſage, ſo that they cannot be wound, and are in the ſame claſs with the perforated cocons.

Neither can the ſoufflons be wound, becauſe their thread being the produce of a weak, ſick worm, it has not the gum it ought to contain. Beſides they cannot be wound off, their thread being interlaced and entangled.

The uſes you may make of theſe cocons are the following; and firſt for the

Soufflons; you muſt let them boil for about half an hour in common water, after which you muſt dry them. When they are quite dry you muſt threſh them on the

 floor

floor with a flail, to bring out the worm, which is reduced to ashes by the fire and air. Afterwards you put them on a distaff and open them; to effect which you must take them by the two ends and stretch them out at arms length, you may then fasten them on your distaff.

2. The perforated cocons; you must observe the same method as for the soufflons, except that you must let them boil three-quarters instead of half an hour, because they contain a greater quantity of gum.

3. The cocons royal. As it is natural to suppose you keep the flower of your cocons for seed; they are fuller of gum than the others, for which reason you must let them boil an hour; after which you must not thresh them as the former, because they contain no worm, neither is it necessary to stay till they are quite dry before you spin them; on the contrary, they open more easily when damp. The produce of these three sorts of cocons, when worked, makes what we call *fleuret*.

After you have boiled the cocons and threshed them well, to shake out the worm they contain, you may card them instead of opening them as above, you will then make a much more beautiful fleuret, and of a brighter colour, but it will at the same time come considerably dearer, because of the waste in carding. A good spinster performs a very reasonable days work if she can spin an ounce of fleuret.

To sum up the whole, and give you a notion of the value of these three sorts of cocons, you may calculate thus.

If the good cocons are worth one hundred, the perforated are worth thirty-three one third, the soufflons twenty-five, the royal cocons two hundred and fifty; but if your royal cocons are not chosen ones for seed, they are worth but two hundred.

The best fleuret is that which proceeds from the royal cocons, afterwards that of the perforated cocons unchosen, last of all that of the soufflons.

CHAP.

CHAP. IV. *Of the Filature, or Winding from the Worm.*

Although the fresh cocons, that is to say, those that have not been baked in the oven, yield a brighter silk than those that have, and at the same time yield better weight, by reason of part of their gum which they have not lost by the fire, yet most people prefer those that are baked, in order to have a silk more even in its colour; unless you could have a considerable quantity of fresh cocons, and time to wind them so; for otherwise it is undeniable, that the fresh would be much more advantageous, as well for the reason above mentioned as because they are easier to wind, not having been dried by the fire.

Before you begin to wind, you must prepare your cocons as follows.

1. In stripping them of that waste silk that surrounds them, and which served to fasten them to the twigs. This burr is proper to stuff quilts, or other such uses; you may likewise spin it to make stockings, but they will be coarse and ordinary.

2. You must sort your cocons, separating them into different classes in order to wind them apart. These classes are,

The good white cocons.
The good cocons of all the other colours.
The dupions.
The cocalons, among which are included the weak cocons.
The good choquette; and, lastly,
The bad choquette.

In sorting the cocons, you will always find some perforated cocons amongst them, whose worm is already born; those you must set apart for fleuret. As I have described above, you will likewise find some soufflons, but very few; for which reason you may put them among the bad choquette, and they run up into waste.

The

The good cocons, as well white as yellow, are the easieſt to wind; thoſe which require the greateſt care and pains are the cocalons; you muſt wind them in cooler water than the others, and if you take care to give them to a good windſter, you will have as good ſilk from them as the reſt. You muſt likewiſe have careful windſters for the dupions and choquettes. Theſe two articles require hotter water than the common cocons.

The good cocons are to be wound in the following manner. Firſt chooſe an open convenient place for your filature, the longer the better, if you intend to have many furnaces and coppers. This building ſhould be high and open on one ſide and walled on the other, as well to ſcreen you from the cold winds and receive the ſun, as to give a free paſſage to the ſteam of your baſons or coppers.

Theſe coppers or baſons are to be diſpoſed (when the building will admit of it) in a row on each ſide of the filature, as being the moſt convenient method of placing them, for by that means in walking up and down you ſee what every one is about. And theſe baſons ſhould be two and two together, with a chimney between every couple.

Having prepared your reels, (which are turned by hands and require a quick eye) and your fire being a light one under every baſon, your windſter muſt ſtay till the water is as hot as it can be without boiling. When every thing is now ready, you throw into your baſons two or three handsful of cocons, which you gently bruſh over with a wiſk about ſix inches long, cut ſtumpy like a broom worn out: by theſe means the threads of the cocons ſtick to the wiſk. You muſt diſengage theſe threads from the wiſk, and purge them by drawing theſe ends with your fingers till they come off entirely clean. This operation is called la Battüe.

When the threads are quite clear, you muſt paſs four of them (if you will wind fine ſilk) through each of the holes in a thin iron bar that is placed horizontally at the edge

edge of your bason; afterwards you twist the two ends (which consist of four cocons each) twenty or twenty-five times, that the four ends in each thread may the better join together in crossing one another, and that your silk may be plump, which otherwise would be flat.

Your windster must always have a bowl of cold water by her, to dip her fingers in, and to sprinkle very often the said bar, that the heat may not burn the thread.

Your threads, when thus twisted, go upon two iron hooks called rampins, which are placed higher, and from thence they go upon the reel. Now at one end of the axis of the reel is a cog-wheel, which catching in the teeth of the post-rampin, moves it from the right to the left, and consequently the thread that is upon it; so that your silk is wound on the reel cross-ways, and your threads form two hanks of about four fingers broad.

As often as the cocons you wind are done, or break or diminish only, you must join fresh ones to keep up the number requisite, or the proportion; I say the proportion, because as the cocons wind off, the thread being finer, you must join two cocons half wound to replace a new one: Thus you may wind three new ones and two half wound, and your silk is from four to five cocons.

When you would join a fresh thread, you must lay one end on your finger, which you throw lightly on the other threads that are winding, and it joins them immediately, and continues to go up with the rest. You must not wind off your cocons too bare or to the last, because when they are near at an end, the bairré, as we call it, that is the husk, joins in with the other threads and makes the silk foul and gouty.

When you have finished your first parcel, you must clean your basons, taking out all the striped worms, as well as the cocons, on which there is a little silk, which you first open and take out the worm and then throw them into a basket by you, into which you likewise cast the loose silk that comes off in making the battüe. You

You then proceed, as before, with other two or three handsful of cocons; you make a new battüe; you purge them, and continue to wind the ſame number of cocons or their equivalent, and ſo to the end.

As I ſaid above, your windſter muſt always have a bowl of cold water by her, to ſprinkle the bar, to cool her fingers every time ſhe dips them in the hot water, and to pour into her baſon when neceſſary, that is, when her water begins to boil. You muſt be very careful to twiſt your threads a ſufficient number of times, about twenty-five, otherwiſe your ſilk remains flat, inſtead of being round and full; beſides when the ſilk is not well croſſed it never can be clean, becauſe a gout or nub that comes from a cocon will paſs through a ſmall number of theſe twiſts, though a greater will ſtop it. Your thread then breaks and you paſs what foulneſs there may be in the middle of your reel, between the two hanks, which ſerves for a head band to tie them.

You muſt mind your water be juſt in a proper degree of heat. When it is too hot the thread is dead and has no body; when it is too cold, the ends which form the thread do not join well, and form a harſh ill-qualified ſilk.

You muſt change the water in your baſon four times a day, for your dupions and choquette, and twice only for good cocons when you wind fine ſilk, but if you wind coarſe ſilk it is neceſſary to change it three or four times. For if you was not to change the water the ſilk would not be ſo bright and gloſſy, becauſe the worm contained in the cocons foul it very conſiderably. You muſt endeavour as much as poſſible to wind with clear water, for if there are too many worms in it, your ſilk is covered with a kind of duſt, which attracts the moth and deſtroys your ſilk.

You may wind your ſilk of what ſize you pleaſe, from one cocon to a thouſand; but it is difficult to wind more than thirty in a thread. The nicety, and that in which conſiſts confifts the greateft difficulty, is to wind even, becaufe as the cocon winds off, the end is finer, and you muft then join other cocons to keep up the fame fize. This difficulty of keeping the filk always even is fo great, that (excepting a thread of two cocons, which we call fuch) we do not fay a filk of three, of four, or of fix cocons, but a filk of three to four, of four to five, of fix to feven cocons. If you proceed to a coarfer filk you cannot calculate fo nicely as to one cocon more or lefs. We fay for example, from twelve to fifteen, from fifteen to twenty, and fo on.

It is eafy to conceive, that it is more difficult to wind a coarfe filk even, than a fine one, becaufe it is harder to keep a great number of cocons always to the fame fize, than a fmall one.

The dupions which you defign for rondelette, or ordinary fewing filk, are to be wound from fifteen to twenty. The reft you may wind as coarfe as poffible, i. e. from forty to fifty: they ferve to cover and fill up in coarfe ftuffs, and may likewife be ufed for fome fort of fewing filk.

The good choquette is to be wound according to the ufes to which you intend to apply it; however not finer than from feven to eight. The bad choquette you may wind from fifteen to twenty cocons.

In winding the good cocons, you will always meet with fome defective, which will not wind off, or are full of gouts and nubs. Thefe you muft take out of your bafon and keep by themfelves. They are called *baffinats*. They are to be wound apart as coarfe as you can. They make a foul, dirty filk. To have a good filk, you muft wind in fine weather. If the wind be high it fhakes your filk, and prevents its lying fmooth on the reel, forms ftrings of threads, which make it very difficult to wind on bobbins. If the weather is rainy the filk is damp, and has not that luftre it ought to have, or which it has when it dries, as it goes upon the reel. You muft mind not to hank it when damp, but let it dry on the reel; otherwife it would be furzy.

I have now only to ſpeak of the waſte that comes from the battüe, and the huſks of the cocons, that have ſtill ſome ſilk upon them, which are thrown into baſkets in winding, and are what we call *moreſques*. Theſe you firſt dry in the ſun, then threſh, and afterwards card and ſpin them to make fleuret. One hundred and fifty ounces of good cocons yield about eleven ounces of ſilk from five to ſix cocons; if you wind coarſer, ſomething more. You may wind about eleven or twelve ounces of ſilk from five to ſix cocons in fourteen hours.

The ſilk which is made of baſſinats and bad choquette ſerves to make ſtockings and coarſe heavy ſtuffs, ſuch as ſattinades and damaſks for hangings, &c. &c.

N° XLII.

The Art of making Anatomical Preparations by Corroſion. By JOHN MORGAN, *M. D. Profeſſor of the Theory and Practice of Phyſic in the Univerſity of Pennſylvania, Member of the Royal College of Phyſicians at Edinburgh, and F. R. S. at London, &c.*

AS no branch of ſcience more certainly leads to an intimate acquaintance with the functions of the animal body, (which is the foundation of all rational knowledge of the cauſes and cure of diſeaſes) than that of the ſtructure of the vaſcular ſyſtem, the origin, diviſions, different ramifications and numerous inoſculations of the veſſels into, and their communication with each other, I have always thought this field of uſeful information deſerved to be cultivated with great induſtry and attention. In effect it brings us immediately, and in the moſt compendious way, to acquire a knowledge of the nature, and of the motions of the fluids which circulate through them, of their diſtribution throughout the different parts of the body,

body, and of the action and uses of the vessels containing, as well as of the humours contained in them. In particular, it behoves every practitioner of physic to study the vascular texture and composition of the viscera, for upon their healthful action the continuance of life, free from disease, principally depends; and the more their functions are injured, the more dangerous diseases are thereby generated. From a relaxation of them arise atonia and weakness, and from obstruction of them infarctions, inflammations, tumors and schirri are produced. To an acquaintance with their structure and anastomoses, and the fluids they carry, we must be chiefly indebted for our knowledge of the doctrines of resolution and suppuration, and for the indications that point out to the physician by what means to accomplish these desirable events, according to circumstances.

The little progress which practical anatomy has hitherto made in America, and the great consequence it may be of to the rising students of physic and surgery, to employ more of their attention on this useful subject, are my motives for laying before you this essay, in hopes through this channel to stir them up to prosecute it with more zeal and ardor. This becomes the more necessary, because, owing to the late revolution, the subjects of North-America having established themselves into independent states, have at present less commerce and less intercourse with the learned and polished nations of Europe. At least fewer students from America have recourse to them for improvement in the knowledge of their profession than formerly, in as much as medical schools and colleges have been founded in several of these different states, since the author of this essay first recommended and assisted in carrying into execution the plan of transplanting physic, as a science, from across the ocean, by instituting medical schools on this western side of the Atlantic*.

* See his discourse on the institution of medical schools in America, delivered at a public commencement in the college of Philadelphia, May 1765.

Upon our own exertions muſt we therefore chiefly depend for building up the medical fabric, erecting uſeful temples of the healing arts, and diffuſing the lights we can kindle through this new world. I know no one ſtep that can be more uſeful to accompliſh this undertaking, than to teach the art of inveſtigating the ſtructure of the different parts of the animal body, by injections and corroſions, and other preparations of wax.

Such is the preſent ſtate of anatomy in this country that there are at preſent but very few, I believe I may ſay no ſuch preparations worth mentioning to be met with here, that have been made in America. Doctor Chovet, now reſident in this city has indeed a good collection of wax preparations, of different parts of the human body, which he made in his younger days and brought hither from Europe. But nothing of this kind has hitherto been practiſed, or it has been ſo taught as never to have been of laſting uſe to any that I know of.

Being well acquainted with the general deſire that fills the breaſts of my countrymen, to acquire and improve every kind of ſcience that is uſeful, which is properly laid before them, I truſt this attempt will ſtir up many to learn and practiſe thoſe leſſons which, for their particular benefit, I now unfold to them; nor do I doubt in a little time but we ſhall ſee ſuch an emulation kindled for improving on theſe hints, that all kinds of uſeful preparations will be ſo common after a while, as not only to give riſe to anatomical cabinets and repoſitaries for ſpecimens of the animal, vegetable and foſſil kingdoms, as will tend to throw great light upon philoſophy in general, but contribute to lay a ſolid and uſeful foundation of natural hiſtory in America.

The firſt rudiments of this art that I acquired was from the two Hunters, known through all Europe for their ſuperior ſkill in anatomy, and acting as practical diſſector to the celebrated doctors Colignon and Smith, profeſſors of anatomy in the univerſities of Cambridge and Oxford,

ford, which I further improved by practice at Paris with Monſ. Süe, to whom I am wholly indebted for my knowledge of anatomical preparations in wax.

The kind of preparations of thoſe parts of the animal body which admit of it that I now propoſe to explain, namely by injection and corroſion, exceeds in beauty, nicety and uſefulneſs, that which is commonly called diſſection.

In fact, in this latter, we can trace nature but very imperfectly, becauſe by diſſection, the larger veſſels only are preſerved from the knife, and for the moſt part all the ſmaller are unavoidably cut away.

On the contrary, in anatomical preparations by corroſion, even the very ſmall veſſels may be kept entire, and we can ſee, at a caſt of the eye, the courſe and diſtribution of all the vaſcular ſyſtem even to the ſize of an hair, called capillary veſſels, and thoſe too diſengaged from the ſurrounding parts, which otherwiſe wholly conceal, or make them difficult to be perceived. It is impoſſible that with only the aſſiſtance of a diſſecting knife, any perſon ſhould be able to lay open to view all thoſe ſmaller veſſels, however ſkilful and experienced the hand may be that directs it. The exact and perfect imitation of nature which this ſort of preparations preſents, the eaſe with which they are made, and their extraordinary beauty and neatneſs, render a knowledge of this art ſo much the more deſirable.

The art of injecting the very fine veſſels of the body with common injection, was well known to the celebrated Ruyſch, the moſt famous anatomiſt, in that way, of any living in Europe in his day; and therefore it has been ſometimes called the Ruyſchian art, but it fell ſhort of the one I now undertake to explain, becauſe in his preparations the minute veſſels only become viſible, ſo far as the ſubſtance through which they proceed was tranſparent, but our art extends to the removal of every ſurrounding

rounding fubftance, and leaves them entirely naked and perfectly expofed to the eye. I once fhewed a preparation of the veffels of a kidney I had thus executed at Paris, to a meeting of the French academy of furgery in the year 1764, who allowed it to be curious and quite new to them. I think none of the members prefent at that meeting, except Monf. Morand, fecretary of that academy, who had been in England and was acquainted with doctor Hunter, alledged their having ever feen a fimilar preparation. At their requeft I prefented a memoir on the fubject, and fince that time Monf. Süe has beftowed one entire fection in treating exprefsly upon it, with a polite acknowledgement of his having acquired his knowledge from me, only with the particularity of naming me as one of the faculty of Edinburgh, without taking any notice of my being a Pennfylvanian by birth, or native of America, which have led fome into miftakes concerning the author of that piece. The reafon may be that Americans before the revolution, being but little confidered in any other light than as colonifts, their nation was feldom taken notice of, and I was introduced to him firft as a graduate of the univerfity of Edinburgh, and known to him afterwards as a member of the royal college of phyficians of that place.

Thefe preparations are, fince that period, become common in France, and the art is now well known, and cultivated fuccefsfully by Monf. Süe and others; but it was unknown there till I communicated it, firft at Paris, and afterward in the fouth of France; where I had the honour of explaining it to the illuftrious Monf. Imbert, chancellor of the univerfity at Montpelier, and to Monf. Bourgelas, principal of the Ecole veterenaire, or academy eftablifhed at Lyons for the improvement of the fcience of horfemanfhip, juftly celebrated for his very elegant preparations of the anatomy of horfes, &c. But what gave me equal pleafure and furprize, was the admiration excited on my

prefenting

presenting only a part of the vascular preparation of a kidney by corrosion, (the rest being broke down in a journey by land of above a thousand miles) which was expressed by the celebrated Morgagni, illustrious professor of anatomy in the university of Padua. He had kept up a literary correspondence with Ruysch when alive, had been favoured with specimens of this great man's preparations, and declared that in comparison to the preparation I gave him, they were "*rudis indigestaque moles.*" From this small specimen, he said, "ex ungue leonem," he could readily comprehend that the usefulness of this kind of knowledge amongst the learned in anatomy, must become great and extensive.

I mention these anecdotes merely to show how recent, or at least how confined the knowledge of this useful art then was, being limited, as far as I know, to Great-Britain only. I suppose it to be owing to this circumstance, viz. that real practical anatomists who have excelled in their preparations, have too generally kept secret the methods and arts they employed in making those preparations. For this reason, much I think is due to the memory of the great Professor Monro, of Edinburgh, who has published a paper upon the art of making injections.

So far as I can learn, this art cannot be traced farther back than to the learned Dr. Nichols of London, who formerly gave lectures in anatomy both there and at Oxford, and from whom Dr. Hunter acknowledged to his pupils that he received his first information. He then deserves to be looked upon as the author and inventor of this art. When Dr. Nichols declined the business, Dr. Hunter and his brother Mr. Hunter, took up the profession of anatomy. Without doubt, those unrivalled brothers in anatomical skill, made considerable improvements in the art of injecting and dissecting animal bodies; and it is likely improved the composition of injections for corrosion.

My

My well meant intention of marking the riſe and progreſs of this art, and of exciting an emulation in my countrymen to proſecute and improve it, will, I hope, not only excuſe but juſtify both the matter and length of my introduction.

Theſe preliminaries being thus ſettled, I now proceed to the main object of this communication. Without expatiating upon the advantages that will attend an accurate knowledge of this art, for the ſake of perſpicuity I ſhall here reſtrict myſelf to laying the following obſervations before you, reduced to general heads, in as few words, and in as conciſe a manner as I am able. They may be uſeful to thoſe who wiſh to put them in practice, and will perhaps give occaſion to perſons who are curious in making experiments in anatomy, to light up ſome new diſcovery.

The art of making anatomical preparations by corroſion, depends on the following principles.

1. We ought for the matter of injection to make uſe of a ſubſtance that is poſſeſſed of a ſuitable degree of conſiſtence, and fine enough to penetrate into the minuteſt veſſels, and which at the ſame time has ſuch a firmneſs of texture as not to alter with the changes of the temperature of the atmoſphere, that is, it ought not to be ſubject to melt with the ſummer's heat, nor to break down from its brittleneſs on being gently handled in the winter.

2. The colours to be employed for ſake of diſtinguiſhing the different orders of veſſels, whether arterial, venal, tracheal or others, ought to be of ſuch a nature as not to be changed upon application of the menſtruum.

3. We ought to make uſe of a menſtruum that is capable of conſuming the muſcular, parenchymatous, cellular or fatty parts that ſurround the veſſels, without affecting the ſubſtance that we uſe for injection to fill the veſſels.

4. Care and addreſs are neceſſary in the perſon who makes the injection.

5. Laſtly, great attention is requiſite in removing the looſe and corroded parts, and in ſeparating them from the injected

injected veſſels without breaking them down by the force applied in cleanſing them.

The following directions will ſerve to guide the operator in theſe different manœuvres.

The common injections are compoſed of wax and ſuet, or of wax and oil; the ſuet or oil is made uſe of to ſoften the wax, and to give it the neceſſary conſiſtence. We cannot employ ſuch a compoſition in our corroded preparations, being oppoſed to the third principle laid down; becauſe the menſtruum we uſe for deſtroying the parts that ſurround the veſſels, will alſo attack and conſume the fat and animal ſubſtances which enter into the compoſition of the injection. But we may employ the following compoſitions, the goodneſs whereof has been proved by repeated trials, viz.

First Receipt.

Take of white or the beſt yellow wax and purified roſin each equal parts, e. g. ten or twelve ounces; melt them together and add a ſufficient quantity of ſpirit of turpentine, to give a due conſiſtence, that is from ſix to eight ounces.

It is adviſeable to melt the roſin firſt, and ſtrain it through a piece of fine linen; becauſe, in the ſtate it is bought out of the ſhops, it is often mixed with foreign ſubſtances.

I am of opinion this injection will turn out to be finer than the following, that is, it will penetrate into ſtill ſmaller veſſels, but it is thought to have the inconvenience of being more brittle; ſo that after corroſion, the moſt ſlender of the veſſels are more liable to break down in handling the preparation.

The following is the receipt which the celebrated Meſſrs Hunter of London, have commonly made uſe of. It is leſs brittle and produces a firmer coheſion of parts, with nearly the ſame conſiſtence as the former. Beſides, it enters very ſufficiently into the capillary veſſels.

Second Receipt.

Take of pure roſin eight ounces, of wax four ounces, of Venice turpentine a ſufficient quantity, that is, about eleven or twelve ounces, to procure a proper confiftence to the injection.

The method of afcertaining the due confiftence and the neceffary firmnefs of the injection, is by taking up any quantity of it, whilft melted, with a fmall wooden fpatula, and then letting it fall drop by drop on the furface of cold water. This immediately fpreads and forms into a thin plate. By rolling it between your thumb and finger firft moiftened, or in the palm of one hand with the fingers of the other, both previoufly made wet to prevent fticking, turn it into the fhape of a cylinder or fmall blood-veffel, then throw it into a bafon of cold water, and let it remain till it is quite cold. If it is then of fuch a confiftence as not to yield to a very flight force, when preffed between your thumb and finger, and yet fo foft as to be capable of bending readily without breaking, it has the due medium of firmnefs and flexibility which is defired. If it appears to be too foft, a further quantity of wax and rofin are to be added in the above mentioned proportion, till it acquires the wifhed for confiftence. If, on the other hand, it is too hard, a proportionably larger quantity of the Venice turpentine is to be added. The fame precaution is to be obferved, if we make ufe of rofin, wax and fpirit of turpentine, as directed in the firft receipt.

Operators feldom are at the trouble of weighing the ingredients; they generally judge of the refpective weights and proportions of each by the eye. This method of determining them will anfwer very well for perfons who have acquired experience; for the different feafon of the year when the injection is made, and the different confiftence or purity of the wax and rofin, with other little circumftances which fometimes happen, occafion fome little variation. In general there is not fo great danger of fpoiling the preparation, by making the compofition a little fofter than is required, rather than harder, becaufe it grows fome what harder by time, and alfo by fteeping the

the parts injected in water, for the ſake of waſhing off the menſtruum that we have made uſe of for performing the neceſſary corroſion.

Theſe injections are well ſuited to make corroded preparations of the viſcera, as of the heart, lungs, liver and kidneys. Yet I doubt not but perſons of ingenuity, who ſhall be at the pains to render themſelves converſant in the art of injection, by giving attention to every circumſtance, will acquire further ſkill and may find other ſubſtances, equally fit for injection without being ſo liable to become brittle, which thoſe compoſitions I have given above are, in ſome degree, even when made with the utmoſt care and exactneſs.

To know whether any ſubſtance of which a perſon wiſhes to make a trial for injection, will withſtand the action of the menſtruum he means to employ, it is ſufficient to put a piece of the compoſition to be uſed as an injection into a ſmall quantity of the menſtruum, and let it remain in it for a week or fortnight; by that means he can judge of its goodneſs, before he is at the trouble of making, or of ſpoiling an injection, as the caſe may happen.

Of the Colours.

The colours we commonly uſe to diſtinguiſh the different orders of veſſels are, 1ſt. Vermilion for colouring the injection for the arteries. 2d. Blue verditure, or what is called Pruſſian blue, for the injection to be thrown into the veins. 3d. For colouring the injections to be thrown into the ureters and pelvis of the kidneys, and the tracheal veſſels of the lungs, what are called in Engliſh king's yellow and flake white are moſtly uſed. A variety of other colours may be employed, but theſe are the principal and the beſt.

We ſhould obſerve to melt the wax thoroughly over a ſlow fire, and the colouring powders ſhould be added by degrees, ſtirring them well in at the ſame time, before the other ingredients are added. This method prevents any effervefcence, which often happens when there is too great a fire, or when all the ingredients are mingled together before the colouring powders are added, efpecially the two laft. The quantity is judged of by trial.

Of the proper MENSTRUUM.

I now pafs to the third head, viz. to confider what fubftances will anfwer, for confuming all the parts furrounding the injection, and leaving the matter in the veffels themfelves untouched.

The beft I have yet tried is the concentrated fuming acid of marine falt, which comes over in the diftillation employed in the procefs for making glaubers falt; it fhould not be diluted with water, nor be dulcified, otherwife it becomes too weak to anfwer the purpofe, or at leaft the time required for compleating the corrofion is thereby protracted beyond what is needful.

The concentrated acids of vitriol and of nitre, are no lefs powerful to deftroy all the animal fubftance, furrounding the injected veffels, but the objection to which, from feveral trials, they appear to be liable, is that they are fuppofed to crifp the veffels; at leaft the fpirit of fea-falt has been moft ufed, and concluded to be the leaft exceptionable menftruum for this operation.

Such were the fentiments I communicated, in my memoir to the royal academy of furgery at Paris in the year 1764, fince which, Monf. Süe, royal profeffor of anatomy in the fchools of furgery, and in the royal academy of painting and fculpture at Paris, having honoured my communication to the abovementioned academy, with a fection in his treatife entitled Anthropotomie, chap. 2. fect. 25. from page 70 to page 84; after acknowledging that he received the art of making thofe preparations from me, thus adds. " Since that time my nephew and I, having worked at them a great deal, and with abundant fuccefs, cèſs, we have diſcovered, that we may change the menſtruum, without injuring the preparation at all, and employ aqua fortis, or the nitrous acid in place of the fuming ſpirit of ſalt; and that aqua fortis is even a more perfect menſtrum, than the ſpirit of ſalt, in as much as the colour of the injection is thereby leſs changed, and the ſmall veſſels better preſerved. Beſides the difference of expence is conſiderable, as the ſpirit of ſalt is worth eighteen livres a pint, whilſt the aqua fortis coſts at moſt but two livres, and the effect is the ſame for quantity. I have alſo employed the ſpirit of nitre for the ſame purpoſe, with great ſucceſs*."

In reſpect to the fourth Principle, namely, the Addreſs of the Operator, and wherein it conſiſts.

He ought to guard againſt cutting away or removing the cellular and other ſurrounding parts, before he has made the injection. In fact, theſe give firmneſs to the veſſels, and prevent their ſtretching unnaturally, or aſſuming forms contrary to nature, from the impulſe of the injection when drove into them by the hand of the anatomiſt. Theſe ſubſtances enable them to reſiſt the too great extenſion and yielding to the force applied.

The injecting pipes ought to be proportioned to the ſize of the veſſels through which the injection is to be made.

It is proper to ſoak thoſe parts in warm water, which we are about to inject, for a ſhorter or longer ſpace of time, as well to waſh them clean, as to carry off the blood and other fluids, and the better to diſpoſe thoſe parts to receive the injection, with which they are to be filled.

The

* Monſ. Morgan, Docteur en medicine de la faculté d'Edinbourg, en a donné une deſcription exacte à l'Academie royale de la chirurgie, et c'eſt de lui que je tiens l'art de préparer ces parties; mais depuis après y avoir beaucoup travaillé, mon neveu et moi, et y avoir eu beaucoup de ſuccès, nous avons decouvert qu'on pouvoit changer le menſtrue, ſans préjudicier en rien à la preparation, et employer, au lieu de ſel fumant, l'eau forte, qui eſt même un menſtrue plus parfait, que l'eſprit de ſel, puiſque la couleur de l'injection eſt moins changée, et que les petits vaiſſeaux ſont mieux conſervès. D'ailleurs la différence eſt encor bien grande pour la dépenſe, puiſque l'eſprit de ſel vaut 18 liv la pinte, au lieu que l'eau ſorte ne coûte tout au plus que 2 liv, et que la quantité eſt la même pour l'effet. J'ai employè auſſi avec beaucoup de ſuccès l'eſprit de nitre. Anthropotomie, pag. 83. 84.

The fubftance employed for the injection ought to be entirely melted over a moderate fire, and be heated to the exact degree that will not permit the cooling of it too faft, upon coming into contact with the parts into which it is impelled, nor ought it to be fo great, on the other hand, as to burn or crifp the veffels, or prove troublefome to the operator in handling the fyringe or pipes, whilft he is making the injection.

The injection fhould be thrown into the part to be prepared, at one uniform impulfe, made flowly and evenly, with a fteady hand, and with fo little force as not to endanger a rupture of the veffels in the foft parts. When the injection is finifhed, the pipes fhould be corked or otherwife clofed, and the parts injected fhould be fuffered to cool by degrees. If they are plunged at once into cold water, before the fubftance of them has acquired a certain degree of hardnefs and firmnefs, a contraction in the elaftic coats of the veffels may be produced, fufficient to occafion a rupture, efpecially in the capillaries, which will always be followed by an extravafation of the injected fubftance.

Having expofed the injected parts to the air during an hour or two, it is proper to commit them to cold water all night, to cool and to harden them thoroughly. After taking them out of the containing veffel and abforbing the water from their furface with a fpunge gently applied, or fuffering it to run off by draining, they fhould be put into a fufficient quantity of the menftruum to cover the preparation entirely.

The next confideration is how to make ufe of the menftruum for corroding the parts to be diffolved and removed from the veffels. For this purpofe the operator fhould be furnifhed with a china bowl or a ftone veffel, on which the menftruum can make no impreffion; or what will anfwer ftill better, a glafs veffel with a mouth fufficiently large to put in and take out the injected parts, without any difficulty.

difficulty. It would be well to furniſh it with a proper cover to reſtrain the acid fumes from eſcaping. I have always uſed a cover of cork lined with wax, into which, whilſt it is in a melted ſtate, the inferior ſide of the cover may be dipped; and this cover muſt be cut ſo as to fit exactly the mouth of the veſſel. The great advantage of a glaſs veſſel over the others is its tranſparency, whereby we are able to ſee how the corroſion goes on, and to judge when it is finiſhed. This takes up from ſix or ſeven days to a fortnight or three weeks, according to the nature of the part to be prepared, and to the quantity and concentricity of the menſtruum employed, in which it ought to be entirely covered. When the acid is very dilute, it proves rather antiſeptic and a preſerver of animal ſubſtances, than a corroſive menſtruum.

Of diſengaging the Corroded Subſtance.

Fifthly. The laſt part of the operation conſiſts in diſengaging the looſened and corroded ſubſtance from that of the injection. In this piece of buſineſs we ought to take the greateſt care, if we wiſh to avoid breaking down the beautiful ſmall veſſels of the part. With this view the acid ſpirit employed as a menſtruum ſhould be decanted from the injection with great caution, whenever the corroſion of the ſurrounding ſubſtance is complete, ſo that it no longer adheres to the veſſels. In place of the corroding menſtruum, ſoak the preparation in ſimple water for three or four days. The looſe ſubſtance may be removed from the veſſels, by pouring freſh water over the preparation ſlowly, and in ſmall quantity at a time; or otherwiſe we may put the preparation in a veſſel pierced with holes, like a cullender, and place this in ſuch a manner as to receive a gentle current or ſtream of water. If we ſhould place the veſſel near the noſel of a pump, and under the droppings or ſmalleſt ſtream which we can procure to fall from it, the preparation may be thus cleanſed from the looſe corroded matter with which it is encompaſſed.

But

But the method I have discovered, and always practised as the safest and best, is to make use of a small syringe, the piston whereof works easily, with which, whilst the preparation is covered three or four inches over with water, I syringe gently, so as to wash and clean it entirely from the corroded substance which is but loosely attached to it. In this manner, using proper care, it may be performed perfectly without breaking down any, even the finest parts, of the tender vessels.

But if more force than needful is employed, even the droppings of a pump from a too great height, when the preparation is taken out of the water, will sometimes break down the extremities of the small vessels, and mar the beauty of it.

After all these directions have been well executed, the preparation is to be suspended for some time in a safe place, till it is dry; then it is to be fixed on a wooden pedestal, having a socket like a candlestick, in which it may be fastened with a little glue or melted wax. Then let it be covered with a transparent glass in form of a globe or bell, with the mouth downward, to guard it from accidents. This finishes the work.

These preparations give us a most exact knowledge of all the ramifications and anastomoses of the vessels, and often of the junction of the arterial and venal system, when the injection is fine enough, and so successfully thrown from the arterial trunk as to penetrate into the veins and fill that system of vessels in the organ prepared, completely, at one and the same time. Thus I have filled both the emulgent arterial and venal system of vessels, in a kidney, at one coup de main, through a single pipe fixed in the great trunk of the emulgent artery, the corresponding trunk of the vein being shut up with a ligature. But it is more common for the injection made by the artery to stop at the extremities of the evanescent branches, and to fill the venal system by a second injection, drove through the great trunk of the emulgent vein.

Upon

Upon the whole, these teach us, in the best manner possible, the true and intimate structure of the viscera in general, and of every particular part; from whence we may assert, without fear of any just censure, that preparations thus executed are exceedingly useful, and enrich the cabinet with choice and beautiful specimens of anatomy.

I have only to add that, in order to enable the artist to succeed and push his discoveries, it behoves him to learn the art of preparing the subject by dissection. This is a new branch, though intimately connected with the foregoing: There are few books that teach it; but one very excellent treatise on the subject is published in French by Monf. Süe, already quoted by the title of anthropotomy, or the art of dissecting, injecting, embalming and preserving the parts of the human body; which, as I think it will greatly contribute to improve anatomy, I have some thoughts, at my leisure, to translate into English, for the benefit of the students of anatomy, physic and surgery in America.

I here subjoin the manner of making Wax Preparations by Monf. Süe.

"WHEN we have a mind to make any preparation of wax, we ought to begin by moulding the part we wish to imitate with fresh plaister of Paris made very fine, taking care to oil it previous to the application of the plaister.

"When the plaister laid on the surface of the part is cold, remove all the pieces that compose the mould one after another, taking care not to break any of them. After

MANIERE DE FAIRE LES PREPARATIONS EN CIRE.

LORSQU'ON veut faire quelque preparation en cire, il faut commencer par mouler la partie que l'on veut immiter avec du platre frais et bien fin, ayant l'attention de bien huillér la partie avant que d'appliquer le platre. Lorsque le platre sera refroidi de dessus la partie, alors on otera toutes les pieces qui composent le moule, l'une après l'autre, prenant garde qu'aucune

After leaving them to dry for ſome time, they may be ſafely uſed. For this purpoſe, melt a ſufficient quantity of virgin wax in a ſkillet, over a gentle fire, and colour it according to the colour of the part which is to be imitated, with carmine, or other paint; or if the piece to be imitated has ſeveral parts of different colours, we muſt not then colour the whole piece at once, but are to place the colour required upon each part, after the piece has been melted.

" Whilſt the wax melts, prepare the mould, which we muſt be careful to oil well with a little bruſh to prevent ſticking; then join all the pieces of the mould together, which muſt be tied faſt with ſmall cords or twine; and that the air may not paſs through the cracks or joinings of the pieces of the mould, place ſome clay on the outſide, by way of luting.

" The mould being thus prepared, and having taken care to leave an opening, we pour the wax into the mould through it, and then carefully turn the mould every way, in order that the wax may ſpread equally through all the interior parts, of it until it is cold. If it be found that the piece is not thick enough, we are to pour on more melted wax, and turn it as before; then let the piece cool in the mould: afterwards, with proper precaution, we are to take the pieces of the mould apart, one after the other. The preparation being taken out of the mould entire, we muſt take off the ſuperfluous portions of wax which penetrated the

qu'aucune ne caſſe. On le laiſſe ſecher pendant quelque tems. Enſuite on peut s'en ſervir. Pour cet effet on fait fondre de la cire vierge dans un poulon à petit feu, et on le colore, ſuivant la couleur de la partie qu'on veut repreſenter, avec du carmin, ou quelque autre couleur; ou bien, ſi la partie a pluſieurs parties de couleur differente, alors on ne colore point la piece. Et on met la couleur ſur chaque partie après que la piece a ete fondüe. Pendant que la cire fonde on prepare lè moule, qu'on a le ſoin de bien huiller avec un pinceau. Enſuite on reſemble toutes les pieces du moule, qu'on retient unies avec des cordes ou de la ficelle, et pour que la cire ne paſſe par les ſentes ou les jointures des pieces du moule, on y met ſur les jointures exterieurement, de la terre glaiſſe. Le moule preparè ainſi, et ayant eu l'attention de y laiſſer une ouverture, on verſe la cire dans le moule, et l'on à le ſoin de tourner le moule en tout ſens, pour que la cire ſe repande egalement par tout l'interieur du moule, juſques a ce qu'elle ſoit rafroidie. Si vous jugez que la piece ne ſoit pas aſſez epaiſſe, vous remettez de nouveau de la cire, et vous faites comme ci deſſus. Aprés quoi on laiſſe rafroider la piece dans le moule, et enſuite on tire avec beaucoup de precaution les pieces du moule, l'une après l'autre. La piece etant entierement depouillée, on la repare. C'eſt a dire qu'on otc de deſſus the joinings of the pieces of the mould, which being entirely repaired, we then colour the different parts which compofe the piece with colours fuitable to each part, that the arteries may be coloured with vermillion; the veins with Pruffian blue; the mufcles with carmine; and fo of the other parts; *which finifhes the preparation.*

" *Note*, When we wifh to make the wax lefs brittle, we muft add fome fpermaceti to it; and fometimes a little of the fineft Venice turpentine that can be procured."

fus les portions èxcedentes de cire qui fe font glifsèes entre les jointures des pieces du moule. La piece etant entierement reparèe, on colore les differentes parties qui compofent la piece avec des couleurs convenables a chaque partie, en forte que les arteres feront colorées avec du vermillon; et les veines avec du bleu de Pruffe; les mufcles avec du carmin; ainfi des autres.
Nota que quand on veut rendre la cire moins caffante on y adjoute le blanc de baleine, quelque fois auffi un peu de terebentine de Venife, tout ce qu'il y a de plus fin.

N° XLIII.

Of a living Snake in a living Horfe's Eye, and of other unufual Productions of Animals. By JOHN MORGAN, *M. D. F. R. S. London, Profeffor of the Theory and Practice of Phyfic, Philadelphia.*

Read June 5, 1782.

WHETHER there is fuch a thing in nature as equivocal generation, by which is to be underftood the production of any new animal independant of a parent ftock of the fame kind, has been a fubject of controverfy amongft philofophers; fome afferting the reality of this doctrine, whilft others, as the celebrated Harvey and his followers as ftrenuoufly reject it. The latter, which is now deemed the orthodox fide of the queftion, affirm that the young of all are produced from an egg, furnifhed by the female, and fœcundated by a male animal. From the light thrown upon this fubject, by the deep refearches of Hippocrates, Galen and Ariftotle, among the antients; and amongſt the moderns by Malphigi, De Graaf, and above all others by the beforementioned renowned Harvey, phyſician to king Charles the firſt of England, (the diſcoverer of the true circulation of the blood) this induction ſeems to be eſtabliſhed upon an induction of facts and experiments, carrying with them the force of conviction, ſo far as that induction reaches.

The only room which ſome ſuppoſe there may be for doubt of the univerſality of the propoſition is, that a variety of animals have been found, at different times, to exiſt in the bodies of other animals, and in extraordinary places, which neither the diſcoverers of thoſe animals, nor others have been able to trace, with clearneſs and certainty, to what mankind, in general, can deem a probable or ſatisfactory origin. Whether it be owing to an impatience to arrive at ſome concluſion, which can ill brook the difficulties of the inquiry, or to the rareneſs of the caſes falling under the notice of perſons capable of making a thorough inveſtigation, and the tedious progreſs of experimental knowledge; or whether it is that we are apt to ſuppoſe the ſubject does not admit of mathematical certainty from the light of philoſophy, I know not; but ſome men have had recourſe to the doctrine of equivocal generation, to account for thoſe productions, as Ariſtotle and his followers had, in other caſes, to certain occult qualities; a term by which they have endeavoured to conceal their ignorance of what they could not explain, but were unwilling to confeſs. Nor are there wanting, in the preſent day, many perſons, who will ſooner deny the teſtimony of their ſenſes, than allow the exiſtence of an animal production, which they know not how to account for.

This I ſuppoſe to proceed from a falſe pride, or an apprehenſion of being deemed credulous in a philoſophic and enlightened age; and becauſe, in times of ignorance, the paſſions of illiterate men were wrought upon by fictions to believe in prodigies, whereby they were led blindfold, into opinions

opinions of religion and philofophy, which had no folid foundation, the race of fceptics I refer to deem it manly not only to with-hold their affent from truths they do not underftand, but to difown and difpute the reality of them. They do not confider that, by fuch conduct, they endeavour to diveft themfelves and others of their rational faculties, and of that natural curiofity implanted in man by his Creator, for the wifeft purpofe, as a guide for inveftigating facts, in order to lead him to knowledge, which has given birth to difcoveries of the greateft importance to mankind.

In anfwer to the cavils of minute philofophers, I would briefly remark the firft ftep to new difcoveries, is an exact attention to the phænomena of nature, unbiafed by preconceived hypothefes, and that it is as much a mark of a defective underftanding to admit too little for truth, upon evidence, as to believe too much from credulity.

I have been led into the above train of obfervations from a fingular phænomenon that may be now feen in this city, and which is worthy the infpection of the curious. It is advertifed in the public newfpapers, viz. the Pennfylvania Gazette, May 23d, as worthy of the attention and critical infpection of all curious perfons, whether philofophers or phyficians, and particularly the latter, as it may, for what they know, if properly examined into, throw fome ufeful light upon the functions and difeafes of the animal body.

What I refer to is an horfe with a fnake in its eye, to be feen in Arch-ftreet, between Sixth and Seventh ftreets, not only poffeffed of mere life, but endowed with a very brifk locomotive faculty. True philofophers will not treat the affertion as idle, fictitious or romantic, but fee and judge for themfelves.

The writer of this piece has undertaken the prefent tafk, on purpofe to excite every clafs of people to fatisfy themfelves of the reality of the fact, that when recorded in the very place where all have it in their power to determine its exiftence, on the teftimony of their own eyefight, they may

not

not plead ignorance and unbelief. He profeffes, for his own part, to be as little credulous or liable to impofitions, from accounts of pretended miraculous appearances, as his neighbours, however learned. Indeed he has ever ftrenuoufly oppofed, and thinks he ever fhall, what he deems empty tales of vifionary fpeculatifts, bred by weak fancies, or raifed by defigning men, to amufe or deceive the vulgar; but he admires and reveres the unfearchable wifdom of the divine architect, who framed this fpacious univerfe, teeming with myriads of animal beings, as well in thofe inftances where his defign and footfteps are vifible, as in thofe which lay more remote from human comprehenfion. Upon the firft relation of this curious hiftory from others, unacquainted with the ftructure of the eye, and therefore more likely to pafs a wrong judgment; and, till he had an opportunity to examine it himfelf, he believed the appearance to be fome unufual difeafe, or a filimentary production on the cryftalline humour, from a ftroke or inflammation of the eye, and that a convulfion in the nerves of its coat might produce an irritation in that organ, and a tremulous motion, which might impofe upon thofe who, not knowing how to account for the appearance, fhould content themfelves with calling it a fnake in the eye, merely from its refemblance, on firft fight, to that animal. But from the clofeft ocular examination, with unwearied attention, repeated more than once, he conceives he is not miftaken, in afferting that there is a real fnake in the eye; which, from the vivacity and brifknefs of its motion, exceeds that of any worm, and equals that of any kind of ferpent he has ever feen.

To fatisfy the public in general, as well thofe who have now an opportunity of feeing it, as fuch who may happen never to fee it, I think it will not be amifs to defcribe its appearance, and to deliver what I have been able to collect of its hiftory.

The

The horſe in whoſe left eye this extraordinary *luſus naturæ* is viſible, is of a ſorrel colour, nine years old; it belonged to Doctor Dayton near the lines at Elizabeth-town, and, I am told, appeared to have no uncommon appearance in either eye, till within a few months ago. The firſt particular circumſtance which excited the owner's attention was, that having lent him to a friend to take a ride in a chair, although it was not known to be vicious or unruly before, it could not now be kept under any government, but ran away with, and daſhed the chair to pieces. The right eye ſtill continues in a ſound ſtate.

Soon after, viz. about ten weeks ago, Mr. Richard Wells, merchant of this city, a gentleman of probity and of great philoſophic knowledge, being at Elizabeth-town in company with Doctor Dayton, this gentleman told him he would ſhew him a curioſity as great perhaps as he had ever ſeen, namely, a living ſnake in a living horſe's eye. Mr. Wells then deſiring to ſee it, upon looking into the eye, diſcovered the animal very plainly, in a conſtant ſerpentine motion, but neceſſarily in a ſomewhat convoluted form, as its length was equal, as nearly as he could judge, to two diameters and an half of the eye, which could not meaſure leſs than between three and four inches. The head and tail, or if you pleaſe, the two extremities of the animal were then viſible, and the horſe's eye ſtill retained its tranſparency enough to admit ſeeing the whole of the ſnake diſtinctly.

The horſe was ſoon after purchaſed by a free negro, on purpoſe to bring to Philadelphia for ſhow, in order to gratify the curioſity of the virtuoſi of every claſs, by giving them an opportunity of ſeeing and contemplating ſo curious a phænomenon, and of communicating the reſult of their inquiries to the learned, for the information of the public at large.

At preſent, apparently from the briſk and almoſt conſtant motion of the animal, which is ſomewhat increaſed

in

in length, ſince the inſpection at Elizabeth-town, and which is as thick as a knitting needle, or piece of common twine, as nearly as can be determined through the intervening medium, the aqueous and vitreous humours of the eye are confounded (the fine cellular texture of the latter being broke down) and tinged with the ſofteſt part of the cryſtalline, ſo as to aſſume ſomewhat of a white milky appearance, bordering on the colour of a cataract. The Iris appears to be greatly dilated, or rather wholly deſtroyed. For the ſeptum, or partition which ſeparates the anterior from the poſterior chambers, in a ſound eye, muſt be broken down, as the animal, or, to ſpeak like a ſceptic, the animal appearance of a ſnake is continually receding into the fundus and back part, and by times coming forward into the anterior part of the eye, with a convoluted briſk motion. I cannot think a ſnake of the ſame ſize moving briſkly in a tumbler-ful of fair water, or of water diſcoloured with a tea-ſpoonful of milk, would be more viſible; but the coats of the eye and humours have now ſomewhat of a milky appearance, or colour of an incipient cataract.

It may be juſtly preſumed, that whatever might be the ſtate of viſion, at firſt appearance of this ſurpriſing phænomenon, that eye muſt be now blind. The lids are commonly cloſed, probably owing to pain excited in the eye by ſo troubleſome a gueſt; but there is no bloodſhot appearance on the cornea, though the ſurrounding parts, namely, the palpebræ, are a little tumid. To get a view of the eye, the keeper commonly ſtrikes the horſe on its back with an open hand, at which, as if frightened, it opens the lid of the left, as well as widens the opening of the right eye, which continues diſcloſed but a ſhort time; however this gives an opportunity for inſpection for five or ſix ſeconds of time together, and the blows muſt be repeated to keep the eye open, when a perſon wiſhes to have a longer time for inſpection.

The

The milky appearance has for ſome weeks grown gradually more opaque; from which circumſtance it is probable the diſeaſe occaſioned by the preſence of an extraneous body, or unnatural animal irritating the organ, will gradually produce too great obſcurity to afford that ſatisfaction in viewing it, which hitherto it has done and ſtill continues to afford.

It has been my wiſh, and I have expreſſed my opinion to ſeveral gentlemen that it would be worth while, to make up a ſum of money and purchaſe the horſe for ſake of diſſecting the eye, whilſt the animal is yet alive, but no notice has been yet taken of it: Perhaps the owner keeping it for ſhow places too high a value upon it. I have further deſired, if that purchaſe is not made, to have an opportunity of taking out the eye and diſſecting it immediately after death, whenever that event takes place, if it happens where I am.

The eye has been inſpected by ſeveral gentlemen of the faculty, who are aſtoniſhed, and at a loſs to account for the appearance on common principles or from known diſeaſes; a queſtion then naturally ariſes in the minds of moſt who have ſeen or heard of it, viz. If it be a real ſnake or other living animal, how it got there, or whether there are other inconteſtible hiſtories to match it, in the annals of medical hiſtory, of animals bred in man or other animals, as difficult to be accounted for?

I anſwer, Facts are what I am more concerned to eſtabliſh than ſpeculative opinions; therefore inſtead of leading to theories that may be idle and groundleſs, I ſhall be ſatisfied to refer all who doubt the reality of its being a ſnake in the eye, firſt to the hiſtory of the Guinea worm, of which I have had more than one caſe falling under my own care, and have ſeen others in the Pennſylvania hoſpital, extracted from the leg, ſeveral yards in length; ſecondly, to the well known hiſtory of a jointed worm bred in the liver of Mrs Holt, in this city, about

 thirty

thirty years ago, of about twenty inches long and near three in circumference, recorded in the medical essays of a society of physicians in London. This worm I have seen ten years after preserved in spirit, in the anatomical cabinet of the celebrated Dr. William Hunter of that place; and thirdly, I refer to the history and engraving of one exactly similar, as large as the life, inserted in the second volume of Edinburgh medical essays, plate fourth; and lastly, to autopsy, by examining the eye of the horse in question, which will afford ocular demonstration of the fact.

I shall add to these some observations of that prince of anatomists in his day, the famous Ruysch, who, as Dr. Haller attests, from a practice of dissection continued for near eighty years, with a diligence, skill and accuracy in examining into morbid bodies, and the niceness of his dissections and of his anatomical injections, exceeded all his cotemporaries; and in fine, whose testimony in those matters was looked upon by Boerhaave and Haller, and by every medical writer since, to be as incontestible authority as that of any other person whatsoever.

In his first volume, observation the 16th, he says, "daily experience proves that worms may be generated in all parts of the body. I strangled a dog that was very lively three hours after being fed, with a view to examine the lacteal or milk vessels. On opening the belly of it a live worm, at least two spans in length, skipped out. I could discover nothing amiss in the omentum, nor any solution of continuity of the parts; and both the mesentery and intestines were sound."

Again he says, observation 54, "I have had room to doubt whether, as Harvey and his followers affirm, all animals are produced out of an egg, from worms being found in the arteries of living horses; as also from worms seen in the parenchyma, or the glandular substance of the liver, as it is now called, and also in the cystic duct and biliary pores of sheep, and very often in the gall-bladder.

I remember

I remember once to have feen them in the human kidneys, and fuch as are more frequently met with in the kidneys of dogs. That worms have been fometimes found in the brain, no body can deny who will be at the trouble of turning over the writings of authors of high repute."

By what paffages thofe animals or their eggs were infinuated into the interior parts of the body, is not eafy to determine. It does not feem probable that they could reach the forementioned places through the pores of the fkin or the organs of refpiration; much lefs that their eggs were taken in at the mouth, and from thence proceeded to their refpective places; nor yet is it likely they could remain entire in the ftomach, where, in the procefs of chylification, there is a remarkable fermentation and breaking down the parts of the food. Nor can the chyliferous or lacteal veffels afford a paffage to the eggs in their rout; and laftly, no one alledges that he has ever feen exactly fimilar worms out of the body.

Were I fo difpofed, or did the defign of this paper require it, I could to thofe obfervations add many extraordinary inftances, of ftrange, rare, and furprizing productions of animals in the human body, from the works of the celebrated Bartholine, phyfician to the king of Denmark, and from other grave and learned authors of unexceptionable repute. But here I paufe, to reverence the hand that framed not only our bodies, but thofe of the meaneft reptiles, with an exuberance of fkill, which proclaims that they are not the effect of chance; and acknowledging I am loft in wonder, I leave the fuller explanation of the uncommon productions, to fome happy genius that may arife, if ever it fhould pleafe God to produce fuch an one into the world, who by tracing out the footfteps of the Creator, fhall be able to throw clearer lights than we yet have, upon thefe abftrufe fubjects.

N° XLIV.

Some Account of a motley coloured, or pye Negro Girl and Mulatto Boy, exhibited before the Society in the Month of May, 1784, *for their examination, by* Dr. JOHN MORGAN, *from the Hiſtory given of them by their owner Monſ. Le Vallois, Dentiſt of the King of France at Guadaloupe in the Weſt Indies, as follows.*

ADELAIDE, the little girl now before the ſociety, is aged two years and little more than one month, is of a clear black colour, verging to brown, except that ſhe has a white ſpot bearing ſome reſemblance to an aigrette; the point of which is at the root of the noſe, and it riſes into the hair, above the forehead, of which it occupies above an inch in width, from the margin to the fontenelle. In this part the colour of the hair is white, and it is curly like the hair of negroes in general, and thicker in that part than on any other part of its head. In the middle of its forehead and on the aigrette, is a large black ſpot; on the external ſide next to the temples, about one half of each eye-lid, both upper and under, is black, and the remaining half next to the noſe is white.

The eyes are black and lively, a little to the left and towards the middle of the chin a white ſpot begins, which is long in proportion to its breadth, but of leſs magnitude than that of the forehead: It ſtretches under the chin to the upper part of the throat. The neck, the upper and under part of the cheſt, the ſhoulders, the back, loins and buttocks to the junction with the thighs, and the pudendum, are of the colour of her face, but the loins and the thicker part of the buttocks are of a deeper black.

The

The arms from the upper and middle part are white, and interfperfed with black fpots. There are fome fmaller and more numerous about her knees than elfewhere.

Upon the large black fpots there are alfo many fmaller and blacker which are very glaring. Many of thefe fpots divide into four, five and fix rays, refembling a ftar, which are not obferved but by a clofe infpection, and then they are very vifible. In feveral parts thofe fpots, being of different fhades, give an exact picture of lunar eclipfes, as they are commonly reprefented in the books of aftronomy. The hands, the middle part of the fore arms, the inferior and middle parts of the legs and feet are black, which have a pretty ftriking refemblance to gloves and to bufkins.

The white that prevails over the breaft, and over the belly, arms and thighs, has a lively appearance. The fkin is foft, fmooth and fleek.

Adelaide has fine features; we meet with few negroes of fo beautiful a form. In her temper fhe is cheerful, gay and fportful, and as tall as children of her age generally are, and hath evidently a very delicate temperament, yet enjoys pretty good health, neither hath fhe eyes, nor ears, nor any particularity in her features, or external conformation, like what may be feen at the firft infpection in thofe who are called white negroes, whofe fkin is altogether of a dead white colour, and whofe woolly white hair and features refemble thofe of their negro parents.

From this detail we may remark that the alteration of the natural colour of Adelaide, takes place over the fame parts of the body, for the moft part, as over the body of Maria Sabina, of whom Monf. Buffon gives an account; and confidering it as a well authenticated fact, from all the information that has been received of Adelaide, that fhe had a negro father and negro mother, we are led to believe, that the Englifh account under the portrait of Maria Sabina is exact, and not afferted merely for the

fake

ſake of covering the honor of the mother, and of the ſociety in which ſhe was a ſlave.

The pyed mulatto boy is named Jean Pierre. He is a month younger than Adelaide; but from his figure, which is robuſt, he appears to be ſix months older. He as well as Adelaide both belong to Monſ. le Vallois. He was born at Grandterre, Guadaloupe, of a negro wench named Carolina and of a white man, an European, whoſe name I did not learn.

A certificate which Monſ. le Vallois has with him, legally authenticated by Monſ. Blin, lieutenant judge, given from under the hand of Monſ. des Eſſart, king's phyſician, and of Monſ. Cumin, king's ſurgeon, at Grandterre, Guadaloupe, atteſts that Adelaide was born at Gros-Iſlet in St. Lucia, that Bridget her mother is a negro of the Ibo nation, and now reckoned to be about twenty-five years old, and that her father, whoſe name is Raphael, is a negro of the Mina nation. In this certificate it is farther declared that the father of Jean Pierre has white ſpots (that is of a deeper white than his natural ſkin) of the ſame ſhape and in the ſame parts of the body as the ſon, and that the mother and one of the brothers of this boy's European father have like white ſpots, and in the ſame parts of the body.

However it may be in reſpect to thoſe obſervations concerning the ſuppoſed reſemblance of the white ſpots they may bear about them, to thoſe which mark Jean Pierre, it ſuffices to take notice here, that his body is entirely of the colour of a mulatto, except that he has from nature a white aigrette in his forehead like that of Adelaide. The hair in that part is white mixed with black, which is not ſo in Adelaide. The ſtomach, and the legs from two inches above the ancles to the middle of the calf of the legs are entirely of a beautiful lively white; there is alſo a white ſpot in the upper part of the penis. Over the white parts of of the legs there is a light white down, longer and thicker than children commonly have at this age.

Such is the natural hiſtory of thoſe two extraordinary children; but what cauſes have produced thoſe ſurpriſing phænomena and alteration of the natural colour of their ſkin, are left for others to inveſtigate and explain.

Monſ. le Vallois relates that the mother of Adelaide, whilſt pregnant with her, was delighted in laying out all night in the open air, and contemplating the ſtars and planets, and that the great grandmother of Jean Pierre (a white lady) during the time of her being with child of her daughter, his grandmother by the father's ſide, was frightened on having ſome milk ſpilled upon her. Whether this will account for her daughter and grandchildren being marked in the manner related, and for the ſpots obſerved on the mulatto boy deſcending to him; or whether the ſtrong impreſſion made upon the mother of Adelaide, by the nightly view of the ſtars and planetary ſyſtem, may be conſidered as the cauſe of the very extraordinary appearances in that girl, every one will determine for themſelves; there being many who diſpute childrens being ever marked by the fears, longings, or impreſſions made by mothers on the bodies of their children, at a certain time of pregnancy; for which they endeavour to account in different ways; whilſt others who have known a variety of children born with different marks on them, (which have fallen under their particular notice) are equally confident of thoſe marks proceeding from the cauſes alledged.

Extract

N° XLV.

Extract of a Letter from BERNARD ROMANS, *of Pensacola, dated August* 20, 1773.

THE common mariners compass has always appeared to accurate observers as an imperfect instrument, but in nothing has it proved to be more defective than in its use in storms, the heaviest brass compasses now in use are by no means to be relied on in a hollow or high sea. This is owing to the box hanging in two brass rings confining it to only two motions, both vertical, and at right angles with each other, by which confinement of the box upon any succussion, more especially sudden ones, the card is always put into too much agitation, and before it can well recover itself, another jerk again prevents its pointing to the pole, nor is it an extraordinary thing to see the card unshipped by the violence of the ship's pitching.

All these inconveniencies are remedied to the full by giving the box a vertical motion at every degree and minute of the circle, and to compound these motions with a horizontal one, of the box, as well as of the card. By this unconfined disposition of the box the effects of the jerks on the card are avoided, and it will always very steadily point to the pole. Experience has taught me, that the card not only is not in the smallest degree affected by the hollow sea, but even in all the violent shocks and whirlings the box can receive, the card lies as still as if in a room unaffected by the least motion.

Lately a compass was invented and made in Holland, which has all these motions. It is of the size of the common brass compasses, the bottom of the brass box instead of being like a bowl, must be raised into a hollow cone, like

like the bottom of a common glaſs bottle; the vertex of the cone muſt be raiſed ſo high as to leave but one inch between the card and the glaſs; the box muſt be of the ordinary depth, and a quantity of lead muſt be poured in the bottom of the box round the baſe of the cone, this ſecures it on the ſtyle whereon it traverſes.

This ſtyle is firmly fixed in the center of a ſquare wooden box, like the common compaſs, except that it requires a thicker bottom. The ſtyle muſt be of braſs about ſix inches long, round and of the thickneſs of one-third of an inch, its head blunt, like the head of a ſewing thimble but of a good poliſh; the ſtyle muſt ſtand perpendicular, the inner vertex of the cone muſt alſo be well poliſhed; the vertical part of the cone ought to be thick enough to admit of a well poliſhed cavity ſufficient to admit a ſhort ſtyle proceeding from the center of the card whereon it traverſes. The compaſs I ſaw was ſo conſtructed, but I ſee no reaſon why the ſtyle might not proceed from the center of the vertex of the cone, and ſo be received by the card the common way. The needle muſt be a magnetic bar blunt at each end; the glaſs and cover is put on in the common way.

A compaſs of this kind was given by the captain of a Dutch man of war to Capt. Burnaby of the Zephyr ſloop; this gentleman gave it to me to examine, and was very profuſe in his encomiums thereon, ſaying that in a very hard gale, which laſted ſome days, there was not a compaſs but it of any ſervice at all. Indeed to me it appears to deſerve all the praiſe he gave it. My ſtay is ſo ſhort here, as not to allow me time to have one made; but I intend to have one made for my own uſe, and ſhall offer it to the ſociety for inſpection. I hope that this uſeful inſtrument may become univerſal, as navigation certainly will be rendered more ſafe through its means; and I ſhall think myſelf highly honoured, if through the channel of this ſociety it becomes public.

 Preſents

Presents made to the American Philosophical Society, since its Revival and Incorporation in 1780, with the Names of the Donors.

DONORS.	PRESENTS.
1780, Apr. 7. Dr. *Coste*,	An analytic, etymologic, and argumentative treatise on the accent and pronunciation of the English language.
Dec. 15. Monf. *Chatilaux*,	His works de la felicité publique, 2 vol.
1781, Jan. 19. Monf. de *Marbois*,	A pamphlet in French, containing an account of, and proposals for printing a work of great merit, entitled voyages metallurgiques, &c. Par M. Jars.
Feb. 16. Gen. *Sullivan*,	Specimens of talc, from a large body of this fossil, lately discovered in New-Hampshire.
Mar. 16. *Sam. Adams*, Esq.	Copy of a Philosophical discourse delivered before the academy of arts and sciences at Boston, by their president James Bowdoin, Esq.
1783, Feb. 16. Assembly of the State of *Pennsylvania*,	One hundred and fifty pounds.
May 2. Mr. *J. M'Henry*,	A curious specimen of petrified pine.

Sept.

DONATIONS.

1783, Donors.	Presents.
Sept. 26. Dr. *Franklin*,	Natural hiſtory of the South of France, by Monſ. L'Abbé Soulavie, 2 vols.
Oct. 17. Mr. *John Hyacinth de Magellan*,	A collection of Philoſophical tracts, &c.
Mr. *Godfrey*,	A collection of prints, repreſenting ſome of the principal events during the late revolution in America.
Abbé de *Fontana*,	Four pamphlets on Philoſophical ſubjects, written by himſelf.
Dec. 5. Mr. *Van Berckel*,	Six volumes of the Tranſactions of the Batavian Philoſophical Society of Rotterdam.
Monſ. de *Etienne*,	On a cement impenetrable by water.
1784, Jan. 16. Mr. *John Felſted*,	A collection of ſpecimens of the ſeveral woods growing in the iſland of Jamaica.
Febru. 3. Mr. *Warder*,	A collection of animal calculi.
Mr. *Mandrillon*,	Voyageur Americain. An anſwer to the queſtion propoſed by the Academy of Sciences at Lyons, viz. Whether the diſcovery of America has been advantageous to mankind or not ? both written by himſelf.

Feb.

DONATIONS.

1784. Donors.	Presents.
Feb. 20. *Samuel Vaughan*, Esquire,	A $3\frac{1}{2}$ feet acromatic telescope, completely mounted on a mahogany stand, with rack work to move it along the meridian, or parallel to the equator, made by Mr. Dolland.
	A Gunter's scale improved, for navigation and astronomy, by Mr. John Robertson, with a description thereof by Mr. Mountaine.
	A magazine microscope, consisting of a single, double, and solar microscope.
Mr. *William Parker* by Mr. *S. Vaughan*,	A burning lens $11\frac{1}{2}$ inches, with a second lens of $6\frac{1}{2}$ inches diameter, completely framed and mounted on a mahogany turned cone, with rack work, pillar, pinnion and apparatus for placing the whole in a proper situation.
March 19. Dr. *Franklin*,	A treatise in French, on the subject of air balloons.
Samuel Vaughan, Esq.	A copy in manuscript of Dr. Priestley's experiments on phlogiston, and the seeming conversion of water into air.
Apr. 2. Mr. *J. Vaughan*,	Dr. Priestley's works, complete in 33 volumes.
	Dr. Priestley's biographical and historical charts.

April

1784. Donors.	Presents.
April 9. *Samuel Vaughan*, Esq.	Dr. Price on reverſionary payments, annuities, &c. 2 vols. Tranſactions of the ſociety of arts, &c. 1783, 1784. Tracts publiſhed by the ſociety for conſtitutional information, 1783. Proceedings at Quebec, 1773. Lind on diſeaſes incident to hot climates. Plinii ſecundi hiſt. nat. Antiquitates Romanæ. Rowe's fluxions. Gregory's practical geometry. Hamilton on vapours and the aurora borealis.
April 16. Dr. *Coſte*,	Eight copies of his inaugural oration, delivered in the college of William & Mary, Virginia.
Oct. 15. Rev. *Jeremy Belknap*,	The firſt volume of his hiſtory of New-Hampſhire.
1784, Nov. 12. Dr. *M. Guthe*,	A ſilver medal in memory of the Rev. Chriſtian Meyer of Heidelburg, late a member of this Society.
——19. Chev. d' *Armours*,	A pamphlet in manuſcript, on the ſubject of animal magnetiſm, by Dr. P. Hervier, of the Sorbonne.
Dec. 9. Monſ. de *Marbois*,	Report of the commiſſioners appointed by the king of France, to examine into the merit of Dr.

DONATIONS.

Donors.	Presents.
	Dr. Mesmer's late discoveries of animal magnetism.
1785, Jan. 22. Mr. *Mandrillon*,	An elegant copy of his work entitled le spectateur Americain.
Mar. 4. Mr. *W. Henry*,	The model of a wheel carriage, to run against the wind by the force of the wind alone.
	Two very large pieces of chrystal found in Lancaster county.
	An exceedingly large tusk and one of the grinders of some unknown species of animals, brought from the neighbourhood of the Ohio.
Assembly of the State of *Pennsylvania*,	A lot of ground in the State-house square, on which to erect a building for the accommodation of the Society.
Mrs. *Pauli*,	A horn of the Canada moose deer of an extraordinary size.
July 15. Rev. Mr. *Muhlenberg*,	A manuscript copy of his *Flora Lancastrienis*.
Dr. *Noel*,	Three volumes of the proceedings of the Royal French Academy.
	An elegant electrical machine, with the necessary apparatus.
Sept. 27. Dr. *T. Redman*,	Specimens of the East-India manner of writing, on long strips of the leaves of the cocoa nut tree.

Monf.

D O N A T I O N S.

1785. Donors.	Presents.
Sept. 27. Monſ. *C. L. d'Heritier*,	A collection of his botanical prints with their deſcription, in Latin.
Decem. 2. Dr. *Hutton*,	His large tables of logarithms, &c. Three pamphlets, containing the hiſtory of the late debates in the Royal Society.
Mr. *Sam. Vaughan*, jun.	Four pamphlets in the German language, containing new diſcoveries in chemiſtry.
Mr. *Ed. Huſſey Delaval*,	His enquiry into the cauſe of the permanent colours of opaque bodies, 8vo.
Arthur Young, Eſq.	His annals of agriculture.
Mr. *Warvill*,	His tableau des ſciences & arts.
Mr. *J. H. de Magellan*,	Botanica characteriſtica, by Dr. Wolf of Dantzick. A remonſtrance againſt Voltaire, by Mr. Needham. Lettre du Pekin, on the Chineſe language. Elements of mineralogy, by Richard Kerwin, Eſq. Medical virtues of digitalis purpurea, or fox-glove, by Dr. Withering of Mancheſter. Twelve copies of his pamphlet on the glaſs apparatus and eudiometer, third edition. Propoſals of Mr. Taylor, for printing a table of logarithms and of ſines, tangents, &c. to every ſecond of the quadrant.

Mr.

DONATIONS.

1785. Donors.	Presents.
Sept. 27. Mr. *J. H. de Magellan*,	Count Brahl's account of the going of Mr. Emery's new chronometer for the pocket, which ſcarce differs two ſeconds from mean time in 24 hours. Price 90 guineas, in a ſilver caſe.
	An account of a new hydroſtatic balance, by Mr. Nicholſon.
Samuel Vaughan, Eſq.	Deſcriptions of uſeful machines and models of the ſociety (in London) for the encouragement of arts, manufactures and commerce, illuſtrated with copper plate deſigns, 2 vols. folio, laſt edition.
	Memoirs of the American academy of arts and ſciences, 4° 1785.
Dec. 16. Mr. *Cha. W. Peale*,	An elegant portrait of Doctor Franklin.
1786, Jan. 20. Mr. *H. Marſhall*,	His book, entitled, an alphabetical arrangement of the foreſt trees, &c. of North-America.
June. Dr. *Ed. Waring*, by Mr. *S. Vaughan*,	Meditationes algebraicæ.
	Miſcellanea analytica, de æquationibus algebraicis, et curvarum proprietatibus.
	Meditationes analyticæ.
	Proprietates algebraicum curvarum.—All written by himſelf.

D O N A T I O N S.

The following Donations were received through the hands of Dr. Franklin.

1786. Donors.	Presents.
June. Monſ. the Count de *Buffon*,	His collection of birds, beautifully engraved and coloured. Supplement à l'hiſtoire naturelle, 5 tome, 4°.
Dr. *Fothergill*,	Illuſtratio ſyſtematis ſexualis Linneæ, per Johannes Miller.
Lord *Stanhope*,	Roberti Simſon, M. D. opera reliqua. 4°.
Signior *Manini*,	Le lettere Americane, 3 vols. 12°.
Monſ. *Grivel*,	L'iſle inconnue, ou memoires du Chev. des Gaſtines, 4 vols. 12°.
Mr. *J. H. de Magellan*,	His collection, de differens traités ſur des inſtruments d'aſtronomie, phiſique, &c. Extrait d'une lettre de M. Magellan ſur une pendule de ſon invention. Deſcription d'une machine nouvelle de dynamique, &c. Nouvelle conſtruction d'alambic pour faire toute ſorte de diſtillation en grande, avec le plus d'economie dans l'operation, et le plus d'avantage dans le reſultat.
M. Court de *Gebelin*,	Monde primitif comparé avec le monde moderne, 1 vol. 4°.
The Author,	Le triomphe du nouveau monde, reponſes academiques, formant

1786. Donors.	Presents.
	mant un nouveau ſyſtême de confederation, fondé ſur les beſoins actuels des nations chrétiens-commercantes, et adapté à leurs diverſes formes de gouvernement, 2 vols. 8°.
June. M. *Elie de Beaumont*,	Memoire pour Dame Marie Reine Petit de la Burthe, Marquiſe d'Anglure, contre le Sieur Pierre Petit.
Pere Berthier,	Hiſtoire de premiere temps du monde, 8vo.
Monſ. *Meſmer*,	Précis hiſtorique de faits relatifs au magnetiſme animal, 1 vol. 8vo. ſix copies.
Monſ. L'Abbé *Sans*,	Gueriſon de la paralyſie par l'electricité, 8°.
Dr. *Ingenhauſs*,	Ingen-Hauſz vermiſchte ſchriften, 1784, 2 vols. 8vo. Obſervations ſur la conſtruction et l'uſage de l'eudiometre du M. Fontana,—both written by himſelf.
Marcus Lemort Demetigny,	Tentamen ΨΥΧΟ-ΣΩΜΑΤΟ-ΙΑΤΡΙΚΟΝ ſeu conſpectus theſiformis de naturâ animæ et corporis, &c. written by himſelf..
Monſ. *Gerbier*,	Lettres et obſervations de M. Gerbier, docteur en medicine, l'un des médicins de monſieur ſervant par quartier, au ſujét de deux nouveaux remedes, contre les maladies ſquirrheuſes, cancereuſes, &c.

Pere

DONATIONS.

1786.	Donors.	Presents.
June.	*Pere Beccaria*,	Della electricita terreftre atmofferica a cielo fereno, offervazioni di Giambatifta Beccaria.

Sundry other books, given by different perfons, will be mentioned in the lift to be publifhed in the next volume.

END OF THE SECOND VOLUME.