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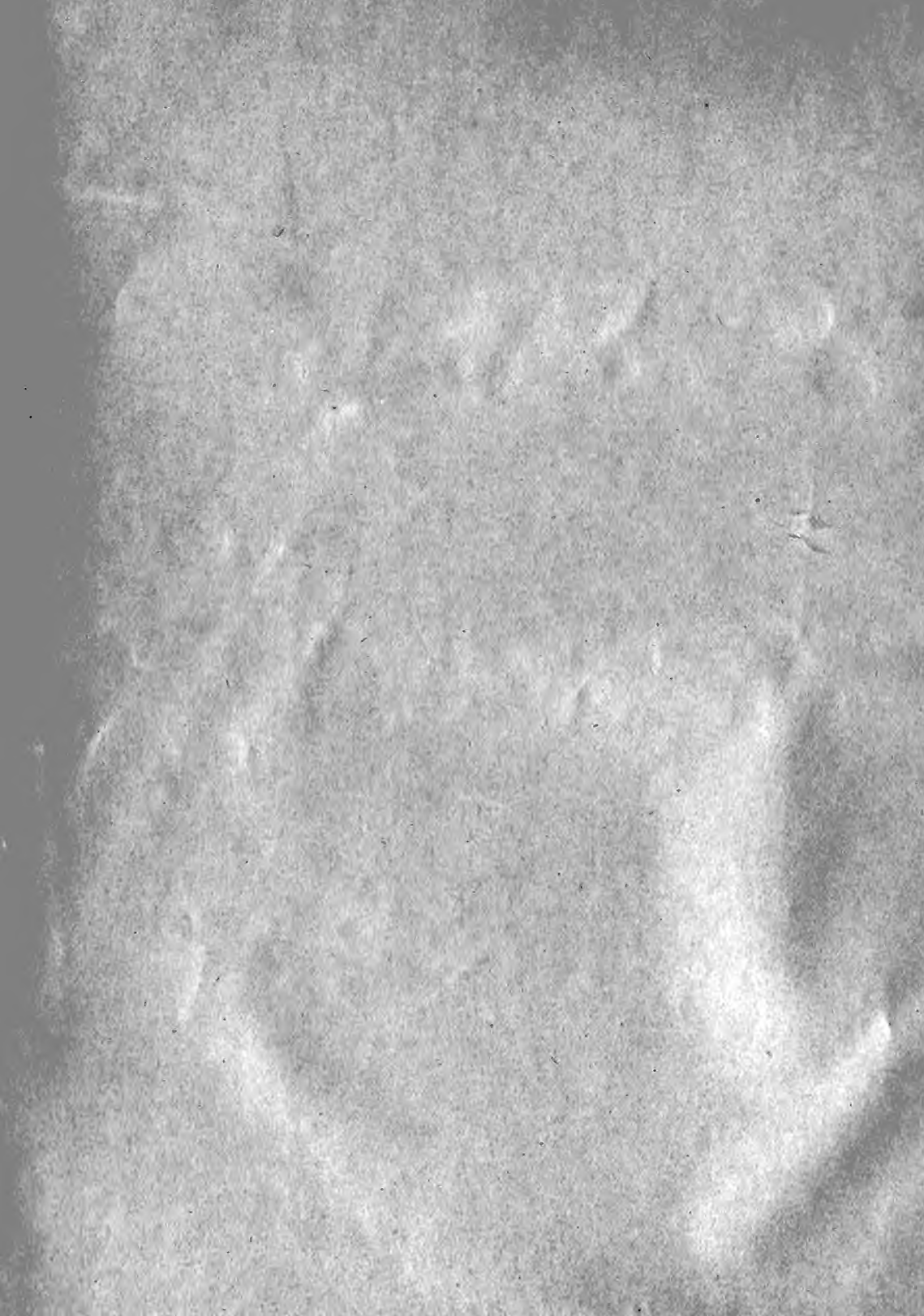
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# TRANSACTIONS

OF THE

AMERICAN

PHILOSOPHICAL SOCIETY,

HELD AT

PHILADELPHIA,

FOR PROMOTING

*USEFUL KNOWLEDGE.*

VOLUME II.

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PHILADELPHIA:

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

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M.DCC.LXXXVI.

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AND THE PROGRESS  
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## ADVERTISEMENT.

**T**HE 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 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 Committee'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 answer, 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."

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 LAWS AND REGULATIONS,

OF THE

*American* PHILOSOPHICAL SOCIETYHELD AT *PHILADELPHIA*, FOR PROMOTING*USEFUL KNOWLEDGE.*

**T**WO 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 purposes

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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* admiffion moneey, and also subscribe for the yearly payment of *Ten Shillings*, before he is intitled to have any vote in the bufiness 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 business of the *Curators* shall be to take charge of, and preserve, all *Specimens of natural Productions*, whether of the *Animal, Vegetable* or *Fossil* kingdom; all models of machines and instruments, and all other matters and things belonging to the Society, which shall be committed to them; to class and arrange them in their proper order, and keep an exact list of them, with the names of the respective donors, in a book provided for that purpose; which book shall be laid before the Society, as often as called for.

The Curators, on entering upon their office, shall give such a receipt for every thing that is committed to their charge, as the Society shall think proper; and, at the end of their term, shall deliver up the same to their successors. For the faithful performance of their duty, and of the trust reposed in them, they shall give bond to the Presidents and Vice-Presidents, in such a sum as they, or any three of them, shall require.

IX. *Of the Meetings of the Society.*

The ordinary meetings of the Society shall be on the first and third Fridays of every month, from October to May, both inclusive, at six o'clock in the evening, and on the third Friday in each of the other four months at seven o'clock.

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

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

No part of the Society's stock shall be disposed of in *Premiums*, or otherwise, nor shall any new laws be made,

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.



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

**W**HEREAS 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 labours

hours 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,

ident, three Vice-Presidents, four Secretaries, three Curators, one Treasurer, together with a Council of twelve Members; and that on the first Friday of January next, between the hours of two and five in the afternoon, as many of the members of the said Society as shall have paid up their arrears due to the Society, and shall declare their willingness to conform to the laws, regulations and ordinances of the Society, then duly in force, according to the tenor hereof, by subscribing the same, and who shall attend in the hall, or place of meeting of the said Society, within the time aforesaid, shall choose by ballot, agreeably to the fundamental laws and regulations herein before referred to, one President, four Secretaries, three Curators, and one Treasurer, and at the same time and place, the Members met and qualified as aforesaid, shall in like manner choose 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 first Friday in January, which shall be in the year of our Lord one thousand seven hundred and eighty-two, and so likewise on the first Friday of January, yearly and every year thereafter, between the hours of two and five in the afternoon, the Members of the said Society met and qualified as aforesaid, shall choose one President, three Vice-Presidents, four Secretaries, three Curators and one Treasurer, to hold their respective offices for one year; and four Councilmen, to hold their offices for three years. *Provided*, That no person residing within the United States shall be capable of being President, Vice-President, Secretary, Treasurer, or Member of the Council, or of electing to any of the said offices, who is not capable of electing and being elected to civil offices within the state in which he resides. *Provided also*, That nothing herein contained, shall be considered as intended to exclude any of the said officers or counsellors, whose times shall  
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)

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*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.*

**W**HEREAS 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.

xx EXTRACTS FROM THE MINUTES OF THE

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 discovery, invention or improvement which shall be found to have the greatest number of concurring votes (being not less than seven) in its favour, shall be successful. Whereupon a certificate in writing shall be forthwith drawn of this adjudication, and signed by those who voted for the crowned subject: And then, *and not till then*, the sealed letter accompanying the crowned performance, shall be opened and the name of the author announced; which certificate shall be presented to the Society at their next stated meeting, and delivered to the Secretary to be entered on record, in a bound book provided for this purpose.

8. A full account of the crowned subject shall be published by the Society as soon as may be, after the adjudication, either in a separate publication, or in the next succeeding volume of their Transactions, or in both.

9. The unsuccessful performances shall lie over for consideration, and remain, as candidates for the premium, for *five* succeeding years next after their presentment; unless the author or authors shall think fit to withdraw them or any of them: And the Society shall publish annually an abstract of the titles, object or subject matter of the communications so under consideration, such only excepted as the counsellors and other officers shall, by vote as aforesaid, have determined not worthy of public notice.

10. No counsellor or officer who is a candidate shall sit in judgment, or give his vote.

11. The letters containing the names of authors whose performances shall be rejected, or shall be found unsuccessful after a trial of five years, shall be burnt without breaking the seals.

12. In case there should be a failure, in any year, of any communication worthy of the proposed premium, there will then be two premiums awarded in the next year. But no accumulation of premiums shall entitle an author to more than one premium for any one discovery, 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

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LIST OF THE OFFICERS

OF THE

*AMERICAN PHILOSOPHICAL SOCIETY,*

Held at PHILADELPHIA, for promoting useful Knowledge,

For the Year 1786.

PATRON. His Ex<sup>ty</sup> 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 Patterfon, Professor of  
Mathematics in the University  
of Pennsylvania.  
Rev. Dr. Samuel Magaw, Vice-Pro-  
vost of the University of Penn-  
sylvania.  
Dr. John Foulk.

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

TREASURER. { The Honorable Francis Hopkin-  
son, 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.

**J**OSEPH Astolinge, Esq. of Georgia.

A

B

Mr. Gerard Banker.

Dr. William Bryant, of New-Jersey. \*

Mr. James Bringham.

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 Carfon.

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 Griffiths.

## 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 Matrefon, 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

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# C O N T E N T S

O F

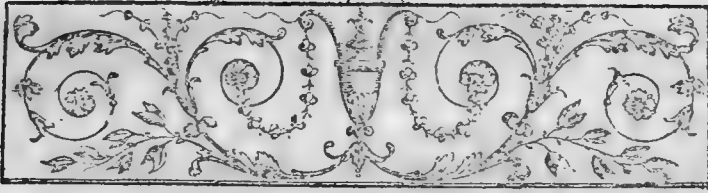
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# TRANSACTIONS

O F T H E

*American* PHILOSOPHICAL SOCIETY, &c.

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N<sup>o</sup> 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.

**I**N 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. That

Plate I.  
Figure 1.

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



discharges a part of it into the room, offending the eyes and damaging the furniture?

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

1. *Smoky chimneys in a new house, are such, frequently from mere want of air.* The workmanship of the rooms being all good, and just out of the workman's hand, the joints of the boards of the flooring, and of the pannels of wainscoting are all true and tight, the more so as the walls, perhaps not yet thoroughly dry, preserve a dampness in the air of the room which keeps the wood-work swelled and close. The doors and the sashes too, being worked with truth, shut with exactness, so that the room is as tight as a snuff-box, no passage being left open for air to enter, except the key-hole, and even that is sometimes covered by a little dropping shutter. Now if smoke cannot rise but as connected with rarefied air, and a column of such air, suppose it filling the funnel, cannot rise, unless other air be admitted to supply its place; and if, therefore, no current of air enter the opening of the chimney, there is nothing to prevent the smoke coming out into the room. If the motion upwards of the air in a chimney that is freely supplied, be observed by the rising of the smoke or a feather in it, and it be considered that in the time such feather takes in rising from the fire to the top of the chimney, a column of air equal to the content of the funnel must be discharged, and an equal quantity supplied from the room below, it will appear absolutely impossible that this operation should go on if the tight room is kept shut; for were there any force capable of drawing constantly so much air out of it, it must soon be exhausted like the receiver of an air pump, and no animal could live in it. Those therefore who stop every crevice in a room to prevent the admission of fresh air, and yet would have their chimney carry up the smoke, require inconsistencies,  
and

and expect impossibilities. Yet under this situation, I have seen the owner of a new house, in despair, and ready to sell it for much less than it cost, conceiving it uninhabitable, because not a chimney in any one of its rooms would carry off the smoke, unless a door or window were left open. Much expence has also been made, to alter and amend new chimneys which had really no fault; in one house particularly that I knew, of a nobleman in Westminster, that expence amounted to no less than three hundred pounds, *after* his house had been, as he thought, finished and all charges paid. And after all, several of the alterations were ineffectual, for want of understanding the true principles.

*Remedies.* When you find on trial, that opening the door or a window, enables the chimney to carry up all the smoke, you may be sure that want of air *from without*, was the cause of its smoking. I say *from without*, to guard you against a common mistake of those who may tell you, the room is large, contains abundance of air, sufficient to supply any chimney, and therefore it cannot be that the chimney wants air. These reasoners are ignorant, that the largeness of a room, if tight, is in this case of small importance, since it cannot part with a chimney full of its air without occasioning so much vacuum; which it requires a great force to effect, and could not be borne if effected.

It appearing plainly, then, that some of the outward air must be admitted, the question will be, how much is *absolutely necessary*; 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 discover this quantity, shut the door gradually while a middling fire is burning, till you find that, before it is quite shut, the smoke begins to come out into the room, then open it a little till you perceive the smoke comes out no longer. There hold the door, and observe 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 fireplace, 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 ad-

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 staircase 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<sup>o</sup> I.

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 cordwood, 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 re-

quire 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<sup>o</sup> 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-dases* 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 fireplace; 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<sup>o</sup> II.

warmer air which enters their tops being cooled in its turn, and made heavier, continues to descend; and this operation goes on, till the funnels are warmed by the passing of warm air through them, or the air itself 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 sufficient to produce these currents, the air will begin to ascend 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 hesitate; and as the heat of the day approaches, it sets downwards, and continues so till towards evening, when it again hesitates for some time, and then goes upwards constantly during the night, as before mentioned. Now when smoke issuing from the tops of neighbouring funnels passes over the tops of funnels which are at the time drawing downwards, as they often are in the middle part of the day, such smoke is of necessity drawn into these funnels, and descends with the air into the chamber.

The *Remedy* is to have a sliding plate, hereafter described\*, that will shut perfectly the offending funnel.

9. Chimneys which generally draw well, do nevertheless sometimes give smoke into the rooms, *it being driven down by strong winds passing over the tops of their funnels*, though not descending from any commanding eminence. This case is most frequent where the funnel is short, and the opening turned from the wind. It is the more grievous, when it happens to be a cold wind that produces the effect, because when you most want your fire, you are sometimes obliged to extinguish it. To understand this, it may be considered that the rising light air, to obtain a free issue from the funnel, must push out of its way or oblige the air that is over it to rise. In a time of calm or of little wind this is done visibly, for we see the smoke that is brought up by that air rise in a column above the chimney. But when a violent current of air, that is, a

C

strong

\* See Appendix, N<sup>o</sup> 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.

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 London, had of late taken to the use of pitcoal for their fires, instead of wood, which filled the air with noxious vapours and smoke, very prejudicial to the health, particularly of persons coming out of the country; and therefore 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<sup>o</sup> 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 diseases

eases, the moisture being pure, unmixed with the poisonous vapours arising from putrid marshes and stagnant pools, in which many insects die and corrupt the water. These places only, in my opinion, (which however I submit to yours) afford unwholesome 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 such air pernicious to those who breathe it. And I imagine it a cause of the same kind that renders the air in close rooms, where the perspirable matter is breathed over and over again by a number of assembled people, so hurtful to health. After being in such a situation, many find themselves affected by that *febricula*, which the English 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 subject, and go out of my depth. So I return again to my chimneys.

We have of late many lecturers in experimental philosophy. I have wished that some of them would study this branch of that science, and give experiments in it as a part of their lectures. The addition to their present apparatus need not be very expensive. A number of little representations of rooms composed each of five panes of sash glass, framed in wood at the corners, with proportionable doors, and moveable glass chimneys, with openings of different sizes, and different lengths of funnel, and some of the rooms so contrived as to communicate on occasion with others, so as to form different combinations, and exemplify different cases; with quantities of green wax taper cut into pieces of an inch and half, sixteen of which stuck together in a square, and lit, would make a strong fire for a little glass chimney, and blown out would continue to burn and give smoke as long as desired. With such an apparatus all the operations of smoke and rarefied air in rooms and chimneys might be seen through their transparent

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 twigs 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

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 3.

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

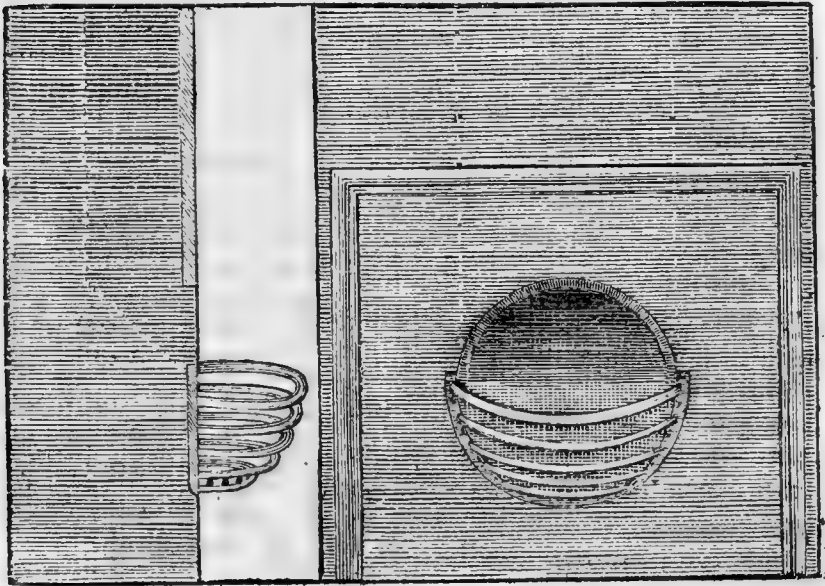
Much

\* See Appendix, N<sup>o</sup> II.

## STAFFORDSHIRE FIRE-PLACE.

SIDE VIEW.

FRONT VIEW.



Much more of the prosperity of a winter country depends on the plenty and cheapness of fuel, than is generally imagined. In travelling I have observed, that in those parts where the inhabitants can have neither wood nor coal nor turff but at excessive prices, the working people live in miserable 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 furnished with necessaries, and have decent habitations. The obvious reason is, that the working hours of such people are the profitable hours, and they who cannot afford sufficient fuel have fewer such hours in the twenty four, than those who have it cheap and plenty: For much of the domestic work of poor women, such as spinning,

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.

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# A P P E N D I X.

NOTES FOR THE LETTER UPON CHIMNEYS.

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## 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 less fire. It is this. The opening of the chimney is contracted, by brick-work faced with marble slabs, to about two feet between the jams, and the breast brought down to within about three feet of the hearth.—An iron frame is placed just under the breast, and extending quite to the back of the chimney, so that a plate of the same metal may slide horizontally backwards and forwards in the grooves on each side of the frame. This plate is just so large as to fill the whole space, and shut the chimney entirely when thrust quite in, which is convenient when there is no fire. Drawing it out, so as to leave a space between its further edge and the back, of about two inches; this space is sufficient for the smoke to pass; and so large a part of the funnel being stopt by the rest of the plate, the passage of warm air out of the room, up the chimney, is obstructed and retarded, and by that means much cold air is prevented from coming in through crevices, to supply its place. This effect is made manifest three ways. First, when the fire burns briskly in cold weather, the howling or whistling noise made by the wind, as it enters the room through the crevices, when the chimney is open as usual, ceases as soon as the plate is slid in to its proper distance. Secondly, opening the door of the room about half an inch, and holding your hand against the opening, near the top of the door, you feel the cold air coming in against your hand, but weakly, if the plate be in. Let another person suddenly draw it out, so as to let the air of the room go up the chimney, with its usual freedom where chimneys are open, and you immediately feel the cold air rushing in strongly. Thirdly, if something be set against the door, just sufficient, when the plate is in, to keep the door nearly shut, by resisting the pressure of the air that would force it open: Then, when the plate is drawn out, the door will be forced open by the increased pressure of the outward cold air endeavouring to get in to supply 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. wrap 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.

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 11.

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.

**C**HIMNEYS 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 sheltered

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.

Blackening 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 hor-

Plate I.  
Figure 13.

zontal 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.

*Explanation*



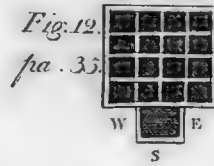
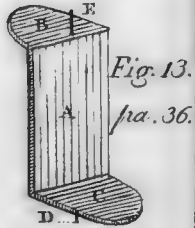
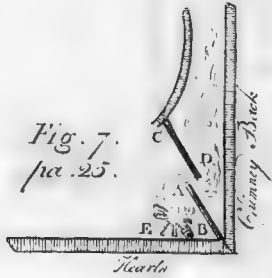
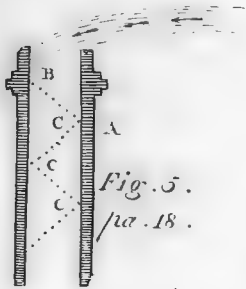
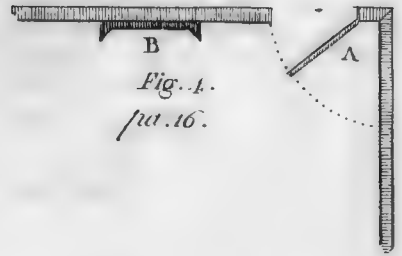
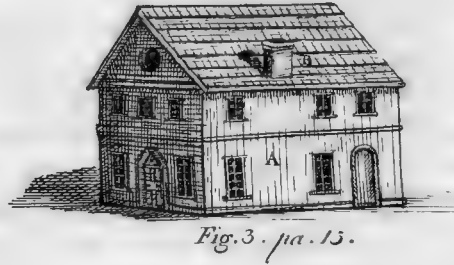
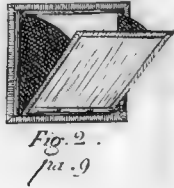
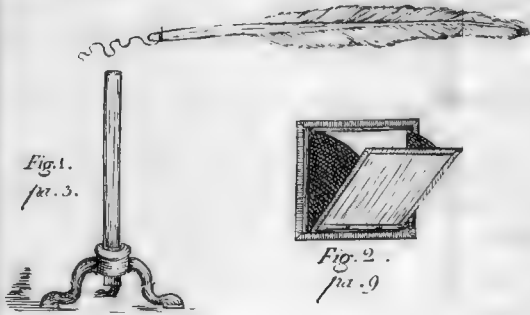
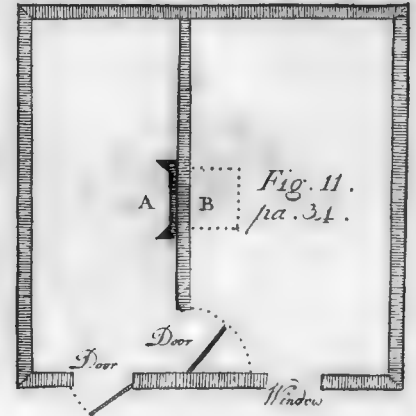
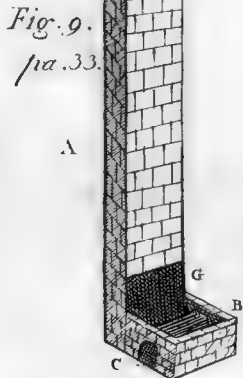
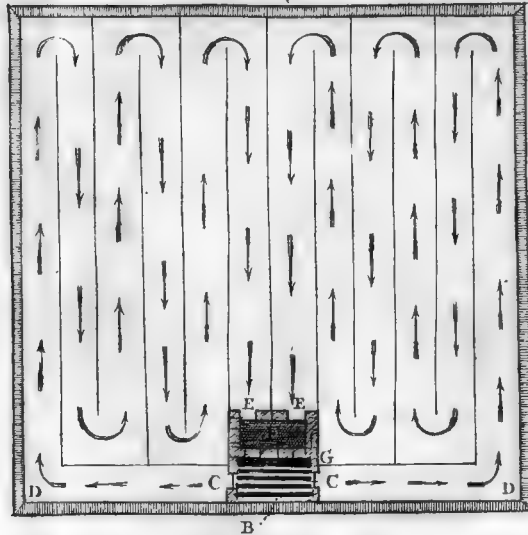


Fig. 10. / pa. 33.



J. S. Couper sculpt.

1871

STATE OF NEW YORK

IN SENATE

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*Explanation of an Optical Deception.*

BY D. RITTENHOUSE.

Read March  
3, 1780.

**S**OME 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 case; for those parts which are farthest from the eye in the object, will always be farthest from the eye in the image, and often in a much greater proportion. After some time I concluded it to be a necessary consequence of the apparent inversion of the object; and many things tended to confirm me in this opinion, before I made the experiments which seem perfectly decisive.

It has often been matter of surprize to me, when viewing the moon through a good telescope, in company with persons not accustomed to such observations, that whilst the cavities and eminences of the moon's surface appeared to me marked out with the utmost certainty by their light and shades, my companions generally conceived it to be a plain surface of various degrees of brightness. The reason I suppose to be this; the astronomer knows from the moon's situation with respect to the sun, and even from the figure of its enlightened part, precisely in what direction the light falls on its surface, and therefore judges rightly of its hills and vallies, from their different degrees of light, according to those rules which are imperceptibly formed in the mind, and confirmed by long experience. But a person unacquainted with astronomy knows nothing of the direction of the sun's light on the moon, nor does he attend to the moon's globular figure, and is besides, perhaps, possessed with a notion of its being self-luminous; no wonder then that the same object has a very different effect on his imagination. It seems to be those rules of judging, which we begin to form in our earliest infancy, which we set aside, re-establish, alter, correct and confirm, and at length rely on with the utmost confidence, even without knowing that we do so, or that we have any such rules: It is these rules, of such infinite general use to us, that sometimes mislead us on new and extraordinary occasions, and particularly in the case now before us. A person entering into a room perceives, at a single glance, whence the light comes which illuminates the objects before

fore him ; and that without remaining conscious for a moment that he has attended to this circumstance : But the effect remains, and will influence his judgment. If on looking at a brick hearth he perceives that those lines which divide the bricks have a dark shade on that side opposite to the light, and a bright streak on the contrary side next to the light, he must at the same time perceive that they have the property which he has constantly observed in ridges, not in furrows. And since the appearance of the hearth will be such, through the glasses, in consequence of their inverting the situation of its several parts, with respect to the light, the observer will instantly pronounce the chinks between the bricks, and every little cavity in them, to be so many perfect elevations above the common surface, nor can any effort of the mind correct the imagination or alter the appearance.

Though I was well satisfied of the truth of this explanation, I resolved nevertheless to bring it to the test of experiment, which I did in the following manner.

In order to give my experiment fair play, I shut all the windows of my chamber excepting one directly opposite to the chimney. I then took the tube, with two convex glasses, and looking through it at the hearth, all the bricks appeared depressed and the clefts between them elevated, as usual. I then placed a looking-glass against the chimney back, so that it reflected the light from the window upon the hearth, and set up a small board before the hearth to intercept the direct light of the window from it. Then looking at the hearth through the glasses, I was much pleased to find it appear in its natural state, with the bricks elevated. I then sat down on a chair at the edge of the hearth, and looking through the tube which I held to my eye with one hand, whilst with the other I moved the board so as to make it sometimes intercept the direct light of the window, and at other times the reflected light of the looking-glass, I constantly found that when the hearth

was:

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

I then considered that since the hearth appeared in its natural state by reflected light, and in its unnatural state by direct light, only in consequence of the inverting property of the glasses, 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 glasses, and looking through the open tube, that the hearth appeared as perfectly, and as constantly in its unnatural state by reflected light, and in its natural state by direct light, as it had before done the reverse through the glasses. But it must be observed that something like a tube is necessary to confine the sight from other adjoining objects, which not being in the same circumstances would otherwise correct the imagination.

If we look through such a tube and glasses at the hearth or other object, suppose a piece of chocolate, the furrows in it appear so many ridges, on removing the tube they sink into furrows, on applying it they again rise into ridges, and the illusion might perhaps be repeated a thousand times, without the mind being at all able to conceive the object to appear through the tube like what it really is. But if whilst you are looking through the tube, and the object appears in its unnatural state, that is, when its furrows appear ridges, you apply your finger and feel that they really are furrows, the deception vanishes in a moment and the object appears in its natural state. This I at first supposed to arise from the superior confidence which we have in the sense of touching, as knowing by experience that this sense more perfectly represents the figure of bodies than the sight does. But I was, at least in part, mistaken. For if whilst you see the object in its unnatural state, another person 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 phenomenon 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,

F

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.

**T**HE 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 invifible 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 seen at immense heights, and inaccessible spots which give a lustre in the night.

Those who have attempted to give an account of these mountains, have ascribed their brightness to shining rocks or white moss, and the highest summit has been represented as inaccessible by reason of the extreme cold which threatens to freeze the traveller in the midst of summer. They have also differed so widely from each other, and their accounts have been embellished with so many marvellous circumstances, and on the whole have been so unsatisfactory, that I have long wished for an opportunity to visit these mountains in company with some gentlemen of a philosophical turn, furnished with proper instruments and materials for a full exploration of the phenomena that might occur. This pleasure I have in part enjoyed the present summer; and though the roughness of the way which prevented the use of convenient carriages, proved fatal to some of our instruments, and the almost continual cloudiness of the weather while we were in that region hindered us from making some observations which we intended; yet till a better account can be obtained, I flatter myself that what follows will prove more satisfactory than any which has yet been published or reported.

The white mountains are the highest part of a ridge which extends north-east and south-west to an unknown length. The area of their base is an irregular figure somewhat resembling an isocles triangle, whose longest extremity is toward the south, and whose whole circuit cannot be less than fifty miles. The number of summits within this area cannot be ascertained at present, the country round them being a thick wilderness. On the north-west side seven summits are in plain view, and this is the greatest number that can be seen at once from any station that is cleared of woods. Of these, four at least are bald. The highest of them is on the eastern side of the cluster,

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 sponge 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.

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 obstructions 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, ascended the mountain on the 19th of June, with some of the same party, and in the same spot the snow was five feet deep. On the 22d of July this year, we were assured by persons who live within plain view of the mountains, on the south side, at the distance of sixteen miles, that the snow had not been gone more than ten days. We were also credibly informed that two men, who attempted to ascend the mountain the first week of September last year, found the bald top so covered with snow and ice, then newly made, that they could not gain the summit; but this does not happen every year so soon, for the mountain has been ascended so late as the first week in October, when no snow was upon it; and sometimes the first snows that come dissolve before the winter sets in; but generally the mountains begin to be covered with snow 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 less of that bright appearance, from which they are denominated *white*. In the spring when the snow is partly dissolved, they appear of a pale blue streaked with white; and after it is wholly gone, at the distance of forty or sixty miles, they are altogether of a pale blue inclining to the colour of the sky; while viewed at the distance of only ten miles, they are of the grey colour of the rock inclining to brown. These changes are observed by people who live within constant view of them, and from these facts and observations it may justly be concluded that the whiteness of them is to be ascribed wholly to the snow and ice and not to any other white substance, for in reality there is none. There are indeed in the summer months some streaks which appear brighter than other parts, but these, when viewed through a telescope, I have plainly discerned to be the enlightened edges or sides of the long deep gullies, and the dark parts the shaded sides  
of

of them; and in the course of a day these spots may be seen to vary according to the position of the sun.

It may not be amiss to query here, if so great a quantity of snow is accumulated and remains on these mountains, may it not be supposed to add a keenness to the winds which blow over them? And how many more mountains may there be toward the north and west, whose hoary summits contain the like or greater bodies of snow and ice, some of which, at the remotest regions, may remain undissolved through the year? May we not then ascribe the piercing cold of our north-west winds to the infinite ranges of frozen mountains, rather than to the lakes and forests?

These immense heights which I have been describing, being copiously replenished with water, exhibit a variety of beautiful cascades, some of which fall in a perpendicular sheet or spout, others are winding and narrow, others spread on the level surface of some wide rock and then gush in cataracts over its edge. A romantic imagination may find full gratification amidst these rugged scenes, if its ardor be not checked by the fatigue of the approach. Three of the largest rivers in New-England receive a great part of their waters from this region. Amonoosuck and Israel rivers, two principal branches of Connecticut, fall from the western side of the mountains, Peabody river and another branch of Amarisogin from the north-eastern side, and almost the whole of Saco descends from the southern side. The declivities being very steep cause this latter river to rise very suddenly in a time of rain, and as suddenly to subside.

On the western part of these mountains is a pass which in the narrowest place measures but twenty two feet between two perpendicular rocks. Here a road is constructing with great labor and expence, which is the shortest rout to the upper Cohos on Connecticut river, and to that part of Canada which borders on the river St. Francis. At the height of this narrow pass the river Saco takes its rise.

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 incruited 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.

G

*Description*

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

## N° IV.

*Description of a remarkable ROCK and CASCADE, near the western side 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 state of Pennsylvania.*

BY THO. HUTCHINS:

Read January 28, 1786.

**T**HIS cascade is occasioned by a rock of a semicircular 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 extensive, and upwards of twenty feet in height, exhibiting a grand and romantic appearance. This very curious production is composed of stone of variegated colours, and a species of marble beautifully chequered with veins running in different directions, presenting on a close inspection a faint resemblance of a variety of mathematical figures of different angles and magnitudes. The operations of nature in this structure seems to be exceedingly uniform and majestic; the layers or rows of stone of which it is composed are of various lengths and thicknesses, more resembling the effects of art than nature. A flat thin stone from eight to ten inches thick, about twenty feet wide, forms the upper part of this amphitheatre, over which the stream 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 succession, principally, of limestone, strata over strata, and each stratum or row, projecting in an horizontal direction a little further out than its base, until it terminates into one entire flat, thin, extensive 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.*

S I R,

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 van-eering, 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.

*Description*

## N° VI.

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

BY DR. B. FRANKLIN.

Read January 28, 1786.

**T**OWARDS 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 I.

H

Whoever

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.

“ C H A P. 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 funnel, 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  
 “ at

Plate II.  
 Figure 20.



“ 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 to-  
 “ gether 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 va-  
 “ pour 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 cle-  
 “ verly, 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, 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 thoroughly 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

## 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 gilt 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 <sup>o</sup> 1, 2, 3 and 4, each, - -	1	3
Length of N <sup>o</sup> 5 and 6, each - - -	0	8 $\frac{1}{4}$
Breadth of the passage between N <sup>o</sup> 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 extre-		
mity of its knobs, - - -	0	5 $\frac{3}{4}$
Thickness of the bars at top, - - -	0	0 $\frac{1}{4}$
-----at bottom, lefs, - - -	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

			Fect.	In.
Diameter of the air-hole at top,	-	-	0	1 $\frac{1}{2}$
—————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 putting 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 grooves 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

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 whiffing, 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 vase when you would burn them. Then see that all the sliding plates are in their places and close shut, that no air may enter the stove but through the round opening at the top of the vase. And to avoid the inconvenience of dust from the ashes, let the ash-drawer be taken out of the room to be emptied; and when you rake the passages, do it when the draft of the air is strong inwards, and put the ashes carefully into the ash-box, that remaining in its place.

If being about to go abroad, you would prevent your fire burning in your absence, you may do it by taking the brass flame from the top of the vase, and covering the passage with a round tin plate, which will prevent the entry of more air than barely sufficient to keep a few of the coals alive. When you return, though some hours absent, by taking off the tin plate and admitting the air, your fire will soon be recovered.

The effect of this machine, well managed, is to burn not only the coals, but all the smoke of the coals, so that while the fire is burning, if you go out and observe the top of your chimney, you will see no smoke issuing, nor any thing but clear warm air, which as usual makes the bodies seen through it appear waving.

But let none imagine from this, that it may be a cure for bad or smoky chimneys, much less, that as it burns the smoke it may be used 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 sufficiently high might be raised in a very lofty room, the management to prevent all disagreeable vapour would be too nice for common practice, and small errors would have unpleasing consequences.

It is certain that clean iron yields no offensive smell when heated. Whatever of that kind you perceive, where there are iron stoves, proceeds therefore from some foulness burning or fuming on their surface. They should  
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 consumed by being turned 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

first that the small grate contained in it might be clogged by cynders, and the passage of the flame sometimes obstructed, I ordered a little door near the grate, by means of which I might on occasion clear it. Though after the stove was made, and before I tried it, I began to think this precaution superfluous, from an imagination, that the flame being contracted in the narrow part where the grate was placed, would be more powerful in consuming what it should there meet with, and that any cynders between or near the bars would be presently destroyed and the passage opened. After the stove was fixed and in action, I had a pleasure now and then in opening that door a little, to see through the crevice how the flame descended among the red coals, and observing once a single coal lodged on the bars in the middle of the focus, a fancy took me to observe by my watch in how short a time it would be consumed. I looked at it long without perceiving it to be at all diminished, which surpris'd me greatly. At length it occurred to me, that I and many others had seen the same thing thousands of times, in the conservation of the red coal formed in the snuff of a burning candle, which while enveloped in flame, and thereby prevented from the contact of passing air, is long continued and augments instead of diminishing, so that we are often obliged to remove it by the snuffers, or bend it out of the flame into the air, where it consumes presently to ashes. I then supposed that to consume a body by fire, passing air was necessary to receive and carry off the separated particles of the body; and that the air passing in the flame of my stove, and in the flame of a candle, being already saturated with such particles, could not receive more, and therefore left the coal undiminished as long as the outward air was prevented from coming to it by the surrounding flame, which kept it in a situation somewhat like that of charcoal in a well luted crucible, which, though long kept in a strong fire, comes out unconsumed.

An easy experiment will satisfy any one of this conserving power of flame enveloping 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 completed 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-

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.*

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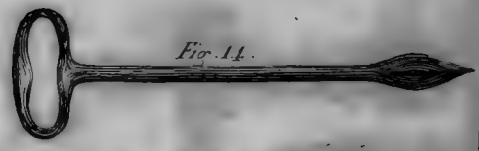
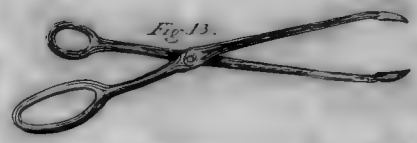
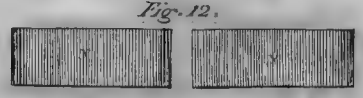
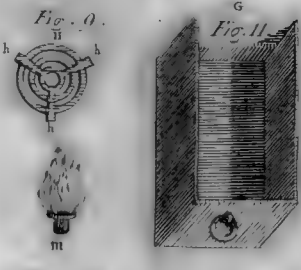
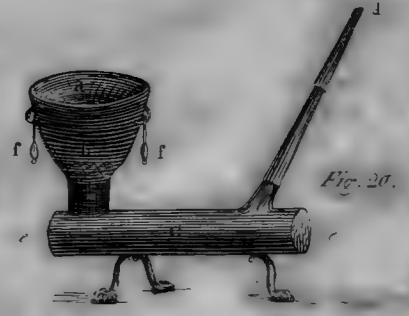
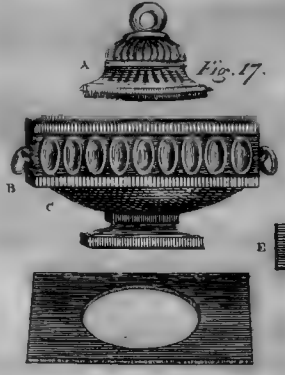
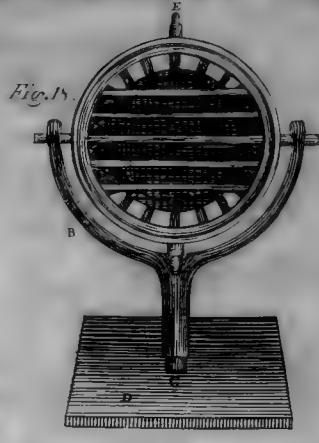
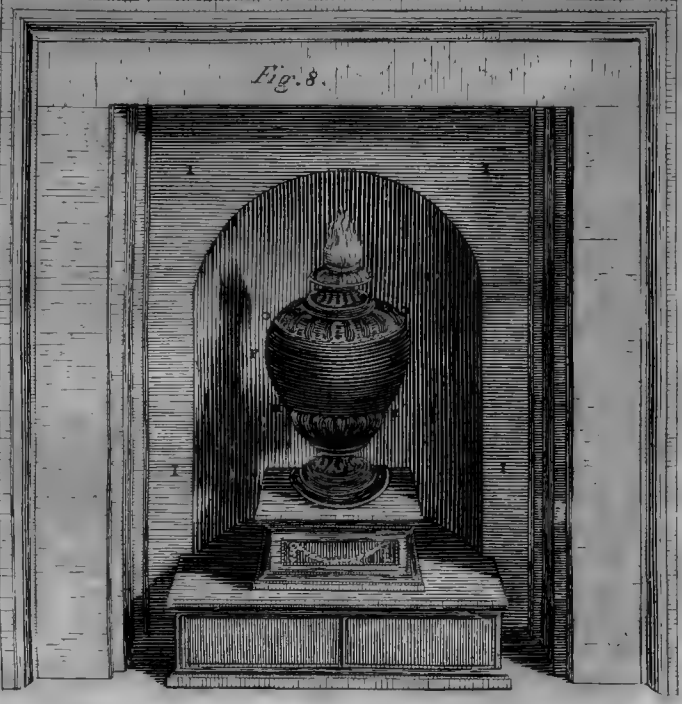
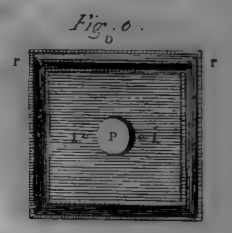
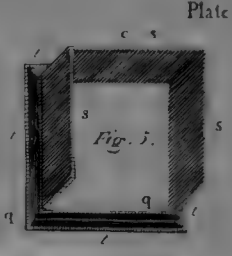
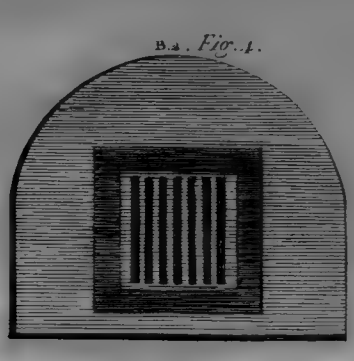
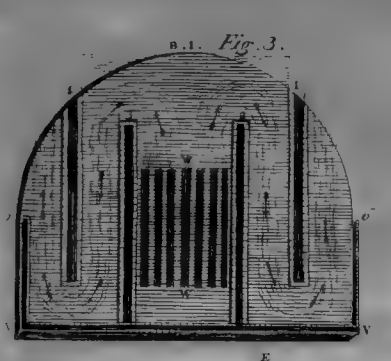
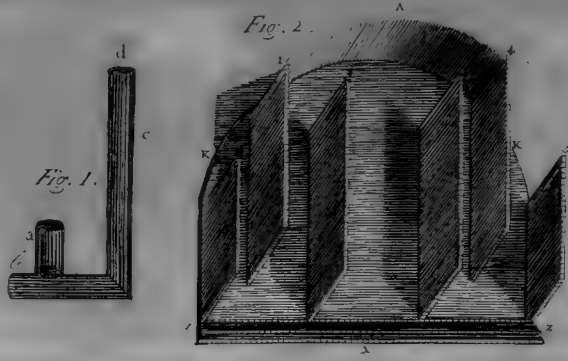
N° VII.

*A Theory of Lightning 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 lightning 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







natural quantities of it\*, discharge their surplufage 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 fufstains 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 doctör *Franklin's* letters on electricity, in which the doctör 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 condensed 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 condensed, fome  
 “ of the water will come out of its inner parts, and flow  
 “ on the furface. If the preffure of the fingers be intire-  
 “ ly removed, the fponge will not only refume what was  
 “ lately forced out, but attraät an additional quantity. As  
 “ the fponge in its rarer ftate will *naturally* attraät and  
 “ abforb *more* water; and in its denfer ftate will *naturally*  
 “ attraät and abforb *lefs* water; we may call the quantity  
 “ it abforbs in either ftate, its *natural quantity*, the ftate  
 “ being confidered.”

The doctör then fupposes, “ 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

K 2

“ 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-  
 “ ning 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 discharge, and his apparatus continued to be as much affected as ever. The clouds must consequently have received 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 lightning in both regions, and throughout the intermediate clouds\*. Here doctor *Priestley* justly observes, that “ the greatest difficulty 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 animates 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, inasmuch 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 487.

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 vacuum, we may adopt the idea of doctor *Franklin*, *mutatis mutandis*, and conclude 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 began to act a little, and that its virtue increased with the labour. Thinking it might be the warmth which produced 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 being 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 excited?” 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

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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 electricity; 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 summer, was perceived in the month of *July* on a *very dry* day, the heavens being very clear, and the sun *extremely 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 344.

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. *Kimmerfley* 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 lightning, 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-

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\* 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 reflection of 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* electrified, 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 doctor Priestley\*) they at length succeeded, by suspending  
" large

\* Page 244.

“ 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 cu-  
 rious electricians who made them, in the words of doctor  
*Priestley*, viz. “ The state of these two plates, they ” (*Wil-*  
*kie* and *Æpinus*) “ excellently observe, justly represents the  
 “ state of the clouds and the earth ” (and perhaps of dif-  
 ferent 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 lightning 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 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, mist or rain upon the surface of the earth from whence they sprang.

These vapors are either detached in streams from the humid ground by the influence of the sun, or thrown off by the perspirations of those infinite multitudes of animals and plants which cover the face of the earth\*, or supplied by evaporation, from the ocean, or other grand collections of water.

Ignorant as we are of the nature of these operations, and of the manner in which they are performed, it is natural to suppose, that the vapors themselves ascend in the same electric state, whether positive, neutral or negative, with the substances from which they arise. Accordingly signior *Beccaria*, in making some of his experiments, observed, that “ steam rising from an electrified eolipile diffuses itself with the same uniformity with which thunder clouds spread themselves and swell into arches, extending itself towards any conducting substance†.” This stream then was electrified as well as the eolipile from whence it proceeded. The sea must necessarily be supposed, in common with the whole terraqueous mass, to contain just its natural quantity of the electric fluid, and no more: We may therefore conclude that both the vapors which arise immediately from it, and the air which sustains them, and from its situation enjoys a more equable temperature, than that over the land, are in the same electrical state with the sea itself, containing neither *more* nor *less* than their *natural quantity*.

Considering the vast extent of the ocean, and the comparatively small degree of moisture of which the dry land is susceptible, we may conclude, that a very small proportion of the clouds which are formed in the atmosphere are exhaled from the latter, and that the ocean is the grand source from whence they principally derive their origin.

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Our

\* See *Hales's* vegetable statics, and *Chambers's cycloped*, under the word, Perspiration.

† *Priestley's History*, 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 extremely 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 Fahrenheit'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 electrified, 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 rarified, 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, con-

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tinually

\* 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 fullen blackness in a storm, excepting where the waves are interperfed 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 lightning 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 negative state of electricity, while pure, should be incapable of communicating its surpluse 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-  
flooded.

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 their 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 texture: 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 rarity with itself; and there it rests in equilibrio. This region is extremely 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 lightning, it would be impossible for them ever to unite into drops, but they must descend in the finest crystals, 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 electrified air which sustains and condenses them, the increasing density of the whole compound mass of air and vapor will, by degrees, occasion its descent 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 inveloped 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.

fourth, 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<sup>o</sup> VIII.

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

**I**N 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. Transf. vol. VIII, by *Martin*, pa. 655, as it was observed by Mr. *Joseph Harris*, May 21, 1732, about sunset, lat.  $32^{\circ} 30'$  N. long.  $9^{\circ}$  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 perpen-  
 “ dicular 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 al-  
 “ teration, 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 crowd 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 lighter 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 concurrence in one place, will excessively crowd 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 concurrence, 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 compression 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 Lightning, &c. page 90.

expelled, which was previously incumbent upon the calm surface beneath; the electric attraction of which probably assists the further ascent of these particles after the first fury of the blast is spent. There it undergoes another operation being converted into vapor, whereby it is wholly discharged of the marine salts it carried up with it\*; which are now left to shift for themselves, together with innumerable other heterogeneous corpuscles which successively float in the atmosphere, and which in due time, become severally subservient to many wise purposes in the œconomy of nature. These 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 dense cloud, in like manner as thunder clouds are formed over the land; but with much greater expedition, as the supply of vapors is more sudden. This cloud will then be ready in a short time to discharge a shower of fresh water upon the sea from whence it rose, and may be attended with thunder and lightening, or not, as the air in which the cloud was formed was more or less electrical, or the cloud extensive.

A previous calm may not be *necessary* to the production of these phenomena, and indeed they frequently happen without one: But, upon the same principle, if it be calmer where they are produced, or the state of the atmosphere there be such as to dispose it to acquire and retain the heat acquired from the sun's rays, more than in the surrounding regions, which, as we have seen above, may be the case, the effects may be the same in kind, though perhaps not in degree; the most perfect water spouts probably rising from whence there has previously been a dead calm, or nearly such, for the foregoing reasons.

If

\* The water carried up in one of these spouts is undoubtedly salt when it first rises from the sea, as it ascends in great quantities, and in a very dense column; but it is always fresh when it descends again in a shower: It must therefore in the mean time have gone through a complete natural distillation.

† Theory of Lightning, &c. page 92.

If there be any wind at the time of the phenomenon, the aerial funnel through which the water ascends, instead of being perpendicular to the horizon, as it would be in a calm, might incline more or less to it, in proportion to the strength or weakness of the prevailing current of air: Or, instead of continuing in one spot, it might have a progressive motion over the surface of the sea, in the direction of the general current; both of which circumstances frequently take place. In either case it is natural to suppose, that both air and water would ascend spirally, as through the worm of a screw, every current which sets in towards the centre receiving an oblique bias from the prevailing current.

It sometimes happens, that after the subsiding of a spout, it is succeeded by a second, and that by a third, either in the same place, or at no great distance from it. But this also is analogous to what we observe upon the plunging of heavy bodies out of air into water. For, after the first subsiding of the small column of water which is occasioned by it, and is above resembled to an icicle, the water again rises and subsides as at first, though not in the same degree, as may be concluded from those fainter concentric circles which expand from the same centre after the subsidence of the first column. The same thing which here takes place in water, may also take place in air, under similar circumstances.

Since writing the foregoing, while I was endeavouring to contrive some experiment to illustrate the subject, a very simple one was suggested to my mind, the success of which I think demonstrates the truth of the hypothesis introduced above to account for the first ascent of the water in the spout; the event being precisely the same as was expected before hand, and as ought to have taken place upon the principles above advanced.

#### E X P E R I M E N T.

In a stiff paper card I made a hole just big enough to insert a goose quill so 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 suction 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 several quarters at the same time, and filling the air hereabouts with mighty thick and black clouds, layer upon layer; the wind thus blowing soon created a great *vortex, gyration* and *whirling* among the clouds; the centre 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 screwing 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 circular 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 beheld 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 greatest condensation and gravitation was, falling down into a pipe or great tube (something like the *cochlea Archimedis*) and that in its working or whirling motion, either sucks up water, or destroys ships, &c. Having travelled about a quarter of a mile farther, it dissolved by the prevalence 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, frightened 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 further mischief.

“ By all the observations that I could make of this, and  
 “ the former, I found that had they been at sea and joined  
 “ to the surface thereof, they would have carried a vast  
 “ quantity of water up into the clouds, and the tubes would  
 “ then have become much more strong and opaque than  
 “ they were, and have continued much longer.

“ It is commonly said that at sea the water collects and  
 “ bubbles up a foot or two high under these spouts before  
 “ that they be joined : But the mistake lies in the pellu-  
 “ cidity and fineness of those pipes, which do most certain-  
 “ ly touch the surface of the sea before that any consider-  
 “ able motion be made in it, and that, when the pipe be-  
 “ gins to fill with water, it then becomes opaque and  
 “ visible.”

I shall here make a remark or two upon the above cited author's mode of expression in the foregoing accounts, which is evidently adapted to a preconceived idea of the *cochlea Archimedis*, by supposing something similar to which, as taking place in our atmosphere, he is not alone in endeavouring to account for these phenomena. In conformity to this idea he speaks of the spout as *drawing upwards*, and *screwing up* whatever it touched ; and supposes that by its *spiral motion* and *swift turning*, water ascends in it as in the *screw of Archimedes*. But this hypothesis, however specious, has been long since exploded as unphilosophical.

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 distinctly viewed a motion like that of a screw ; and as such he seems to have supposed it acted, viz. either in the manner of a cork-screw upon solids, or as the *cochlea Archimedis* upon fluids, drawing them up into the atmosphere. But as he himself afterwards, when applying his observations to a spout at sea, very justly concludes that the pellucidity and fineness of these pipes over the water render them invisible below,  
 “ notwithstanding

“ notwithstanding (as he conceives) that the pipes do most certainly touch the surface of the sea before any considerable 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 concurrence 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

P 2

stream,

\* Lowthorp's Abridgm. Phil. Transf. 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 wherever 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.*

**I**N 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.

## E X P E R I M E N T 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,  
fill

fill 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.

### E X P E R I M E N T   I I I .

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  $\frac{3}{4}$  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:

<i>The several sorts of plants.</i>	<i>Weight of the plants.</i>	<i>Water evaporat- ed in 30 days.</i>
	<small>Grains.</small>	<small>Grains.</small>
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, grafs, 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<sup>h</sup> A. M. the second at noon, and the third at 9<sup>h</sup> 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	30,0	18½	W. to S. W. little.	Fair day; snow in the night.
2	30,1	27	W. little.	Fine pleasant weather.
3	30,1½	18	S.	Cloudy day; rain in the night.
4	29,5	33	S. W.	Fine pleasant weather.
5	29,6	40	N. W. strong.	Clear bright day.
6	29,7½	18	N. W.	Ditto. <i>Aurora Borealis</i> in the evening.
7	29,9½	13	N. W. to S.	Cloudy, dull weather.
8	30,1	22½	W. little.	Fine pleasant day.
9	30,1½	29½	Ditto.	Ditto.
10	30,1	27	Ditto.	Ditto.
11	30,1½	34	Ditto.	Cloudy day; snow in the night.
12	30,1	30	Ditto.	Flying clouds with bright intervals.
13	29,8	26	E. fresh.	Mild pleasant day.
14	29,6	32	S. W. to W.	Ditto.
15	29,5½	34½	W. little.	Snow A. M. clear P. M.
16	29,7½	30	Ditto.	Clear bright day.
17	29,4½	25	S. E. to W.	Cloudy, with some snow.
18	29,2	27	W.	Cloudy dull weather.
19	29,5	21	N. to S. W.	Storm of snow, all day; bright evening.
20	29,1	14	N. to N. W.	Fair weather.
21	28,8	26½	N. W.	Cloudy dull day.
22	29,1	20	S. E.	Cloudy, with bright intervals.
23	29,0	19	S. little.	Dull miffing day.
24	29,0½	34	W. little.	Fine pleasant day.
25	29,3	30	N. W.	Ditto.
26	29,6	26	N.	Cloudy dull weather.
27	29,5½	21½	Ditto.	Ditto, with rain.
28	29,2	17	W. little.	Fair and clear.
29	29,3	23	N. E. fresh.	Cloudy day; storm of snow in the night.
30	28,7	20	N. E. to S.	Cloudy dull weather.
31	28,0	37		

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

FEBRUARY, 1772.

Days.	Barometer.		Thermometer.		Winds.	Weather.
1	29.0 $\frac{1}{2}$	29.1	31	35	S. W. little.	Fair A. M.; cloudy P. M.
2	29.3 $\frac{1}{2}$	29.4	29	35	E. fresh.	Fine bright day.
3	29.2	29.1	20	23	S. to N. W. fresh.	Cl. day; hea. stor. of sn. & hail in the nig.
4	28.9	29.2	26	32	N. W. fresh.	Dull cloudy day.
5	28.7	29.7	21 $\frac{1}{2}$	23 $\frac{1}{2}$	S. W. to W.	Flying clouds, with bright intervals.
6	30.1	30.1	14 $\frac{1}{2}$	20 $\frac{1}{2}$	N. W. little.	Fine bright day. <i>Aur. Bor.</i> in the night.
7	30.0 $\frac{1}{2}$	29.9	23	31	S. W. to W.	Fine pleasant weather.
8	30.2 $\frac{1}{2}$	30.2	23	25	N. to E. S. E.	Cloudy dull day.
9	29.4	29.1	37	40	S. to W. fresh.	Rainy A. M.; cloudy P. M. bright even.
10	29.7 $\frac{1}{2}$	29.9	22	25 $\frac{1}{2}$	N. W. fresh.	Fair and clear.
11	30.0	29.6	25	30	S. W. to W. fresh.	Ditto.
12	30.2 $\frac{1}{2}$	30.2	20	19 $\frac{1}{2}$	N. W.	Fair day.
13	30.4	30.4	3	11	N. W. to N. E.	Fair day; cloudy evening.
14	30.2	29.9	11 $\frac{1}{2}$	12 $\frac{1}{2}$	N. to W.	Snow A. M.; clear P. M.
15	30.1	30.1	7 $\frac{1}{2}$	14	W. to E.	Cl. A. M; in. with ra. P. M. & in the nig.
16	29.2 $\frac{1}{2}$	29.1	27	32	S. W. little.	Fair pleasant day.
17	29.6	29.6	16 $\frac{1}{2}$	26	W. to S. W. fresh.	Flying clouds, with bright intervals.
18	29.6	29.6	25	39	S. W. little.	Fine pleasant day.
19	29.6	29.6	36 $\frac{1}{2}$	41 $\frac{1}{2}$	S. W.	Ditto.
20	29.6	29.6	40 $\frac{1}{2}$	49 $\frac{1}{2}$	Ditto.	Fine pleasant day.
21	29.2	29.4	49 $\frac{1}{2}$	48	S. W. to E.	Cloudy day; rain in the night.
22	29.5	29.5	44	49	Ditto. fresh.	Fine pleasant day.
23	29.6	29.6	42	43	S. W. to E.	Ditto.
24	29.8	29.8	38	40	N. E. to S. strong.	Heavy storm of snow and rain all day.
25	29.0	29.0	35	34	W. to N. strong.	Flying clouds, with bright intervals.
26	28.6	28.6	31	32	W.	Fine pleasant day. <i>Aur. Bor.</i> in the nig.
27	29.3	29.3	25	35	S. W. little.	Ditto.
28	29.3	29.4	31	37	Ditto. fresh.	Ditto.
29	29.2	29.2	37	39		

Quantity of rain in February, 2.40 } Inches.  
Quantity of water evaporated, 1.65 }

MARCH, 1772.

Days.	Barometer.		Thermometer.		Winds.	Weather.
1	29,0½	29,5	28	31	N. W. to S.	Fair weather.
2	29,0	29,3½	29	30½	S. W. little.	Fine bright day.
3	29,0	29,9	27	31	N. E.	Cloudy dull weather.
4	30,0½	30,0	30	34	N. to W. little.	Fine pleafant day.
5	29,8	29,4	27½	33	N. E. strong.	Heavy form of fnow.
6	28,8	29,1	31	34	W. freh.	Clear pleafant weather.
7	29,8	29,8	16	25	W. little.	Ditto.
8	29,9	29,9	23	37	Ditto.	Ditto.
9	30,2	30,1½	26	31	N. freh.	Cloudy with fome fnow.
10	30,0	30,0	17½	25	E. little.	Fine pleafant day.
11	29,7½	29,5	17	27½	N. N. E.	Clb. A. M; heavy form of fnow P. M.
12	28,7	29,2½	30	32	W. freh.	Cloudy morning; clear bright day.
13	29,8	29,8	16	26	N.	Cloudy with fome fnow.
14	29,8	29,8	22	27	W.	Clear bright day.
15	30,2	30,2	12	23	Ditto.	Ditto.
16	30,2	30,1	10½	23	W. to S. freh.	Dull cloudy weather.
17	29,6	29,7	28	31	W. to S. freh.	Clear bright day.
18	30,0	30,0	20	36½	W. to S.	Clear A. M; cloudy P. M.
19	29,6	29,4	33	39½	S. W. to S. E.	Ditto.
20	29,4½	29,2½	34	32½	N. E. to N.	Clb. mor. for. of fn. P. M. & in the even.
21	28,9	28,9	33	38	W. S. W. freh.	Cloudy weather.
22	29,3	29,3½	37	40	Ditto.	Flying clouds, with bright intervals.
23	29,6	29,6½	40½	46	S. little.	Fine pleafant day.
24	29,6	29,6	39	45	S. to S. E.	Cloudy weather.
25	29,6	29,6	38	46½	S. to E. little.	Fine pleafant day.
26	29,6	29,6	36	39½	N. to S. E. little.	Dull mifling weather.
27	29,6	29,6	38	47½	S. to E. little.	Fine pleafant weather.
28	29,7½	29,7½	36	40	E. to N. E. freh.	Cloudy day; rainy evening and night.
29	29,3	29,2	36	39	F. freh.	Heavy form of fnow & rain, all day.
30	29,3	29,4	36	36½	N. N. E.	Rainy weather.
31	29,8	29,9	36	44	N. to E. little.	Fine pleafant day.

Quantity of rain in March, 4,93 } inches.  
 Quantity of water evaporated, 1,45 }

APRIL, 1772.

Days.	Barometer.		Thermometer.		Winds.	Weather.
1	29,8	29,8	47	S. little.	Fine pleafant weather.	
2	29,7½	29,6	35	N. E. frefh.	Cloudy dull day.	
3	29,3½	29,4	37	N. frong.	Ditto, with fome fnow.	
4	29,6	29,7	36	N. to W. little.	Cloudy A. M.—fair and pleafant P. M.	
5	29,8	29,8	39½	W. to S. W. little.	Fine pleafant weather.	
6	29,5	29,4	45	E. little.	Ditto.	
7	29,5½	29,3½	45	S. E. little.	Ditto.	
8	29,5½	29,4	48	E.	Clear A. M. clou. P. M. and in the night.	
9	29,2½	29,1	38	N. E. to N. frong.	Storm of rain all day.	
10	29,2½	29,1	51	N. W. to S. W.	Fine pleafant day.	
11	29,2½	29,1½	54½	S. W. to S. little.	Fair day; clo. evening; fmall fhowers in the night.	
12	29,0	29,0	46	S. W. to W. frefh.	Fair weather. <i>Aur. Bor.</i> in the night.	
13	29,2	29,2	41½	Variable.	Cloudy dull day.	
14	29,2	29,2	46	S. W. frefh.	Flying clouds, with bright intervals.	
15	29,6	29,6	49	W. by S.	Fine bright day; rain in the night.	
16	29,8	29,5½	47	S. to S. E.	Rainy all day.	
17	29,4	29,5½	50½	S. E. to W. little.	Fair pleafant weather.	
18	29,9½	30,0	49	W. little.	Ditto.	
19	30,1	30,0½	53	N. E.	Fair A. M. cloudy P. M.	
20	29,8	29,1½	44	NE to NW frong.	Storm of rain 'till 4 P. M. clou. evening.	
21	29,4½	29,6½	39½	N. W. frefh.	Fair A. M. fhowers P. M. & in the even.	
22	29,7	29,7	55	S. W.	Fine pleafant day.	
23	29,3	29,1	58	Variable.	Fair A. M. fhowers P. M. and in the even.	
24	29,2	29,2	47	S. to W. frefh.	Dull cloudy day.	
25	29,5	29,6	45	N. E. to W.	Ditto.	
26	29,6½	29,6	48½	W. to S. E.	Fine pleafant day.	
27	29,7½	29,5½	53	S. little.	Ditto.	
28	29,6	29,6	64	Ditto.	Ditto.	
29	29,8	29,8	62	Ditto.	Ditto.	
30	29,8	29,5½	60	E. frefh.	Ditto.	
			54	S. W.	Ditto.	
			69			

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

MAY,

M A Y, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29,6	64	75½	Fair weather.
2	29,3	71	75	Do. Light, in the night, with thund.
3	29,8	58½	N. W.	Cloudy, with bright intervals.
4	30,2	42½	E. E.	Clear A. M. cloud. P. M. & in the evening.
5	30,1½	46	S. E.	Dull cloudy weather.
6	29,9½	49	Variable.	Fine pleasant day.
7	29,8½	55	E.	Ditto.
8	30,0	58½	N. E. to N. N. W.	Cloudy A. M. rainy P. M. & in the night.
9	29,9	49	S. W. fresh.	Fair A. M. small showers P. M.
10	29,1	51	N. W. fresh.	Fair and pleasant.
11	29,4½	49	W.	Ditto.
12	29,8½	43	Variable.	Cloudy A. M. rainy P. M.
13	29,7	52½	W.	Clear and pleasant.
14	29,6½	50	Ditto.	Ditto.
15	29,7½	52	S. W. little.	Ditto.
16	29,6	60½	Ditto.	Ditto.
17	29,5½	63	Variable.	Ditto.
18	29,3½	67	E.	Ditto.
19	29,5	60	N. E.	Cloudy A. M. rainy P. M.
20	29,4½	53	S. W. to N. W.	Fair day; cloudy evening.
21	29,5	57	N. W.	Small showers A. M. cloudy P. M.
22	29,6½	59½	S. W. fresh.	Fine pleasant day.
23	29,5	56	Ditto.	Ditto.
24	29,7	72	S. W. to N. E.	Fair till 3 P. M. cloudy evening & night.
25	29,3½	65	N. to N. E.	Dull cloudy day.
26	29,3	53	Ditto.	Ditto.
27	29,4	70	N. E. to S. W.	Ditto. } Small showers.
28	29,7	55	S. W. to N.	Ditto. }
29	29,6½	55	N. W. to S.	Ditto. }
30	29,5½	61	S. W. fresh.	Fair and pleasant.
31	29,5	63		Ditto.

Quantity of rain in May. 1,90 }  
 Quantity of water evaporated, 6,35 }  
 Inches.

JUNE,

JUNE, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29,4	65	S. W. fresh.	Fair day; thun. & lighten. in the night.
2	29,5	64	N. E.	Dull cloudy weather.
3	29,7	59½	Ditto.	Fine pleafant day.
4	30,0	64	N. W. S. W.	Ditto.
5	30,1	73	W. to S. S. W.	Ditto.
6	30,0	61	S. W.	Ditto.
7	29,5	78	S. W. to N. N. E.	Ditto.
8	29,5	72½	S. S. W.	Ditto.
9	29,8	62	W.	Ditto.
10	29,8	55½	W. to N. E.	Fair A. M; cloudy P. M.
11	29,7	62	N. E. to S. S. E.	Cloudy day; showers in the night.
12	29,3	58	W. fresh.	Fine bright day.
13	29,0	64	Ditto.	Ditto.
14	29,6	67	S. W.	Ditto. lightening in the evening.
15	29,6	77	S. W. fresh.	Ditto.
16	29,3	73	S. W. to W. fresh.	Cloudy A. M; rainy P. M.
17	29,5	69	N.	Cloudy dull day.
18	29,5	66	Ditto.	Rainy day.
19	29,4	60	Ditto.	Ditto.
20	29,3	60½	Ditto.	Ditto.
21	29,4	63½	N. to E.	Fair evening.
22	29,4	62	N. E.	Cloudy dull day.
23	29,4	67½	N. E.	Cloudy, with showers.
24	29,4	56½	N. to W.	Flying clouds with bright intervals.
25	29,4	61	Ditto.	Ditto.
26	29,4	70	Ditto.	Ditto.
27	29,3	74	N. W. to E.	Cloudy, with rain.
28	29,3	59	N. W. fresh.	Flying clouds, with bright intervals.
29	29,8	67	W.	Fine pleafant weather.
30	29,7	73	S. W.	Ditto.
	29,6	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	74	77½	S. W.	Small showers A. M. fair & pleasf. P. M.
2	29.4	29.4	71	75	Ditto.	Cloudy, with small showers.
3	29.4½	29.4½	74	77	N. to E.	Ditto, lightening in the evening.
4	29.3½	29.2½	71	78	S. W. to W.	Ditto.
5	29.6	29.6½	73½	80½	W.	Fine pleasf. day. <i>Aur. Bor.</i> in the evening.
6	29.6	29.6	71½	81	S. W. strong.	Fair A. M.; cloudy P. M.
7	29.8	29.7½	72	79	S. W. to N. W.	Fair till 2 P. M. clou. with th. & ra. nig.
8	29.5½	29.4½	77	86	W.	Fair day. <i>Aur. Bor.</i> in the night.
9	29.6	29.6	77	81	Ditto.	Fine pleasf. day.
10	29.6	29.6	66	67	Ditto.	Fair A. M. shower of rain and hail P. M.
11	30.1	30.1	62	68½	Ditto.	Fine pleasf. day.
12	30.0½	30.0	78	74	Ditto.	Ditto.
13	29.9	29.9½	70	75	W. to S. little.	Ditto.
14	29.9	29.9½	70	78	S. W.	Fair A. M. a small shower P. M.
15	29.6	29.6½	73	77½	Ditto.	Cloudy, dull weather.
16	29.6	29.6½	71	76	Ditto.	Cloudy, with bright intervals.
17	29.6½	29.6½	74	78	Ditto.	Fine pleasf. weather.
18	29.8½	29.9	65	72	Ditto.	Fair A. M. cloudy P. M.
19	29.9½	29.9½	72	71	Ditto.	Fine pleasf. day.
20	30.1½	30.1	72	70	Ditto.	Fair A. M. cloudy P. M. rain in the night.
21	30.1	29.9	68	75	Ditto.	Cloudy A. M. clear P. M.
22	30.0½	29.7	66	73	Ditto.	Ditto.
23	29.5	29.5	65	71	N. E. to W.	Cloudy, with thunder and rain.
24	29.6	29.6½	68	72	Variable.	Pleasant day. <i>R. m. Aur. Bor.</i> in the nig.
25	29.8½	29.9	73	80½	W. to N.	Fair pleasf. weather.
26	30.0½	29.7	74	86½	S. to S. W.	Ditto.
27	29.8½	29.8	76	87	Ditto.	Lightening in the evening.
28	29.8	29.6	78	87	Ditto.	Ditto.
29	29.4	29.3	81½	81	Ditto.	Fair A. M. thunder and rain P. M.
30	29.5	29.3	78	78	Ditto.	Dull rainy day.
31	29.8	28.9	72½	71½	Variable.	

Quantity of rain in July, 2.95 } Inches.  
 Quantity of water evaporated, 4.60 }  
 \* This observation was taken at 1½ P. M. The thermometer being hung in a shade in the open air.

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AUGUST, 1772.

Days,	Barometer.	Thermometer.	Winds.	Weather.
1	29.5	65	N. W.	Flying clouds, with bright intervals.
2	29.9 1/2	66	W.	Fine pleafant weather.
3	30.3 1/2	73 1/2	Ditto, little.	Ditto.
4	30.2	79	S.	Ditto.
5	30.1 1/2	78	N.	Heavy showers; clears up in the night.
6	29.7	70	S. W.	Fair weather.
7	29.7	82	Ditto.	Ditto.
8	29.5	77 1/2	Ditto.	Fair day; terr. thun. & highcg. in the nig.
9	29.4	83	Variable.	Cloudy A. M.; fair P. M.
10	29.6	86	N. E. little.	Cloudy day; small showers in the even.
11	29.2	67	Variable.	Unfictled weather.
12	28.9	81	S. W.	Fair and pleafant.
13	29.2	79	Ditto.	Ditto.
14	29.5	80	Ditto.	Ditto.
15	29.5	76	S.	Fair A. M. showers P. M.
16	29.4 1/2	80 1/2	N. E.	Dull cloudy day.
17	29.4	72	Ditto.	Cloudy day; rain in the night.
18	29.5	66	Ditto.	Dull rainy weather.
19	29.6	63 1/2	S. to N. E.	Unfictled weather.
20	29.8	68	Variable.	Ditto.
21	29.9	65 1/2	W.	Fine pleafant weather.
22	29.9	70	S. W.	Ditto.
23	29.5	71	Ditto.	Fair A. M. small showers P. M.
24	29.3 1/2	80	N. E. to E. little.	Fine pleafant day.
25	29.9	62 1/2	S.	Cloudy day; heavy showers in the night.
26	28.5 1/2	67	S. strong.	Flying clouds, with bright intervals.
27	28.3 1/2	72	Ditto, little.	Fine pleafant day.
28	29.2 1/2	78	W.	Ditto.
29	29.6 1/2	71	Ditto.	Ditto.
30	30.0	69	S. W. little.	Ditto.
31	30.3	74	S. W. little.	Ditto.
	30.1	76		

Quantity of rain in August, 3.15 Inches.  
 Quantity of water evaporated, 4.35 }

SEPTEMBER, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29,6	29,7		
2	29,7	29,6½		
3	29,2	29,9		
4	29,2½	29,2		
5	29,7½	29,8		
6	30,1	30,0½		
7	30,1	30,1		
8	29,8	29,8½		
9	29,6	29,8		
10	29,4	29,7½		
11	29,2	29,4		
12	29,1	29,1½		
13	29,8½	29,9		
14	30,0	30,0½		
15	30,0	29,9		
16	29,9	29,8		
17	29,4½	29,3		
18	29,5	29,5		
19	29,8	29,8½		
20	29,8	29,7		
21	29,8	29,8		
22	29,8	29,8½		
23	29,4½	29,4		
24	29,6½	29,6½		
25	29,8	29,8		
26	29,8	29,8½		
27	29,9	29,9½		
28	29,9	29,8		
29	29,7	29,6½		
30	29,7	29,7		

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

OCTOBER, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29,7	56	N.	Dull cloudy day.
2	29,7 1/2	64	W.	Fine plea. day. <i>Aur. Bor.</i> in the night.
3	29,8 1/2	52	Ditto.	Ditto.
4	29,9 1/2	54	Ditto.	Ditto.
5	30,0	57	S. little.	Fair weather.
6	29,9 1/2	58	N. E.	Dull cloudy day.
7	29,7 1/2	56 1/2	N.	Storm of rain all day.
8	28,8	54	Ditto.	Ditto.
9	28,6	56	Ditto.	Dull milding day.
10	29,2	57	Ditto.	Ditto.
11	29,6 1/2	57 1/2	W. little.	Fair pleafant weather.
12	29,8 1/2	57	Ditto.	Ditto.
13	30,0	60	S. W. little.	Ditto.
14	30,0 1/2	57	W. N. W.	Ditto.
15	30,0 1/2	55	Ditto.	Ditto.
16	30,2	55 1/2	Ditto.	Ditto.
17	30,0 1/2	62	E. to N. W.	Ditto.
18	29,7 1/2	57 1/2	W. to S. E.	Ditto.
19	29,7	60	Variable.	Cloudy dull day.
20	29,2	61	S. W. little.	Fine pleafant day.
21	29,3	56	N. E. little.	Cloudy A.M. milding P.M. & in the nig.
22	29,2	50	W. fresh.	Cloudy A. M.; rainy P. M.
23	29,4	50	W.	Fair weather.
24	29,4 1/2	49	W. to S. W.	Fine pleafant day.
25	29,4	54	S. W. fresh.	Cloudy A. M.; fair P. M.
26	29,3	50 1/2	S. W.	Fine pleafant day.
27	29,6	50 1/2	N. E. fresh.	Cloudy with some rain.
28	29,2	49	N. E.	Dull cloudy weather.
29	29,2 1/2	53	S. W.	Cloudy A. M.; rainy P. M. & in the nig.
30	29,1	51	S. E. fresh.	Flying clouds with bright intervals.
31	29,4	52	N. W. fresh.	

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

# METEOROLOGICAL OBSERVATIONS.

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NOVEMBER, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29.6	48	S. W. little.	Fine pleasant day.
2	29.7	47½	S. E.	Fair A. M.; clo. P. M. sho. in the nig.
3	29.7	53	N. W. fresh.	Fair and pleasant.
4	29.6	53	S.	Ditto.
5	29.6	52	N. W. fresh.	Ditto.
6	29.6	42	N. W. fresh.	Ditto.
7	29.9	42	Ditto.	Ditto.
8	30.0½	39	S. little.	Ditto.
9	30.0½	46	W. little.	Ditto.
10	30.3	48	S. E. little.	Ditto.
11	30.4	47	S. fresh.	Cloudy dull weather.
12	30.4	45	N. W. fresh.	Fair weather.
13	30.0	46	Ditto.	Fine pleasant day.
14	29.9	42	S. E. to E. fresh.	Rainy day.
15	29.5	38	N. W.	Fair pleasant weather.
16	29.9	43	S. W. to W.	Ditto.
17	30.0	39	W.	Ditto.
18	30.4	35	Ditto.	Ditto.
19	30.5	27	S. W. to W.	Ditto.
20	30.2	32	Ditto.	Ditto.
21	30.3	34	S. W. to W.	Ditto.
22	30.4	37½	Ditto.	Ditto.
23	30.4	40	W.	Ditto.
24	30.3	43	N.	Dull cloudy day.
25	30.4	40	Variable.	Ditto.
26	30.3	43	W.	Fair pleasant weather.
27	30.4	42	Variable.	Cloudy dull day.
28	30.0	40½	S. E.	Dull rainy weather.
29	29.7	46	S. E. to E.	Ditto.
30	29.6	44	N. E. to N. W.	Cloudy day; fair evening.
31	29.4	45	W. little.	Fine pleasant day.
1	29.8	41	E. fresh.	Cloudy with rain; stormy night.
2	30.0	38	Variable.	Cloudy day; fair evening.
3	30.1	50		
4	29.7	48		
5	30.0	50½		
6	29.6	48		
7	29.7	48		
8	29.7	48		
9	29.7	48		
10	29.7	48		
11	29.7	48		
12	29.7	48		
13	29.7	48		
14	29.7	48		
15	29.7	48		
16	29.7	48		
17	29.7	48		
18	29.7	48		
19	29.7	48		
20	29.7	48		
21	29.7	48		
22	29.7	48		
23	29.7	48		
24	29.7	48		
25	29.7	48		
26	29.7	48		
27	29.7	48		
28	29.7	48		
29	29.7	48		
30	29.6	48		

Quantity of rain in November, 3.90 } Inches.  
Quantity of water evaporated, 2.45 }

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DECEM-

DECEMBER, 1772.

Days.	Barometer.	Thermometer.	Winds.	Weather.
1	29.8	43	Variable.	Fair day; rainy evening.
2	29.8	47	S. E.	Rainy weather.
3	29.6	47½	N. W. fresh.	Cloudy A. M.; fair P. M.
4	29.8	43	W. little.	Fine pleafant day.
5	29.9	38	Ditto.	Ditto.
6	30.0	36	N. W. to N. E.	Cloudy unpleafant weather.
7	30.1	34	N. W.	Fair day.
8	30.0	31	Ditto.	Flying clouds, with bright intervals.
9	29.9½	28	W. to S.	Fair A. M. clo. with fmall flowers P. M.
10	29.9	25	S. fresh.	Fine pleafant day.
11	29.8	33	N. W. fresh.	Fair and clear.
12	29.9½	25	N. W. little.	Ditto.
13	29.9	30	S. fresh.	Cloudy day; fair evening.
14	29.8½	33	W. little.	Fine pleaf. day. <i>Aur. Bor.</i> in the night.
15	30.3	39	E. N. E. fresh.	Fair A. M. cloudy P. M.
16	30.4	36	N. E. fresh.	Cloudy A. M.; rainy P. M.
17	30.0	34½	S. W.	Cloudy unlettled weather.
18	30.2	33½	Ditto.	Ditto.
19	30.1½	40	Ditto.	Fine pleafant day.
20	30.2	38½	Ditto.	Cloudy A. M.; rainy P. M.
21	30.1	41½	N. E.	Fine pleafant day.
22	29.8½	41	W. little.	Ditto.
23	30.1	44	S. little.	Ditto.
24	29.8	43	W.	Cloudy day; fnow in the evening.
25	29.9	38	N.	Fine pleafant day.
26	30.1	35	W.	Cloudy day; rain in the night.
27	29.4½	38	Variable.	Dull mulling day.
28	29.6	41	N. W.	Fine pleafant weather.
29	29.8½	39½	Wby S. little.	Ditto.
30	29.6	45	N. W.	Ditto.
31	29.9	35	S.	Cloudy, dull weather.

Quantity of rain in December, 2.15 } Inches.  
 Quantity of water evaporated, 3.10 }

## METEOROLOGICAL OBSERVATIONS. 135

The greatest height of the barometer this year, was on the 17th of November: The mercury was then at  $30.5\frac{2}{3}$  inches. The least height was  $28.2\frac{2}{3}$  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  $3^{\circ}$ : 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 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<sup>h</sup> 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<sup>h</sup>. The thermometer on the 8th of July, at 12<sup>h</sup> $\frac{3}{4}$ , 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 0. 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<sup>h</sup>. It began about 20 $^{\circ}$  above the horizon and instantly spread itself in a horizontal direction to the distance of 30 $^{\circ}$  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.

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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<sup>h</sup>  $\frac{3}{4}$  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, scattered or carried off, and the various kinds of lumber that lay dispersed on the shore, were whirled about in different directions, and to different distances. Some houses and vessels that seemed the most exposed to the wind, suffered nothing at all; and others that seemed to be the least exposed, were much damaged or carried off. The number of buildings that suffered was about 120: And though many persons were carried to some distance, and others much hurt, being covered in the ruins or in the cellars of their houses, no lives were lost. At the place where this destruction was done, the buildings were pretty thick, amounting to about 150. The general *direction* of the hurricane was from east-south-east to west-north-west. 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 first began till all became still and quiet again.

At the same time considerable damage was done at *Harverhill*, ten miles higher up the river. There the hurricane came on from the south-west. Its first effect was the destruction of a large new barn. The barn containing a large quantity of hay and grain, was crushed down in an instant. The hay, boards and shingles were scattered round to all points of the compass, to the distance of four or five rods from the place where the barn stood; and some of them were carried to the distance of three miles north-east. A large dwelling house at eight or ten rods distance, was much damaged; every board and rib was torn from the roof, and the chimney wrecked to the foundation. Five barns were almost wholly destroyed, and a number of houses and other buildings were much damaged. In some, the windows and doors were drove in; in others, the boards and shingles were ripped off and scattered in all directions. The stone walls in some places were almost 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<sup>h</sup> A. M. stood at  $29,8\frac{1}{2}$  inches. At the time of the hurricane it suddenly fell to  $29,6\frac{1}{4}$ ; and just after to  $29,5\frac{3}{4}$ , which was its least height that day. By noon it rose to  $29,7\frac{1}{2}$ , and at 9<sup>h</sup> P. M. it got up to  $29,8\frac{1}{2}$ ; alterations very common in this part of America. *Fahrenheit's thermometer* in the morning was at  $74^{\circ}$ ; at noon and 9<sup>h</sup> P. M. it was one degree lower: The wind continuing very moderate between west and north-west from 8<sup>h</sup> 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 constantly at south or south-west. The air next to the surface of the earth must therefore have been greatly heated and rarefied; and probably was become specifically lighter than the air in the higher regions. The consequence would be that the lighter rarefied air would ascend, while the heavier condensed air would descend; and in this way the *equilibrium* of the air would be destroyed: To the place where the equilibrium of the air was thus broken, the adjacent air would instantly flow on every side as to a common centre; forming eddies and whirlpools, and thus assuming a *circular motion*. And this it is probable gave rise to those sudden changes, different directions, and violent gusts of the wind. The place where this circular motion began, seems to have been in the upper region of the air, at some distance from the surface of the earth; for several of the effects of the hurricane bear the marks of a *descent*. At the place where it first struck Merrimack river, it hove up the waters, as if some great force had been impressed upon its surface: And at Haverhill, where it was first felt it crushed a barn to pieces as if some immense weight had fallen upon it. When the descending air and whirl came to the earth, being stopped in its descent it seems to have instantly spread itself in to a larger circle or compass, blowing every way from the centre. And hence the pieces of the barn that was crushed down by it, became scattered to the distance of four or five rods all around. A descent of the air in one place would be immediately succeeded with an *ascent* of it in another. And thus the whirlwind, where the air was descending crushed down the buildings before it: But where the air was ascending, lifted up, unroofed, or carried them away; shattering and throwing off the materials thus carried up, as they came to the extremity of the whirl, in tangent lines, to different distances and in all directions. Besides this circular, the whirl-

winds

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.*

DEAR SIR,

William and Mary College, Virginia, November, 1779.

**A** GREEABLY 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 cir-

U

cumstances

cumstances 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 immersed 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 evaporation 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 sitting 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 evaporation

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.			Observations.
				8 H.	12 H.	4 H.	
3	N by E	Clear	30 1 3	73	73½		
4	N b E	Clear	30 1 0	69½	73	74	
5	N b E	Clear	30 0 0	70½	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	80	
17	S W	Clear	29 8 4	77	81	82	
18	S W	Cloudy	29 8 7	79	82	82	
19	N W	Clear	29 9 2	74	75	82	
20	N b W	Clear	29 9 2	74	75	75	
21	N b E	Clear	30 0 5	74	75	74	
22	N b E	Cloudy	30 0 5	73	73	74	
23	S W	Clear	29 9 5	74	77	74	
24	N b E	Clear	29 9 0	80	80	80	
25	S W	Clear	29 9 9	77	82	82	
26	N b E	Clear	29 9 0	80	82	82	
27	S W	Clear	29 9 0	80	81	82	
28	W	Clear	29 3 0	80	81	82	
29	N b W	Clear	30 0 0	78	79	80	
30	N E	Clear	30 1 3	77	78	79	
31		Clear					

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, 1777.

Days.	Winds.	Weather.	Barometer.		Thermometer.		Observations.
			8 H.	12 H.	8 H.	4 H.	
1	S W	Clear	30	1	80	83	<p>Much lightning in the evening.                      Rain with lightning. The barometer was observed to rise before the rain to 30 I O.</p> <p>It was hotter according to the sensation of the human body this evening than had been observed this summer. There was no wind.</p> <p>Much rain and lightning. The point was driven off the Capitol rod, considerably fused.</p> <p>Frequent rain.</p> <p>Rain with lightning.</p> <p>Rain.</p>
2	S W	Clear	30	2	77	84	
3	S W	Clear	30	4	79	85	
4	S W	Clear	30	0	83	88	
5*	S W	Clear	30	0	83	85	
6	S W	Rainy	30	0	81	86	
7	S W	Cloudy	30	0	82	83	
8	S W	Clear	30	0	80	82	
9	S W	Clear	30	2	80	82	
10	S W	Clear	30	2	80	84	
11	S W	Clear	30	1	83	86	
12	S W	Clear	30	1	83	88	
13	S W	Clear	30	1	83	87	
14	S W	Clear	30	1	84	88	
15	S W	Clear	30	1	86 $\frac{1}{2}$	88	
16	S W	Clear	30	1	86	89	
17	W	Clear	30	1	86	91	
18	W	Clear	30	1	84	87	
19	E	Cloudy	30	1	83		
20	W	Clear	30	1	79	83	
21	W	Clear	30	1	82	85	
22	S W	Clear	30	1	82	86	
23	S W	Clear	30	1	85	86	
24	S W	Clear	30	1	82	83	
25	S W	Clear	30	1	83	85	
26	S W	Clear	30	0	80	84	
27	N W	Cloudy	30	0	82	79	
28	N E	Cloudy	30	1	76	76	
29	N E	Clear	30	2	73	75	
30	N E	Clear	30	1	73	74	
31	S W	Clear	30	2	74	76	

SEPTEMBER, 1777.

Days.	Winds.	Weather.	Barometer.	Thermometer.			Observations.
				8 H.	12 H.	4 H.	
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	71	
5	S E	Clear	30 2 5	73	75	74	
6	S W	Clear	30 1 9	72	78	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 2 5	79	81	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	
18	N E	Cloudy	29 9 0	68	67½	67	Rain. 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	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	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	

OCTOBER, 1777.

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

# NOVEMBER, 1777.

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Days.	Winds.	Weather.	Barometer.	Thermometer. 8 H.   12 H.   3 H.	Observations.
1	NE	Clear	30 5 0	56	
2	NE	Cloudy	30 4 8	53 58	
10	W	Clear	30 1 3	42	
11	SW	Clear	29 7 0	42	
12	W	Clear	29 9 0	47	
13	W b N	Clear	30 0 8	41	
15	N	Cloudy	30 0 8	45	
16	N	Cloudy	30 0 9	44	
17	N	Clear	30 0 0	41	
18	N	Clear	30 3 7	37	
19	N	Clear	30 2 9	33	
20	N W	Clear	30 2 4	37	
21	SW	Clear	30 2 4	39	
22	W b S	Clear	30 1 0	54	
23	SW	Clear	30 0 7	55	
24	SW	Clear	30 0 5	57	
25	W	Clear	30 0 3	55	
26	W	Clear	30 0 2	59	
27*	N W	Clear	30 0 0	36	
28	E b S	Cloudy	29 8 4	42	
29†	N W	Snow	29 3 9	41	
30	N W	Rain	29 4 4	39	

The ground frozen.

Frost.

Wind high and cold.

\* Remarkable Aurora Borealis at 7 o'clock this evening. It was terminated towards the east by the two stars  $\beta$  and  $\theta$  in Auriga, and its greatest altitude reached nearly Capella. The stars in the tail of the Great Bear terminated it to the west. It is observable that the barometer was falling from the 18th, and was never observed *to rise* but once, little after the vernal equinox, throughout the whole year, as it was in loon after the Aurora. Its sudden rise was also remarkable. It became cloudy about one o'clock.

† Snow fell  $7\frac{1}{2}$  inches in 24 hours.

DECEMBER, 1777.

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

Snow in the evening.

An Auróra.  
Cloudy evening.

JANUARY, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer.			Observations.
				8 H.	12 H.	3 H.	
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	S 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	
24		Cloudy	29 8 0	47	50	57	
25	S W	Clear	30 1 0	38	42	44	
26	N W	Clear	30 3 2	32	40	43	
27	N W	Cloudy	30 2 0	38	52	57	
28	S W	Rain	29 9 3	61	62	63	
29	S W	Clear	30 3 2	47	55	56	
30	N W	Clear	30 2 0	48			
31	S W	Rain					

## FEBRUARY, 1778.

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

Much rain in the evening.

Wind W at 8.

MARCH,



MARCH, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer.		Observations.
				8 H.   12 H.   3 H.		
1	W.	Clear	29 9 0	36	47	
2	N W	Clear	30 0 6	35	41	
3	E b N	Cloudy	27 6 0	36	32	
4	N W	Clear	30 0 6	22½	35	
5	S W	Clear	30 0 6	23	42	
6	W	Clear	30 2 7	42	49	
7	S W	Clear	30 2 5	47	57	
8	E	Clear	30 2 0	45	46	
9	E	Cloudy	30 1 8	45	48	
10	N	Cloudy	30 1 9	45	47	
11	S W	Cloudy	30 1 5	47	52	
12	S W	Clear	30 0 4	59	69	
13	S W	Clear	30 0 5	68	71	
14	S W	Clear	30 0 6	69	72	
15	S W	Cloudy	30 1 7	65	66	
16	S W	Cloudy	30 0 8	63	74	
17	S W	Clear	29 8 6	67	67	
18	N	Clear	30 2 9	51	51	
19	S	Cloudy	30 1 9	47	54	
20	N W	Clear	29 9 6	50	50	
21	N W	Clear	29 9 4	49	51	
22	N	Clear	30 2 6	39	47	
23	N b E	Cloudy	30 1 0	44	45	
24	E	Clear	30 0 6	45	51	
25	E	Rain	29 9 6	45	47	
26	S E	Rainy	29 9 5	48	57	
27	N	Clear	29 7 1	46	57	
28	N W	Clear	29 8 0	47	56	
29	S E	Rainy	29 3 0	50	54	
30	N W	Cloudy	29 5 0	45	48	
31	N W	Clear	29 0 0	47	49	

Snows very fast.

Peach trees in blossom.

The wind at N W for part of the day.

Winds very high.  
Ditto.

W

APRIL,

APRIL, 1778.

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

M A Y, 1778.

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

JUNE,

JUNE, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer.			Observations.
				8 H.	12 H.	3 H.	
1	S E	Rainy	30 1	65	69	72	
2	N W	Cloudy	30 0	70	75	76	
3	N b E	Cloudy	30 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	65	75	73	
8	N	Clear	29 8 7	65	74	75	
9	N	Clear	30 0 6	65	75	76	
11	W b S	Clear	29 9 9	70	80	84	
12	N E	Cloudy	29 8 0	70	73	74	
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	
22	S W	Clear	29 9 9	76	83	87	
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	86	
28	S W	Clear	29 9 5	80	87	88	
29	S W	Clear	29 9 9	75	86	88	
30	S W	Clear	30 0 0	80	86	89	

Rain.

Rain.  
Much Rain.

JULY,

METEOROLOGICAL OBSERVATIONS. 157

JULY, 1778.

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

X

AUGUST,

AUGUST, 1778.

Days.	Winds.	Weather.	Barometer.	Thermometer.			Observations.
				8 H.	12 H.	4 H.	
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	76	85	85	
9	E	Cloudy	30 0 0	76	78	81	
10	S	Clear	30 0 2	77	77	72	
11	E	Rain	29 7 3	77	77	76	Much rain.
12	N W	Cloudy	29 8 0	72	67	68	
13	S E	Cloudy	29 7 8	66	67	67	Rain.
14	S	Cloudy	29 9 0	68	73	75	
15	S	Cloudy	29 9 9	75	80	82	
16	S	Cloudy	29 9 9	75			
17	S W	Cloudy	30 0 2				

*Description*

## N° XI.

*Description of a Machine for Measuring a Ship's Way through the Sea, by F. HOPKINSON, Esquire.*

Read July  
11, 1783.

**T**HE errors and uncertainties incident to the mariner's log, in common use, are too obvious not to be univervally acknowledged. Were it not for the observations navigators are accustomed to make of a ship's progress, by the apparent passage of the water along side, and the signs that usually present on approaching the land, the log, alone, would be a very unsafe dependence.

Several attempts have, therefore, been made to improve the log, and render its indications more accurate, but without success. All the machines of this kind, that I have heard of, were composed of a number of wheels, which were to be put in motion by the twisting of a line let out astern, having a drag at the end so constructed as to whirl round, faster or slower, according to the motion of the ship.

The objections to a machine, so constructed, are, *First*, If the line is not very long the drag will be considerably affected by the ship's wake; and, if it is very long, the twistings will be irregular, and the line liable to kink. *Secondly*, If the drag is so heavy as to sink below the bottoms of the waves, when the ship sails fast, it will be too heavy, and sink too deep when she hath but a slow progressive motion: Or, otherwise, if the drag is of a proper weight when she makes but little way, it will skip from wave to wave through the air when she sails with a brisk gale, and so be of no use. And, *Thirdly*, It will be liable to most of the other irregularities to which the log in common use is exposed.

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 fails 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 fore-castle, 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 increase 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 immersed 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 increase 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 fore-castle: 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\*.

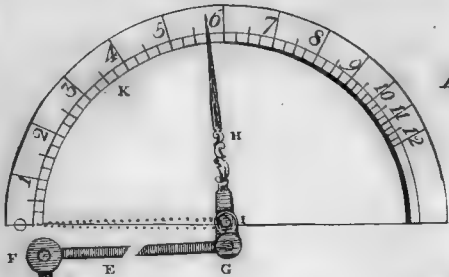
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## N° XII.

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

**S**URINAM 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.



Page. 159.

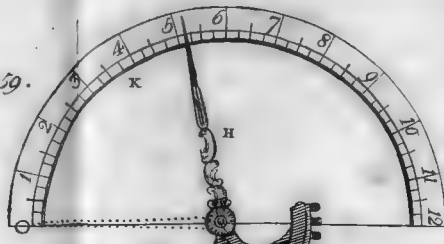


Fig. 1.



Fig. 2.

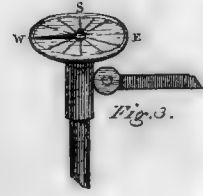


Fig. 3.

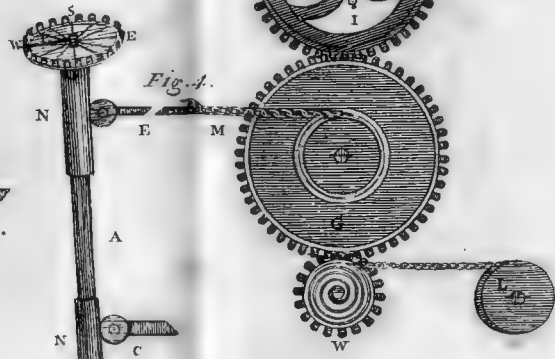


Fig. 4.

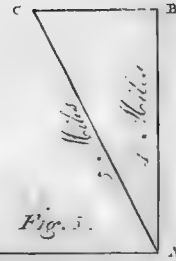


Fig. 5.

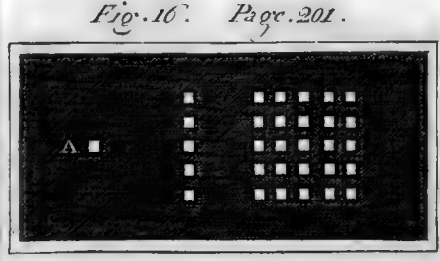


Fig. 16. Page. 201.



Fig. 6.



Fig. 7.

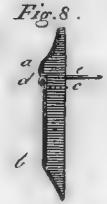


Fig. 8.



Fig. 9.

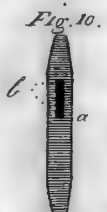


Fig. 10.

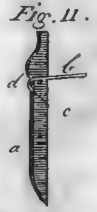


Fig. 11.

Page 185.

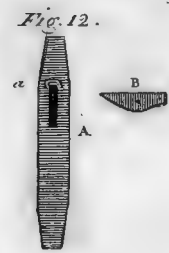


Fig. 12.



Fig. 13.



Fig. 14.

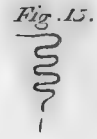


Fig. 15.

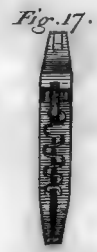


Fig. 17.

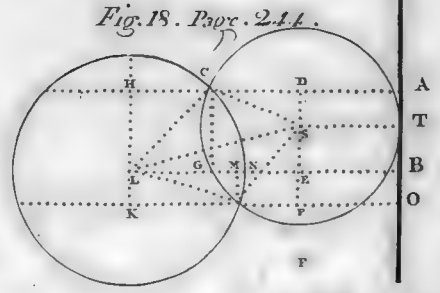


Fig. 18. Page. 211.

J. J. Gayard sculp.

The first part of the history is divided into three books. The first book contains the history of the world from the beginning of the world to the birth of Christ. The second book contains the history of the world from the birth of Christ to the death of the apostles. The third book contains the history of the world from the death of the apostles to the present time.

The second part of the history is divided into three books. The first book contains the history of the world from the beginning of the world to the birth of Christ. The second book contains the history of the world from the birth of Christ to the death of the apostles. The third book contains the history of the world from the death of the apostles to the present time.

The third part of the history is divided into three books. The first book contains the history of the world from the beginning of the world to the birth of Christ. The second book contains the history of the world from the birth of Christ to the death of the apostles. The third book contains the history of the world from the death of the apostles to the present time.



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

glafs bottle, fealing-wax, &c. Yet I cannot obferve the leaft diminution of this quality by placing the tub which contains the fish 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 furrrounding 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 fish 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 fish 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 fish, 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.

## 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 Essequibo. 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. Reaumur 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,

HENRY COLLINS FLAGG.

South-Carolina, }  
October 8, 1782. }

N<sup>o</sup> XIV.

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

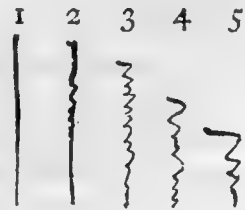
Williamsburg, December 4, 1779.

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  
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\* 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 31<sup>st</sup> of October, at about 6<sup>h</sup> 10<sup>m</sup> 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<sup>m</sup>, 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*.

DEAR SIR,

Philadelphia, January 16, 1780.

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.  $70^{\circ}$  W. nearly, from Philadelphia, and comparing this course with that observed by you, I find it must have fallen on or near the Ouafiota mountains mentioned in Lewis Evans's map, about 480 miles from Philadelphia and 365 from Williamsburg. And taking its altitude  $7^{\circ}$  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  $9^{\circ}\frac{1}{2}$ , 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 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 6.  
7, 1783.

**A**S 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 *lufus nature*. 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.

**A** GREENABLE 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 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 crystal 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 nevertheless little more than conjecture, until confirmed by further experiments.

I am, dear sir, yours, &c.

DAVID RITTENHOUSE.

N° XVII.

*New Method of placing a Meridian Mark, in a Letter to the Rev. Dr. EWING, Provost of the University. By D. RITTENHOUSE, Esquire.*

DEAR SIR,

Read Nov. 1785. **S**OME time ago I mentioned to you a new invention I had for fixing a Meridian Mark for my Observatory. This I have since executed, and as it answers perfectly well, I shall give you a particular description of it.

When my observatory was first erected, I placed a meridian mark to the northward at the distance of about 1200 feet, my view to the south being too much confined by adjacent buildings, and that to the north was not distant enough to have the mark free from a sensible parallax. But last summer a new brick house was built directly north of the observatory, and much too nigh for distant vision with the transit instrument. Now though a fixed mark is not absolutely necessary where you have a good transit instrument, the position of which may be examined and accurately corrected, if necessary, every fair day, by the passage of the pole-star above and below the pole, it is nevertheless very convenient, saves much trouble, and may sometimes prevent mistakes. We have an instance in the observations of the Astronomer Royal at Greenwich. His

A a

mark

mark being taken down at repairing the building to which it was secured, the transit instrument was accidentally thrown out of its true position, and the observations with it were continued for a considerable time before the error was detected. My meridian mark being thus rendered useless, I contrived several other methods of supplying its place, all of which were, on sufficient deliberation, rejected for the following.

I fastened the object glass of a thirty six feet telescope, firmly, to the wall which supports the transit instrument, opposite to and as near as convenient to the object glass of the transit, when brought to a horizontal situation. In the focus of the thirty six feet object glass I screwed fast a piece of brass to a block of marble, supported by a brick pillar built on a good foundation, for this purpose, in my garden. On this piece of brass are several black concentric circles; the rest of the plate is silvered. The diverging rays of light which proceed from every point in these circles, after passing through the thirty-six feet glass become parallel, and entering the transit instrument, an image of the plate and its circles is formed in the same place where the images of stars or the most distant objects are formed. The circles are therefore distinctly seen through the transit, and being placed in the same meridian with the centre of the thirty six feet glass, the innermost circle, about the size of a brevier *c*, serves for a meridian mark, to the centre whereof the cross hair of the transit may be nicely adjusted.

This mark is in several respects preferable to one placed in the common way. It is entirely free from parallax, which the other cannot be, unless placed at a very great distance, when glasses of great magnifying powers are used. It is not sensibly affected by the undulation of the air, which very often renders it impossible to set the transit accurately to a distant mark. And it can be illuminated at night without difficulty, should the suspicion of any  
accident



accident to the tranfit make it neceffary. But it has likewife one difadvantage. Should the pillar in fetting, carry the mark a little to the eaft or weft, the error will be greater in proportion to its nearnefs.

I am, dear fir, your humble fervant,

DAVID RITTENHOUSE.

P. S. The great improvement of object glaffes by Dolland has enabled us to apply eye glaffes of fo fhort a focus, that it is difficult to find any fubftance proper for the crofs hairs of fixed instruments. For fome years paft I have ufed a fingle filament of filk, without knowing that the fame was made ufe of by the European aftronomers, as I have lately found it is by Mr. Hirschell. But this fubftance, though far better than wires or hairs of any kind, is ftill much too coarfe for fome obfervations. A fingle filament of filk will totally obfcure a fmall ftar, and that for feveral feconds of time, if the ftar be near the pole. I have lately with no fmall difficulty placed the thread of a fpider in fome of my instruments, it has a beautiful effect, it is not one tenth of the fize of the thread of the filkworm, and is rounder and more evenly of a thicknefs. I have hitherto found no inconvenience from the ufe of it, and believe it will be lafting, it being more than four months fince I firft put it in my tranfit telescope, and it continues fully extended, and free from knots or particles of duft.

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N° XVIII.

*Account of a Worm in a Horfe's Eye, by F. HOPKINSON, Efquire.*

Read Sep.  
26, 1783.

**H**AVING been myfelf a witnefs to the following curious fact, I thought it fhould

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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 negro. 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, swollen 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<sup>o</sup> XIX.

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

Read Dec.  
5, 1783.

**M**UCH 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,



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 ease in its birth, although not pinched or restrained in any part; *a*, figure 10, represents the tongue in front, and *b*, the mortise, of which the slanting shape cannot be seen in this view; but will be better understood by observing the position of the quill in figure 11, where *a* is a profile of the same tongue, *b*, *c*, the quill lying in the mortise, and *d*, the pin over which the top of the quill passes.

I have need to apologize to the society for directing so much of their attention, to an object which may appear to some to be of little importance. To the musical tribe, however, this improvement will present itself in a different light. Many persons who play very well on the harpsichord, are not able to keep the instrument in order: And to send for a person to repair the quills and tune the instrument as often as it shall be necessary, is not only troublesome and expensive, but such assistance is not always to be had, especially in the country. And for these reasons many a good harpsichord or spinnet lies neglected and the scholar loses the opportunity of practice. To such persons a method of quilling that shall seldom want repair is a *disideratum* of no small importance. And this, I flatter myself I have accomplished.

The difficulty of *quilling* being thus removed, I considered in what manner *tuning* might be made easy to the practitioner in music. Harpsichords are tuned by means of *fifths* and *thirds*; but such is the musical division of the monochord as to make it necessary, that none of these *fifths* or *thirds* should be perfect; an allowance must be made; and to do this with judgment, so that the chords may be good and the instrument 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 harpsichord. To render this task easy, I have procured *twelve tuning forks*, for the *twelve semitones* of the octave; these I had perfectly tuned; and as they will not be sensibly af-

fects 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.

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*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,

effect, causing it to yield a full and pure body of tone, free from all jingle and very pleasant to the ear.

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

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

Figure 13, is the palate in profile, with the tongue properly mounted and moveable on the centre pin. This figure also shews how the palate must be hollowed in behind to expose the root of the tongue, and the small hole in it for the reception of one end of the wire spring.

Figure 14, is a back view of the palate, shewing the groove in which the hair spring of the jack lies, and a small wire staple at *b*, to which the lower end of the steel spring is to be fastened.

Figure 15, is the spring which is to govern the tongue. It must be of fine steel wire, somewhat 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 must play freely within the fork or jaws of the jack, the windings of the spring must not exceed the width of the palate. The upper end of the spring being run through the small hole in the root of the tongue and bent round, so as to secure it, and the superfluous part cut off; the lower end of the same spring must be run under the little staple (*b*, figure 14,) and bent upwards with a gentle strain, so as to hook it on and secure it to that staple; the spring will then operate with all its elasticity, and the tongue will be subjected to its operation.

Figure

Figure 17, represents the palate in a back view with the zig zag spring fastened by one end to the root of the tongue, and by the other to the little staple.

To prevent the tongue from rising by the force of the spring above a horizontal position, there must be a wire staple 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, should be armed with a small piece of soft leather just under the staple, to prevent noise.

It must be left to future experiment to determine the most proper of all substances of which the tongue should be made; different substances drawing different tones from the string. After many essays to this purpose, I have concluded to furnish my harpsichord in the following manner.

The tongues of the first unison are of *Ben sole-leather*. Those of the second are of a soft leather faced with Morocco, such as is frequently used in harpsichords, though applied in a different way, and the tongues of the octave are of wood, such as pear tree, laurel, or any wood of an even grain and not too hard in substance. But all mounted on springs, as above described, and their faces well polished with black lead where they come in contact with the strings.

My reasons are. The sole-leather produces a full, sweet and vigorous tone from the first unison. The second unison, which is the piano of the instrument when the pedal is pressed, is furnished with Morocco leather, which draws a full but more soft and smothered tone from the string. And the octave is struck with wooden tongues for the sake of vivacity or brilliancy, which is the genius of that stop; yet I am not sure but that the octave also had better be struck with sole-leather, like the first unison\*.

A harpsichord thus furnished, will produce a body or quantity of sound, and a purity of tone, that will astonish  
at

\* Because, after the stroke has been given, the wooden tongue repassing the string, 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<sup>o</sup> 4 to n<sup>o</sup> 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.

*Observations on a Comet lately discovered; communicated  
by DAVID RITTENHOUSE, Esquire.*

Read Mar.  
19, 1784.

ON the 21st of January last, John Lukens, Esquire, informed me that he had discovered a comet the preceding evening, and on the evening of the same day, assisted by Mr. Lukens and Mr. Prior, I observed the apparent place of the comet to be in the 15th degree of Pisces, with  $16^{\circ} 6'$  south latitude. By subsequent observations I found its motion to be north easterly, with respect to the ecliptic, and that its nearest approach to us had preceded our first observation. It passed the ecliptic on the 31st in the  $25^{\circ}$  of Pisces, and February the 17th it was in Pisces  $29^{\circ}$  with  $13^{\circ} 10'$  north latitude. This was the last time I saw it, clouds and moonlight having since prevented.

The light of this comet was so very faint that it was impossible to observe it with accuracy, at least without better instruments than I am possessed of, especially as the comet was always involved in day light, moonlight or the thick atmosphere of the horizon. No pains or attention however were wanting, and from the best observations I could make, I find it passed its perihelion about the 20th of January, its distance from the sun being about  $\frac{7}{10}$  of the sun's distance from us. The place of its ascending node is in the 25th deg. of Taurus, and the inclination of its orbit  $53^{\circ}$ . Its motion is retrograde, that is, contrary to the order of the signs. I have still hopes of seeing it in the morning, though its distance is now so very great that it can scarcely be visible 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.

**D** ID 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.”

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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 ochreous 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.

Read Apr.  
16, 1784.

SIR,  
**A**MONG the number of esculent roots, the  
*parsnip* has two singular good qualities.

C c 2

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 autumn; 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 scurvy 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.

## N° XXIV.

*An Optical Problem, proposed by Mr. HOPKINSON,  
and solved by Mr. RITTENHOUSE.*

Philadelphia, March 16th, 1785.

DEAR SIR,

Read Feb.  
17, 1786.

**I** TAKE the liberty of requesting your attention to the following problem in *optics*. It is I believe entirely new, and the solution will afford amusement to you and instruction to me.

Setting at my door one evening last summer, I took a silk handkerchief out of my pocket, and stretching a portion of it tight between my two hands, I held it up before my face and viewed, through the handkerchief, one of the street lamps which was about one hundred yards distant; expecting to see the threads of the handkerchief much magnified. Agreeably to my expectation I observed the silk threads magnified to the size of very coarse wires; but was much surprised to find that, although I moved the handkerchief to the right and left before my eyes, the dark bars did not seem to move at all, but remained permanent before the eye. If the dark bars were occasioned by the interposition of the magnified threads between the eye and the flame of the lamp, I should have supposed that they would move and succeed each other, as the threads were made to move and pass in succession before the eye; but the fact was otherwise.

To account for this phenomenon exceeds my skill in *optics*. You will be so good as to try the experiment, and if you find the case truly stated, as I doubt not you will, I shall be much obliged by a solution on philosophical principles.

I am sir, with great sincerity,  
Your most affectionate friend,  
And very humble servant,

F. HOPKINSON.

*The Answer, by Mr. RITTENHOUSE.*

DEAR SIR,

**T**HE 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 increase these angles, regularly, in a certain ratio; suffering such rays as were parallel

parallel before they enter the telescope to proceed on, parallel, after passing through it. The extended image which we see in this experiment must therefore be formed by pencils of rays, which before they entered the eye, had very considerable degrees of inclination with respect to each other. But coming from a small distant flame of a lamp, they were nearly parallel before they passed through the silk handkerchief. It was therefore the threads of silk which gave them such different directions.

Before the silk is placed to the eye, parallel rays of light will form a single lucid spot, as at A, Plate III. Figure 16. And this spot will still be formed afterwards by such rays as pass through the little meshes uninfluenced by the threads. But suppose 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 side of the original one at A. By a similar 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 square of twenty-five bright spots, separated by four perpendicular dark lines and four horizontal ones; and these lucid spots and dark lines will not change their places on moving the web of silk over the eye parallel to any of its threads. For the point of the retina on which the image shall fall is determined by the incidence of the rays, with respect to the axis of the eye, before they enter, and not by the part of the pupil through which they pass.

In order to make my experiments with more accuracy, I made a square of parallel hairs about half an inch each way. And to have them nearly parallel and equidistant, I got a watchmaker to cut a very fine screw on two pieces of small brass wire. In the threads of these screws, 106 of which made one inch, the hairs were laid 50 or 60 in number. Looking through these hairs at a small opening in the window shutter 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 refrangible 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 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.

**I**T 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 Susquehanna 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 unwholesome 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

D d 2

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 NOT 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 mill-ponds, (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. Ingenhauz, 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.

I. 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 Jamaica 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 island to be buried in a woollen shirt or winding sheet. The law would be much wiser if it compelled every body to wear woollen garments next to their skins during life, and linen after death.

3. The diet in the sickly months should be generous. Wine and beer should be the drinks of this season instead of spirits and water. I do not think that fruit and vegetables of any kind produce fevers, but as the season of the year produces languor and weakness, a larger quantity of animal food than usual is best calculated to oppose them. Salted meat for this reason is preferable to fresh meat. Food of all kinds eaten during the sickly months should be well seasoned.

4. The evening air should be avoided as much as possible. There are at present few places in Pennsylvania where it is safe to sleep, or even to set, after the going down of the sun, in the sickly months, with the windows open. The morning air before the sun rises, should not be breathed, until the body has been fortified with a little solid aliment, or a *draught* of bitters. These bitters should be made of centaury, wormwood, camomile, or the bark of the willow or dogwood trees, infused in *water*. Bitters made with spirits, or even wine, cannot be taken in a sufficient quantity to do service, without producing intoxication, or the deadly habit of loving and drinking spirituous liquors.

5. Too much cannot be said in favour of cleanliness, as a means of preventing fevers. The body should be bathed or washed frequently. It has been proved that in the highlands of Jamaica adding salt to water, renders it more powerful in preventing diseases when applied to the body. Equal pains should be taken to promote cleanliness in every species of apparel. Offal matters, especially those which are of a vegetable nature, should be removed from the neighbourhood of a dwelling house. The dung of domestic animals during its progress 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.

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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 phyto-lacca 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 sore 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 distinctly a number of white particles scattered through it. I suspected at first that they were corrosive sublimate, but the usual tests of that metallic salt soon convinced me that I was mistaken. Recollecting that arsenic was the basis of most of the celebrated cancer powders that have been used in the world, I had recourse to the tests for detecting it. Upon sprinkling a small quantity of the powder upon some coals of fire, it emitted the garlic smell so perceptibly as to be known by several persons whom I called into the room where I made the experiment, and who knew nothing of the object of my enquiries. After this with some 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, discovered an evident whiteness imparted to both of them. One of the pieces afterwards looked like dull silver. These two tests have generally been thought sufficient to distinguish the presence of arsenic in any bodies, but I made use of a third, which has lately been communicated to the world by Mr. Bergman, and which is supposed to be in all *cases* infallible.

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

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

The



1. The use of caustics in cancers and foul ulcers is very ancient, and universal. But I believe *arsenic* to be the most efficacious of any that has ever been used. It is the basis of Plunkett's and probab'y of Guy's well known cancer powders. The great art of applying it successfully, is to dilute and mix it in such a manner as to mitigate the violence of its action. Dr. Martin's composition was happily calculated for this purpose. It gave less pain than the common or lunar caustic. It excited a moderate inflammation, which separated the morbid from the sound parts, and promoted a plentiful afflux of humours to the fore during its application. It seldom produced an eschar; hence it insinuated itself into the deepest recesses of the cancers, and frequently separated these fibres in an unbroken state 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 sound skin. This Dr. Hall proved by confining a small quantity of it upon his arm for many hours. In those cases where Dr. Martin used it to extract cancerous or schirrous tumors that were not ulcerated, I have reason to believe that he always broke the skin with Spanish flies.

2. The arsenic used by the doctor was the pure white arsenic. I should suppose from the examination I made of the powder with the eye, that the proportion of arsenic to the vegetable powder, could not be more than  $\frac{1}{4}$  part of the whole compound. I have reason to think that the doctor employed different vegetable substances at different times. The vegetable matter with which the arsenic was combined in the powder which I used in my experiments, was probably nothing more than the powder of the root and berries of the solanum lethale, or deadly nightshade. As the principal, and perhaps the only design of the vegetable addition was to blunt the activity of the arsenic, I should suppose that the same 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 fore 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 scirrhous 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 prevent

vent the deplorable cafes that have been mentioned, which are often rendered fo by delay or unskilful 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 cafes, where they have been attended with fungous flefh or callous edges, I have ufed the doctor's powder with advantage.

I flatter myself 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 a£tive and ufeul medicines.

N° XXVII.

Illuſtriſſimæ ac celeberrimæ Societati Scientiarum quæ  
eſt Philadelphix.

S. P. D.

CHRISTIANUS MAYER Ser<sup>mi</sup> Electoris Palatini  
Aſtronomus.

**S**CRIBENDI occaſionem a Cl. D. Ferdinando Far-  
mer oblatam eo minus negligendam putavi quod hac  
ratione aliquantum reſpondeam honori, quo me illuſtriſſima  
ſocietas affecit, cum me in album fuorum ſociorum ad  
ſcripſit. Ex libro Philadelphix impreſſo & ad me tribus  
circiter abhinc annis tranſmiſſo intellexi non ſine magno  
animi mei ſenſu, etiam Philadelphix excoli aſtronomiam.  
Libro illo ſcriptiſque meis aſtronicis infelici incendio  
abhinc biennio conſumtis, de novis meis quibuſdam in cœlo  
inventis ad ſocietatem illuſtriſſimam aliquid ſcribendum  
eſſe, duxi. Speculam novam ad omnes uſus accommoda-  
tam:

tam Manhemii inhabito; nec defunt pretiosissima Londinensia instrumenta, in quibus præcipue eminet quadrans muralis æneus 8 pedum in radio a cel. artifice 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 magnitudinis 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 plerumque 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 Britannica 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 fideris. 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 finissima 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.	$\rho$ Leonis Comes 7æ. five 8væ.	2'. 15".	38'. 58". 6. A.	2'. 18".	38'. 50". A.	1690 Die 25 Mar.
1777 28 Feb. item 23 Feb.	Com. $\gamma$ Leonis 6tæ. $\gamma$ Leonis 4tæ	5".	21'. 57". 2. A.	2".	22'. 5".	1691 6 Aprilis.
1777 4 Aprilis.	Comes $\tau$ 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 II. 8væ	1'. 40". 5.	3'. 6". 2. A.	1'. 34".	4'. 40". A.	1691 4 Octo.
1777 13 Nov.	Com. 7mæ & Pifcium	51".	2'. 57". 1. B.	47".	2'. 30". B.	1700 Nov. 13.
1777 Aprilis 6.	Comes $\Pi$ Cancrî	2'. 49".	1'. 40". B.	2'. 44".	1'. 12". B.	1690 Feb. 18.
1777 Aprilis 8.	Com. 7æ $\beta$ Leonis	30".	18'. 27". 1. A.	33' <sup>1</sup> / <sub>2</sub>		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.		
1777 Aprilis 1.	O Cancrî seg. $\Pi$ Cancrî 6.	14'. 56".	32. 29. 6. B.	A. R. 0. 0. 4. 15. 1.	0. 23'. 58". 8. 32. 25. B.	1765 20 Mai. 1691 10 Martii.

Apparet ex omnibus Arcturum omnium celerime 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 correctâ. 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  $49^{\circ} 27' 50''$  Gaudebo maxime si has meas observationes illustrissimæ societati haud ingratas accidisse intellexero, cujus benevolentia me demississime commendo.

Illustrissimæ ac celeberrimæ Societati,  
Cueter et servus perpetuus,  
CHRISTIANUS MAYER, Serenissimi  
Electoris Palatini et Ducis Baviaræ 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 High-  
ness 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, to-

F f together

gether with my own astronomical papers, having been unhappily destroyed by fire about two years ago, I have resolved to give your illustrious society some short account of certain new celestial discoveries which I had made.

My residence is now at Manheim, in a new observatory, fitted for every astronomical purpose; and well furnished with the most precious and accurate instruments made at London; amongst which the chief is a brass mural quadrant of eight feet radius, the workmanship of that celebrated artist Mr. Bird, finished in the year 1775, fitted with an achromatic telescope, and fixed to a solid wall towards the meridian. With this instrument I make daily observations of the heavens, when the weather will permit, and two years ago I distinctly discovered, among many of the *fixed stars* (from the *first* to the *sixth* magnitude) other *concomitant* or *attendant little stars*; some of which, from their mild, faint (or unsparkling) light, have the appearance of planets, while others of them have the appearance of telescopic stars, in respect to their smallness.

But what surprised me most was, that none of these *attendant little stars*, a few perhaps excepted, have ever been noted in any catalogue which I have seen; although I could clearly collect the singular use which may be made of them for ascertaining and determining the proper motion of the fixed stars, as it is called. When the difference of right ascension and declination between two stars is at most but a few seconds, any variation arising from the precession of the equinoxes, the variation of the obliquity of the ecliptic, the deviation of the instrument, the aberration of light or the nutation, or from any other cause depending on the mutable state of the air or latitude of places, must affect them both equally. Therefore when after any length of time a greater variation of right ascension or declination is found in one of such stars than in the other, it affords a certain argument of the proper motion of one or the other, whether that change affects the fixed star 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 Flamsteed 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 Flamsteed'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

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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 Flamsteed'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.	$\rho$ Leonis attendant 7m.	2'. 15".	38'. 58". 6. A	2'. 18".	38'. 50". A	1690 March 25.
1777 Feb. 25, 28.	Attendant 6m. $\gamma$ Leonis 4m.	5".	21'. 57". 2. A	2".	22'. 5".	1691 6 April.
1777 April 4.	Attendant, $\tau$ 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. $\epsilon$ Piscium	51".	2'. 57". 1. B	47".	2'. 30". B	1700 Nov. 13.
1777 April 6.	Attendant, $\Pi$ Cancri	2'. 49".	1'. 40". B	2'. 44".	1'. 12". B	1690 Feb. 18.
1777 April 8.	Attendant 7m. $\beta$ Leonis	30".	18'. 27". 1. A	33' <sup>1</sup> / <sub>2</sub>		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.
1776 May 18.	Arcturus Attendant,	0. 0. 6.	0. 23. 37. 4	MASKELYNE.		1765
1777 April 1.	O Cancri $\Pi$ Cancri 6m.	14'. 56".	52. 29. 6. B	A. R. 0. 0. 4. 15. 1.	0. 23'. 58". 8. 32. 25. B	20 May. 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 Flamsteed'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.

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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.

**D**URING 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 Fahrenheit's thermometer had stood at near  $90^{\circ}$ . 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 supuration. 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

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, forbade 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.

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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.

**T**HE 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

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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 modernised, 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 unfightly 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

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.

**A**S 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.

coasts. They arrive in Georgia and Carolina the latter end of January, and in Virginia in February; and coasting from thence eastward to New-England, they divide and go into all the bays, rivers, creeks and even small streams of water in amazing quantities, and continue spawning in the fresh water until the latter end of April, when the old fish return into the sea, where they change their latitudes by a northward direction and arrive at Newfoundland in May; after which we neither hear or see any thing more of them in America, until their return amongst us the ensuing spring, and bring with them a providential blessing to the poor. Their coming sooner or later up our rivers depends on the warmth or coolness of the season: And it is further observed that if a few warm days invite them up, and cool weather succeeds, it totally checks their passage until more warm weather returns. From all which circumstances 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 distances of the sun. Thus they are found in the British channel in September, but leave it when the sun is at too great a distance from them in the southern hemisphere, and push for a more agreeable climate; and when the weather in America becomes too warm in May, (after having deposited their eggs in shallow water and secured their young fry from the fish of prey,) steer the course which leads to the cooler northern seas, and by that prudent change of place perpetually enjoy the temperature of climate best adapted to their nature; which from the table hereto annexed, shewing the places and times of their visitation, and the calculation of the distance of the sun at those times from them, is that degree of warmth which is produced by the mean distance between 37 to 43 degrees; except whilst they are spawning; during which they bear a greater degree of heat from the necessity of remaining in it a short time to spawn; and also on the other extreme, when detained at too great a distance by the island 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 schools 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  $23^{\circ}$  N. and  $70^{\circ}$  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 route 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 schools 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	10 N.	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 Observations and Reasons given for the course of the Herrings, and the variation in their Mean Distance from the Sun in different months of the year.

[See Map B, Plate V.]

**JANUARY.** In this month the herrings are supposed to be returning from too warm a climate and the approaching sun, from which they retreat fast.

**FEBRUARY.** The time of spawning now drawing nigh, the herrings, in this month pass through the gulph stream, and fall on the coast of America, in order to deposit their spawn in fresh shoal water.

**MARCH.** Now being the beginning of the time of spawning, the largest and strongest fish, which perhaps are the oldest, rush up into the bays, inlets and fresh water streams.

**APRIL.** In this month the lesser, weaker, and perhaps younger fish, rush up even to the heads of small streams, as far as it is possible for them to get, and lay their spawn. These are twice as numerous as the other.

**MAY.** Having been detained by the spawning season, they are overtaken by the sun, and nearer to it now than at any other time; they therefore hasten out of the rivers in this month, and make great way towards the North sea.

**JUNE.** Now having by a rapid progress pushed into a cold climate, on a chilly, icy coast, and the sun beginning to draw towards the south, they whirl round eastward.

**JULY.** The coldness of this sea, and the sun's declination towards the south, now inclines them that way, in which they fall on the Orkneys, and the scoole divides.

**AUGUST.** The grand scoole being divided; now surround the whole island of Great-Britain and Ireland, and are caught on every side.

**SEPTEMBER.** Having been detained the last month by their obstruction amongst the islands, and being harrassed by the fishermen, their mean distance is now the greatest; they collect into one body and hasten to the southward.

**OCTOBER.** Being now under great way, they lessen their mean distance, and by the course which they steer, which perhaps is inclined more westward by the current of the trade wind, they pass the Atlantic.

**NOVEMBER.** Being now more in the trade, and having approached a warmer climate, their motion is supposed to incline more westward.

**DECEMBER.** The sun now beginning to return, they are supposed to incline more northward, to the place where we began; where they are supposed to meet their young fry.

## N° XXXI.

*Observations on a Solar and a Lunar Eclipse, communicated to the Society by M. M. De GRAUCHAIN, Major General of the French Squadron.*

*(Translated from the French.)*

GENTLEMEN,

Newport, 5th December, 1780.

**T**HE study of astronomy having often occupied my leisure during the peace, I could not refuse myself even in the midst of the preparations for war, an opportunity

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, à l'occasion qui fut présentée 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 à fixer l'époque de l'indépendance de l'Amérique l'une des plus intéressantes de l'histoire du genre humain. C'est un motif pour vous en dédier les observations, messieurs, et je rends cet hommage avec le plus grand plaisir à une société illustre dont les membres scavant en même temps éclairent leur patrie par leur connoissances dans les mathématiques et dans la physique, et la servent utilement dans les conseils et dans les armées.

Je suis avec respect, Messieurs,

Votre très humble et très obéissant serviteur,

DE GRAUCHAIN, Major general de l'escadre Françoise.

A Newport le 5 Novembre, 1780.

An

OBSERVATION OF AN ECLIPSE. 241

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,	11	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 $\text{D}$ 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,	45	38		19	8	
The lower limb of the sun at the horizontal,	45	27		21	17	
The preceding limb $\odot$ 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 $\text{D}$ 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 $\odot$ 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,  $41^{\circ} 30' 20''$  N.

Ans:

242 OBSERVATIONS ON TWO ECLIPSES.

An Observation of the Eclipse of the Moon on the 11th of Nov. 1780, at Newport in the State of Rhode-Island.

	Time by the Clock.			True Time.		
	h.	'	"	h.	'	"
The beginning of the Eclipse, - - - -	7	40	5	10	24	39
Immersion of Grimaldi begins, - - - -	7	48	50	10	33	25
Ditto, - - - ends, - - - -	7	51	25	10	36	10
Immersion of Tycho begins, - - - -	8	0	36	10	45	12
Ditto, - - - ends, - - - -	not observed.					
The shadow to Gallileo, - - - -	8	3	42	10	48	18
Immersion of Copernicus begins, - - - -	8	27	54	11	12	31
Ditto, - - - ends, - - - -	8	32	35	11	17	12
The shadow in the middle of Dionysius, - - - -	8	46	55	11	31	33
The shadow 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 Eclipse, - - - -	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 Observation of the Eclipse of the Sun.*

THE clock by which the time was observed, is a pendulum one with a verge of compensation, made by Mr. Parthond, a celebrated clock maker at Paris. It was regulated many days before and after the observation by corresponding altitudes taken with a quadrant of Ramsden, having a radius of one English foot; it is a very good one and well graduated.

The same quadrant served to observe the time when the horns and limbs of the moon and sun arrived at the hori-

Éclaircissements sur l'observation de l'eclipse de Soleil.

LA pendule dont on s'est servi pour obtenir l'heure est une pendule a verge de compensation faite par M. Barthond celebre horloger de Paris, elle a été réglée plusieurs jours avant et apres l'observation par des hauteurs correspondantes prises avec un quart de cercle de Ramsden d'un pied anglois de rayon tres bon, et tres bien divisé.

Le même quart de cercle a servi pour les observations des passages des cornes et des bords du soleil et de la lune au fil horizontal, et au fil vertical de la lunette qui y est adaptée.

L'observateur qui en embarquant des instrumens d'astronomie n'avoit eu pour objet que de régler des montres marines, n'étoit pas aussi 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 été obligé de se servir pour les observations du commencement et de la fin de l'eclipse d'une simple lunette achromatique de mer de quatre pieds de foyer.

Cependant on croit pouvoir répondre de la fin de l'eclipse à quatre ou cinq secondes pres; l'instant du commencement est beaucoup plus incertain; le soleil étoit déjà considérablement entamé, lors qu'on s'en est aperçu; pour conclure au moins grossièrement l'instant vrai du commencement de l'eclipse, on a estimé à peu pres quelle étoit la distance des cornes au moment ou on s'est aperçu que le disque du soleil étoit entamé, et vers la fin de l'eclipse on a examiné combien il s'est écoulé de tems depuis l'instant ou la distance des cornes a été sensiblement la même jusqu'à la fin de l'eclipse. C'est de cette maniere que l'on a jugé qu'il devoit s'être écoulé environ  $1' 20''$  depuis l'instant vrai du commencement de l'eclipse, jusqu'à celui ou on s'est aperçu, quelle étoit commencée.

On a d'abord crû inutile de chercher à observer la grandeur de l'eclipse avec le quart de cercle, à cause de la lenteur du mouvement des deux astres dans le sens vertical, cependant à la réflexion on a pensé que les passages des cornes et des bords du soleil et de la lune au fil vertical seroient suffisoient pour donner les différences de hauteur et d'azimuth des centres des deux astres, et par conséquent leur différence de latitude et de longitude. On les a donc observés vers la fin de l'eclipse, et en même tems on a observé les passages au fil horizontal, mais sans espérer qu'ils pussent être d'un grand secours pour calculer la distance des centres.

Dans la premiere observation le passage de la corne supérieure au fil vertical est un peu douteux. On pense donc qu'il est à propos d'employer de preference dans le calcul de cette observation les passages du bord de la lune, et de la corne inférieure au même fil vertical. De cette maniere on connoitra immédiatement les lignes NB et CO dont la premiere combinée avec les deux diamètres LN et ST donnera LE; différence d'azimuth des centres des deux astres. On obtiendra aussi facilement la différence apparente de hauteur SE 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 differences 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 ob-

Plate III.  
Figure 18.

ervation; 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.

In

culant SF et CM cotes des triangles rectangles CSF et CLM dans les quels on connoit deja les deux autres cotés.

Dans la seconde observation on a omis 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 CA et CO d'ou soustrayant ST, on aura CD et CF connoissant ces deux lignes et le demi-diametre du soleil on calculera les angles CSD; CSF soustrayant leur somme de  $180^{\circ}$ , on aura l'angle CSL partageant cet angle par la moitié on aura l'angle CSL du triangle CLS, on connoitra donc aisement SI, coté de ce triangle et du triangle SLE calculant enfin ce dernier triangle SLE dans le quel on connoit deux angles et un côté on obtiendra LE; difference d'azumuth et SE; difference apparente de hauteur des centres des deux astres.

La dernière observation étant plus complete on pourra la calculer indifféremment de l'une ou l'autre maniere. On pourra même faire usage du passage au fil horizontal pour conclure la difference de hauteur attendu que le mouvement des deux astres dans le sens vertical commençoit à devenir moins lent lorsque cette observation a été faite.

In the second observation was omitted, by inattention, the instant of the arrival of the limb of the moon to the vertical thread, which however may be calculated by the arrival of the horns to the same thread, by which CA and CO are obtained: By subtracting ST from these, the remainder will be CD and CF, having these two lines and the semi-diameter of the sun, the angles CSD, CSF, may be had, and subtracting their sum from  $180^\circ$  the remainder will be the angle CSC, the half of which is the angle CSL; from the triangle CSL you may readily obtain SL, a side of this triangle, and of the triangle SLE; from these at length this last triangle SLE may be calculated, in which are had two angles and one side, from which may be obtained LE the difference of azimuth, and SE 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,

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relatively

Si l'on vouloit dans la premiere observation verifier 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 secours des quantites deja connues. Il fera 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 observés relativement l'un a l'autre, pendant l'espace de tems ecoulé entre leurs passages au même fil.

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 étoit cependant un peu différente 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 wished.

The third of June proved favourable to our wishes. The air was uncommonly clear, and the sky serene. About twenty minutes before the transit, I began to keep my eye steadily fixed on that part of the sun's limb, on which the planet by calculation was to enter; an assistant counting the clock in the mean time, while another stood by to write down the observations. Thus prepared, we waited with a kind of agreeable anxiety for the high satisfaction of seeing Venus on the sun; a satisfaction I had once before enjoyed in viewing the transit of 1761\*, and which I knew must end with that of 1769! The first impression of Venus on the sun, I expected would not appear like a distinct well defined black spot coming on as it were in an instant, but rather like an ill defined mixture of limbs. The event was agreeable to the conjecture, for at 2<sup>h</sup> 30' 14", apparent time, I imagined I saw a *small disturbance* on the sun's limb; but the impression was then so small, irregular and ill defined, that it was not till after several seconds that I was certain the transit was begun. But the impression increasing and growing more distinct, I fixed on the time mentioned above as the time of the *external contact*. To observers with telescopes and eyes equally good, and fixed on that part of the sun on which the planet entered, I conceive this first impression might have been observed to an agreement of 5 or 6 seconds. Though perhaps it might be the contact of the atmosphere, rather than of the body of Venus with the sun.

In about ten minutes after the *external*, I began to look for the *internal contact*. From the form in which Venus appeared, being surrounded with a glimmering light, not very distinctly defined, I concluded it would be difficult if not impossible to fix upon the precise moment when her

I i 2

limb

\* At St. John's, in Newfoundland.

limb would be exactly coincident with that of the sun; and therefore determined to wait till there should appear a small thread of light between them. As the contact drew near, the thread of light began to form, and seemed to dart on each side of the planet for several seconds without being fixed or settled. At  $2^{\text{h}} 48' 44''$ , with a seeming uncertainty of not more than  $7''$  it became closed and fixed; Venus then appeared wholly within the sun, separated from its limb by a fine stream of light flowing gently round it. This I fixed upon as the *internal contact*, though this might also be the contact not of Venus but of her atmosphere with the sun. Not having a *micrometer* or *hairs* fixed in the reflector, instead of making any further observations, we could only enjoy the pleasure of viewing this curious phenomenon, and showing it to a number of gentlemen that had assembled on the occasion.

To determine the latitude of the place, we took the meridian altitude of the sun on the day of the transit, by transmitting his rays from a style 10 feet high, upon a large horizontal platform. From this observation our latitude came out  $43^{\circ} 2'$  north. Suspecting the observation was not sufficiently accurate, I have since carefully examined the matter, and from several observations which nearly agree, I find it to be but  $42^{\circ} 57'$ . With regard to our longitude, the mean of six or seven observations of the eclipses of Jupiter's first satellite, gives it about  $4^{\text{h}} 42' 30''$  west from *Greenwich*.

In the above account of the *contacts*, the duration of the ingress, or passage of Venus over the sun's limb, is  $18' 30''$ ; near a minute longer than in most of the *American* observations. By theory it should be  $18' 56''$ , but as this must have been contracted at the place of observation,  $15''$ , by parallax, the apparent duration of the ingress, would be but  $18' 41''$ ; that is,  $11''$  longer than it was made by observation. I much doubt whether it was possible to discern the planet so soon as  $11''$  after the first

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 completed. 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<sup>h</sup> 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<sup>h</sup> 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 $\frac{1}{2}$ <sup>h</sup>, 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<sup>h</sup> 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.

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N<sup>o</sup> 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.

**O**F 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 for thi.

this purpose, requires the previous knowledge either of the latitude of the place, or hour of the day, at the time of observation; neither of which can, by the common apparatus of a surveyor, be found with sufficient accuracy.

The sun'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 instruments, which surveyors can but seldom command.

That method, which is perhaps the most exact, viz. measuring the time between the passage of two stars which differ considerably, in declination and but little in right ascension, over the same vertical circle, is still farther out of the reach of common surveyors.

The following table of the pole star will, it is presumed, furnish a more easy, and yet sufficiently accurate method of determining this problem; free from all the above inconveniencies, and requiring no difficult calculation, nor any other instrument than the common theodolite, or circumferentor. For though the latitude of the place should not be known within a whole degree, nor the hour of the night within 2 or 3 minutes, this table, by a single observation of the magnetic azimuth or bearing of the pole star, will generally give the variation of the needle true to a single minute of a degree. Nay if the observation be made (as it may be every night) when the star is near its greatest elongation, an error of 10, or even 20 minutes in time will, as is plain from the table, produce little or no sensible error in the azimuth. And as these observations may be repeated at pleasure 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 desired. Besides, 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

K k

nearest

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 westward of the true azimuth, the variation will be westerly; but if to the eastward, the variation will be easterly.

If the time before the star's passage over the meridian be some intermediate minute, or the latitude of the place some intermediate degree, not found in the table, a proportional intermediate azimuth, by means of the differences, must be taken.

The right ascension of the pole star annually increases 10 seconds of time, and its polar distance decreases 20 seconds of a degree, therefore to its present right ascension (in 1785,) viz.  $0^{\text{h}} 49^{\text{m}}$ , must be added one minute every year; and from its present polar distance ( $1^{\circ} 50'.5$ ) one minute must be subtracted, and a proportional part from all the numbers in the columns of azimuth, every three years. The effect of aberration and nutation may be safely neglected; as the error arising from these causes can never amount to more than half a minute of a degree in azimuth.

In computing the sun's right ascension to the time of the star's passage over the meridian nearly, the following little table will be useful.

## T A B L E.

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  $40^{\circ}$  N. the magnetic azimuth of the pole-star had been observed to be  $0^{\circ} 38'$  easterly; required the variation of the needle at the time and place of observation.

Star's

VARIATION OF THE COMPASS. 257

	H.	M.
Star's R. A. increased by 24 hours, -	24	49
Sun's R. A. computed to 1 <sup>h</sup> A. M. (taken from the nautical almanac, or any other table of the sun's R. A.) subtract -	11	25
<hr/>		
True time of star's passage over meridian, reckoned from noon, - -	13	24
Hour of the night subtract, - -	8	0
<hr/>		
Time before star's passage, - -	5	24
Which corresponds to true azimuth, 2° 23' E.		
Magnetic azimuth, - -	0	38 E.
<hr/>		
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, suppose the magnetic azimuth of the pole star, to be 2° 40' east; required the variation of the needle.

	H.	M.
Star's R. A. + 24 <sup>h</sup> , - - -	24	49
Sun's R. A. - - - -	6	54
<hr/>		
Time of star's passing meridian, -	17	55
Time of observation, - - -	10	48
<hr/>		
Time before star comes to meridian, -	7	7
Which corresponds to true azimuth, 2° 4' E.		
Magnetic azimuth, - -	2	40 E.
<hr/>		
Variation, - - -	0	38 E.

## EXAMPLE III.

Latitude of the place  $42^{\circ}$  north, time of observation, January 17th 1785, at  $2^{\text{h}} 40^{\text{m}}$  A. M. Magnetic azimuth,  $1^{\circ} 5'$  easterly.

	H.	M.
Star's R. A. + $24^{\text{h}}$	24	49
Sun's R. A.	20	18
	<hr/>	
Time of star's passing the meridian,	4	48
	24	
	<hr/>	
	28	48
Time of observation reckoned from noon,	14	40
	<hr/>	
Time before star comes to meridian,	14	8
	<hr/>	
Corresponding to true azimuth,	$1^{\circ} 16'$	W.
Magnetic azimuth,	1	5 E.
	<hr/>	
Variation,	2	21 E.

A TABLE



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 $\pi$ lat. 13' N.
October 15th,	7 21 18	$\infty$ 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,	9 <sup>h</sup> 34' 50"	morn.	} Mean Time.
Internal, uncertain,	40 0		
Second internal,	10 51 30		
Last contact,	57 35		
Greatest distance of $\gamma$ center from sun's limb, 31".			

1784,		On Meridian.
Jan. 29th.	$\gamma$ Gemino	9 <sup>h</sup> 49' 20"
	$\epsilon$ Gem.	9 54 45
	New Planet	10 1 48
	$\zeta$ Gem.	10 15 19
Feb. 12th.	$\gamma$ Gem.	8 <sup>h</sup> 54' 19"
	New Planet,	9 4 54
	$\zeta$ Gem.	9 20 18

		On Meridian.		
1785.				
January 26th.	ζ Gemino	10 <sup>h</sup>	24'	14"
	New Planet	10	31	22
	δ Gem.	10	40	7
<hr/>				
February 4th.	ζ Gem.	9	48	48
	New Planet	9	54	32
	δ Gem.	10	4	41

Feb. 12th. Observed zenith distance,  $\left\{ \begin{array}{l} \zeta \text{ Gemino } 19^\circ 5' 7'' \\ \text{New Planet } 16 \text{ } 40 \text{ } 45 \\ \delta \text{ Gem. } 17 \text{ } 35 \text{ } 33 \end{array} \right.$

Febr. 17th.	ζ Gem.	8 <sup>h</sup>	57'	42"	zenith distance, 16° 40' 42"	
	New Planet	9	1	46		ditto, 17 35 40
	δ Gem.	9	13	35		

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	41 $\frac{1}{2}$
	δ Gem.	7	43	6 $\frac{1}{2}$

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	50 $\frac{1}{2}$

Micrometer measure of Z. D.      Difference in min. & seco.

1786,				
January 25th.	δ Gemino	10 <sup>h</sup>	45'	1"
	New Planet	10	56	48
26th.	δ Gem.	10	41	5
	New Planet	10	52	44

	r.	d.
+	1	35
-	4	37 $\frac{1}{2}$

Difference, 6 24 $\frac{1}{2}$  = 20' 59"

		On Meridian.			Micrometer measure of Z. D.		Difference in min. & seco.
		h	m	s	r.	d.	
1786, January 27th.	♁ Gemino	10 <sup>h</sup>	37'	8"	+	1 34	
	New Planet	10	48	37	-	4 34	
					Diff.	6 29	= 21' 17"
31 <sup>st</sup> .	♁ Gem.	10	21	24			
	New Planet	10	32	13			
February 1 <sup>st</sup> .	♁ Gem.	10	17	28	}	Diff.	7 0 = 22 33
	Pluto	10	28	7			
2d.	♁ Gem.	9	16	23½			
	♁ Gem.	10	13	33		8 40	
	New Planet	10	24	2		16 0	
					Diff.	7 8	= 23 5
3d.	♁ Gem.	-	-	-		8 39	
	New Planet	-	-	-		16 3½	
					Diff.	7 12½	= 23 24
6th.	♁ Gem.	9	57	50		8 39¾	
	New Planet	10	7	43		16 17	
					Diff.	7 25½	= 24 7
10th.	♁ Gem.	-	-	-		13 21½	
	♁ Gem.	9	42	7			
	New Planet	9	51	24		16 29½	
					Diff.	3 8	= 10 13
21 <sup>st</sup> .	♁ Gem.	8	58	52			
	New Planet	9	6	46			
March 10th.	♁ Gem.	7	52	2		12 5½	
	New Planet	7	58	31		21 21	
					Diff.	9 15½	= 30 5
11th.	♁ Gem.	6	50	55			
	♁ Gem.	7	48	5		12 5	
	New Planet	7	54	31		21 20¾	
					Diff.	9 15¾	= 30 4
12th.	♁ Gem.	7	44	9		13 17½	
	New Planet	7	50	32		22 34½	
					Diff.	9 17	= 30 9
18th.	♁ Gem.	6	23	23			
	♁ Gem.	7	20	33		13 16¾	
	New Planet	7	26	43½		22 35½	
					Diff.	9 18¾	= 30 16



1786,		On Meridian.	Micrometer mea- sure of Z. D.	Difference in min. & seco.
			r. d.	
March 19th.	♄ Gem.	7 16 37½	14 36	
	New Planet	7 22 47½	24 8½	
			Diff. 9 20½	= 30 24
21st.	♄ Gem.	7 8 46	5 18	
	New Planet	7 14 53	14 37½	
			Diff. 9 19¼	= 30 18
23d.	♄ Gem.	7 0 54½	3 28½	
	New Planet	7 7 1½	12 47	
			Diff. 9 18½	= 30 15
24th.	♄ Gem.	6 56 59	8 14¾	
	New Planet	7 3 6	17 34	
			Diff. 9 19¼	= 30 18
25th.	♄ Gem.	6 53 3		
	New Planet	6 59 16		
27th.	♄ Gem.	6 45 11	8 13¾	
	New Planet	6 51 18	17 31	
			Diff. 9 17¼	= 30 10
April 2d.	♄ Gem.	6 21 36		
	New Planet	6 27 53		

N. B. In these observations the declination of the New Planet was constantly greater than that of ♄ Geminorum, but less than μ.

N° XXXV.

*A Letter from Mr. OTTO, to Dr. FRANKLIN, with a  
Memoir on the Discovery of America.*

New-York, 1st April, 1786.

SIR,

Read Apr.  
7, 1786.

**A**LMOST all the authors who have written upon the discovery of America, make mention of some information which Christopher Columbus procured at Madeira, upon the existence of a western continent; but they do not tell us, positively, how far this in-

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,

O T T O.

His excellency Dr. Franklin..

A New-York, le 1 Avril, 1786.

MONSIEUR,

PRESQUE tous les auteurs qui ont écrit sur la découverte de l'Amérique, font mention de quelques renseignemens que Christophe Colomb s'est procurés dans l'île 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 être utiles, ou quelle en a été la source. J'ai toujours été curieux de débrouiller cette partie intéressante de l'histoire; et en parcourant plusieurs anciens historiens, tant Allemands qu'Espagnols, j'ai trouvé quelques détails qui m'ont paru établir d'une manière indubitable une découverte antérieure à celle de Colomb. J'ai l'honneur de vous en adresser le résumé, et si vous croyez que cette pièce soit digne d'être mise sous les yeux de la société philosophique, je vous supplie de la lui présenter comme une marque de mon hommage et du désir que j'ai de lui être de quelque utilité.

J'ai l'honneur d'être avec un respectueux attachement,

MONSIEUR,

De votre Excellence,

Le très humble et très obéissant serviteur,

O T T O.

S. E. M. FRANKLIN.

*Memoir upon the Discovery of America.*

**I**T has always been looked on as a piece of injustice, not to have given the name of Columbus to that valuable part of the world which he discovered; and that Americanus Vespucius, who did nothing but follow his footsteps, has had the good fortune of having his name handed down to the most distant posterity, to the prejudice of his predecessor. What then will be said, if it shall be proved, that neither of those celebrated navigators were the first discoverers of this immense country, and that this honor belongs to a man scarcely known in the republic of letters. This, however, is what I shall attempt in the following paper; and if the obscurity of contemporary writers and the distance of time, do not afford arguments sufficient for an absolute demonstration, there will however be enough to call in question the pretensions of Christopher Columbus.

I shall not here enter into an examination of the reveries of some historians, on the voyages of the Carthaginians, the Atlantis of Plato, the bold expedition of Madoc prince of Wales and son of Owen Guinnett, of which Hackluyt has preserved some account, nor on the voyages of Bacchus, or the land Ophir of Solomon. Conjectures of this kind, whether true or false, cannot lessen the glory of Columbus, were there not proof that he received, just before

## MEMOIRE SUR LA DECOUVERTE DE L'AMERIQUE. MARS 1786.

**O**N a regardé jusqu'ici comme une injustice qu'on n'ait point donné le nom de Christophe Colomb à la belle partie du monde qu'il a découverte, et qu'Americ Vespuce, qui n'a fait que marcher sur ses traces, ait eu le bonheur de faire passer son souvenir à la postérité la plus éloignée au préjudice de son prédécesseur. Que droit'on s'il étoit prouvé qu'aucun de ces grands navigateurs n'a le mérite de la première découverte de ce pays immense, et que l'honneur en est dû à un homme presque inconnu dans la republique des lettres? C'est ce que je me propose de faire dans ce mémoire, et si l'obscurité des écrivains contemporains et l'éloignement des époques ne me permettent pas de pousser mes arguments jusqu'à l'évidence, ils suffiront au moins pour établir des doutes fondés sur la prétendue découverte de Christophe Colomb.

Nous n'examinerons point ici les rêves de quelques historiens sur la navigation des Carthaginois, sur l'Atlantide de Platon, sur les expéditions hardies de Madox, prince de Galles, et fils d'Owen Guynet, dont Hakluyt nous a conservé les détails, ni sur les voyages de Bacchus ni sur l'Ophir de Salomon; ces conjectures vraies ou fausses ne sauroient diminuer la gloire de  
Christophe

before his expedition, the charts and journal of a learned astronomer who had been in America.

Garcilaffo 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. Garcilaffo says nothing more of this Behenira; and since we know of no Spanish geographer of this name, Garcilaffo 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 Garcilaffo, 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 Garcilaffo; 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.

Garcilaffo de la Vega, né à Cusco en Perou, nous a donné une histoire de sa patrie dans laquelle, pour oter à Colomb le mérite de la découverte de l'Amérique, 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 célèbre géographe nommé Martin Behenira. Garcilaffo ne nous dit rien de plus sur ce Behenira, et comme on ne connoit point de géographe Espagnol de ce nom, on a soupçonné de la Vega d'avoir sacrifié la vérité au desir de ne pas laisser à un Génois la gloire d'avoir découvert le nouveau monde.

En parcourant avec attention la liste de tous les savans du XV siècle, 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 Garcilaffo, que les syllabes *ira* ajoutées à son nom doivent être dues à une circonstance particulière, et cette circonstance je la trouve dans la confiance dont il a été honoré par Jean II roi de Portugal. Il est donc possible que ce Martin Behem soit le même homme que ce Martin Behenira mentionné par Garcilaffo; mais cette conjecture vague aura tous les caractères de l'évidence par les détails suivans.

The literary history of Germany gives an account of a Martin Behem, Beheim, or Behin, who was born at Nuremburgh, an imperial city of the circle of Franconia, of a noble family, some branches of which are yet extant. He was much addicted to the study of geography, astronomy and navigation, from his infancy. At a more mature age he often thought on the possibility of the existence of the Antipodes and of a western continent. Filled with this great idea, he paid a visit in 1459 to Isabella daughter of John 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 Nuremberg, from which the following is copied. “ Martin Behem tender-  
 “ ed his services to the daughter of John king of Lusitania, who reigned after the death of Philip of Burgundy surnamed the Good, and from her procured a ship,  
 “ by means of which, having sailed beyond all the then known limits of the western ocean, he was the first who,  
 “ in the memory of man, discovered the island of Fayal,  
 “ abound-

L'histoire littéraire de l'Allemagne nous apprend que *Martin Behem, Beheim ou Behin* est né à Nuremberg, ville impériale du cercle de Franconie, d'une famille noble dont quelques branches existent encore aujourd'hui. Dès sa plus tendre jeunesse il se livra à l'étude de la géographie, de l'astronomie et de la navigation. Parvenu à un âge mur il réfléchit beaucoup sur la possibilité de l'existence des Antipodes et d'un continent occidental. Rempli de cette grande idée, il 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 découverte de l'île de Fayal. Il y établit une colonie Flamande, dont les descendants existent encore aujourd'hui aux Açores, qu'on a appelées pendant quelque tems les îles Flamandes. Cette circonstance est prouvée, non seulement par les auteurs contemporains, mais par des manuscrits conservés dans les archives de Nuremberg, dont voici la copie : “ Martinus  
 “ Behemus, Joannis Lusitaniae regis filia, quæ post obitum Philippi Burgundi cognomento  
 “ boni, rerum dominabatur, operam suam addixit, et ab ea navim impetravit, qua occidentalis  
 “ oceani hæctenus cognitos terminos et fines prætervectus *primus* post hominum memoriam  
 “ *Fayalem*: insulam, *Fogo* arbore, quam Lusitani *Faye* vocant, ac undè appellatio ei hæbit abundem

“ 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 *Effos* or *Efores* 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 *Azores* dictas (Lusitani enim hoc vocabulo *accipitres* esseferunt, et Galli quod que in aucupis verbum *Effos* et *Efores* 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 Gonsalve Velho, on ne sauroit douter de son authenticité; il se trouve confirmé par plusieurs auteurs contemporains, et surtout par Wagenfeil, un des plus grands favans du dernier siècle, 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 géographie. 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 vécu sous le regne de Jean II. roi de Portugal, dans une île qu'il a trouvée lui même, et qu'il a appelée “ île de Fayal; elle est située aux Açores dans l'océan occidental.”

Après avoir obtenu de la regente Isabelle la propriété de Fayal, et après y avoir employé environ

which time he was busied in making fresh discoveries in geography, by small excursions, 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 south-west. This prince gave him some ships, with which he discovered that part of America, which is now called Brazil; and he even failed to the streights of Magellan, or to the country of some savage tribes, whom he called Patagonians, from the extremities of their bodies being covered with a skin more like a bear's paws than human hands and feet. This fact is proved by authentic records, preserved in the archives of Nuremberg. One of which in particular deserves attention "Martin Behem, traversing the Atlantic ocean for several years, examined the American islands, and discovered the strait which bears the name of Magellan, before either Christopher Columbus or Magellan failed those seas; and even mathematically delineated on a geographical chart for the king of Lusitania, the situation of the coast, around every part of that famous and renowned strait." This assertion is supported by Behem's own letters, written in German and preserved in the archives of Nuremberg, in a book which contains the birth and illustrious actions of the nobility of that city. These letters

M m ters

viron 20 années à faire des recherches ultérieures sur la géographie, dans de petites excursions que ne méritent par d'être rapportées ici; Behem s'adressa en 1484, c'est à dire huit années avant l'expédition de Christophe Colomb, à Jean II. roi de Portugal, pour obtenir de lui les moyens d'entreprendre une grande expédition vers le sud-ouest. Ce prince lui confia quelques vaisseaux avec lesquels il découvrit la partie de l'Amérique connue sous le nom de Brésil, et il étendit même sa navigation jusqu'au détroit de Magellan, ou à la terre de quelques hordes sauvages qu'il appella Patagons, puisque les extrémités de leurs corps couvertes de peaux ressembloient plutôt à des pattes d'ours qu'à des pieds et à des mains. Ce fait est prouvé par des documens authentiques déposés dans les archives de Nuremberg. Il y en a un surtout qui mérite notre attention: "Martinus Behemus per oceanum Atlanticum huc illic annos plures oberrans, ante Christophorum Columbum Americæ insulas, ante Fernandum Magellanum, fretum quod ab eo cognomentum habet pervestigavit, unde et in tabula geographica longe prius quam Magellanus de expeditione sua cogitasset, omnem circa insigne clarissimumque fretum illud oræ habitudinem Lusitanæ regi radio delineavit." Cette assertion se trouve appuyée par des lettres de Behem écrites en Allemand, et conservées dans les archives de Nuremberg, 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 southward beyond the straits of Gibraltar. He gave the command 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 sailing, 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 eastward, 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 découverte 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 Nuringberga superioris Germaniæ de Bonna familia natum, hominem inquam in cognoscendo situ terræ peritissimum, marisque patientissimum, qui Ptolomæi longitudes et latitudes in occidente ad unguem experimento longævaque navigatione novit. Hi duo, bono Deorum auspicio mare meridionale fulcantes a littore non longe evagantes superato circulo a quinoctiali in alterum orbem excepti sunt, ubi ipsis stantibus orientem versus umbra ad meridiem et dexteram projiciebatur. Aperuere igitur sua industria alium orbem bætenus 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 æris impatientiam mortuis."

Ce qui



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 frequent, under the reign of Henry son of John king of Lusitania. After his death, Alphonfus the fifth prosecuted the design, and John who succeeded him followed the plan of Alphonfus 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 farther south, until he arrived at the remotest strait, beyond which, Ferdinand Magellan, following his tract, afterwards failed and called it after his own name."

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All

Ce qui rend ce passage plus intéressant 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 inséré dans le corps de cette histoire. Nous trouvons d'ailleurs dans les notes que Petrus Matæus a faites sur le droit canon, deux ans avant l'expédition de Colomb, les détails suivans: "Primæ navigationes christianæ ad novas insulas elucescere ceperunt sub Henrico Johannis Lusitanæ regis filio, &c. illo mortuo Alphonfus V. capta prosecutus est; Alphonsum Johannes imitatus opera *Martini Bobemi*, hominis in cursu navium peritissimi, ut brevi tempore nihil celebrius per totum orbem audiretur ipso Lusitanæ nomine." Cellarius, un des plus grands savans de son siècle, dit expressément: "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."

Toutes

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 Nuremberg. 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 Alphonfus 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 presque inconnu, paroissent démontrer que la première découverte de l'Amérique est due aux Portugais, et non aux Espagnols; et que c'est un astronome Allemand qui étoit à leur tête. L'expédition 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 côtes de l'Amérique tracée par Behem, et conçut dès lors le projet hardi de suivre la route de ce grand navigateur. Jerome Benzon, qui a donné en 1550, une description de l'Amérique, fait mention de cette carte, dont on a conservé une copie dans les archives de Nuremberg, où Behem l'avoit envoyée lui-même; le célèbre astronome Riccioli, qui étoit lui-même Italien, ne paroît pas cependant vouloir attribuer à son compatriote cette importante découverte; il dit dans sa géographie réformée, livre 3. p. 90. “ Christophorus Columbus, cum  
 “ prius in Madera insula, ubi consiciendis et delineandis chartis geographicis vacabat, indicio ba-  
 “ bito a Martino Bohemo, aut ut Hispani dicunt ab Alphonso Sanchez de Huelva, nauclero  
 “ qui forte incidit in insulam, postea Dominicanam 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 Magellan, 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 Allafavas, 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, Africa and Guinea; and his chief squire was the king himself, 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 celebratur quantum 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 manière la plus solennelle, et en présence de toute sa cour. J’ai sous mes yeux un document Allemand, tiré des archives de Nurenberg, de la teneur suivante: “ En 1485, le 18 Fevrier, en Portugal dans la ville d’Aliafavas, 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 étoit le roi lui-même, qui mit son épée à son ceinturon; et le duc de Begia étoit son second ecuyer qui lui mit son épéron droit; et son troisième ecuyer étoit le comte Christophe de Mela cousin du roi, qui lui mit son épéron gauche; et son quatrième ecuyer étoit le comte Martini Marbarinis, qui lui mit son casque de fer; et le roi lui donna lui-même un coup sur l’épaule; ce qui se passa en présence de tous les princes et de tous les seigneurs et chevaliers du royaume; et il a épousé la fille d’un grand seigneur

“ 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 ter-  
 restrial 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 set out  
 on his expedition, from whence it is not possible that  
 Behem could have profited by the works of this naviga-  
 tor, who besides, went a much more northerly course.

After having performed several other interesting voy-  
 ages, 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'île 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'où 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  
 même 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 Nu-  
 renberg, 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 bibliothèque de cette  
 ville. On y voit la trace de ses decouvertes sous le nom de terres occidentales, et par leur si-  
 tuation on ne peut disconvenir qu'elles ne soient les cotes actuelles du Brésil et les environs du  
 détroit de Magellan. Ce globe est fait dans la même année où Colomb a commencé son  
 expedition; il est donc impossible que Behem ait profité du travail de ce navigateur, qui d'ail-  
 leurs a dirigé sa course beaucoup plus au nord.

Après avoir achevé plusieurs autres voyages intéressans, le chevalier Behem mourut à Lis-  
 bonne

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 Nuremberg 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 écrits de Ptolomée, de Plin, 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 sur 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 Nuremberg se glorifie d'avoir vu naître dans ses murailles. Il est vraisemblable qu'au moment où il fut créé 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 ressemblance 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 auroit dû 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 Alvarez 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 inconnus 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 prêter 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, tandis que 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 découvertes de Behem, est donc plutôt 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 après l'expédition de Vasco de Gama dans l'Inde, qu'Emanuel jugea à propos de tirer parti des découvertes de Behem, en envoyant au Bresil Alvarez 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 nevertheless, 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

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is

le Portugal et l'Espagne, que du desir de faire des établissemens avantageux aux quels l'Inde étoit 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'île 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'île de St. Brandon, une terre qui paroît 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 Nuremberg 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 île 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 même 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 à Nuremberg, 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 île fabuleuse; nous pouvons supposer avec autant de fondement, 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 île 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'expédition 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:

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I. Before

Lieu en 1483, il est donc possible qu'à son retour Behem ait projeté 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 déférence 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 différent du sien sans nous rendre coupable de présomption.

Mais, dira-t-on, 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 événement qui a eu lieu à l'extrémité 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 répondre à toutes ces questions je soumettrai au lecteur impartial les remarques suivantes:

I. 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 concevoir la possibilité de l'existence d'un nouveau continent, qu'à chercher des terres dans une region où il étoit 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 singulièrement diminuée.

2. Toutes les preuves historiques que nous avons données 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 ennemis de Colomb, et de Behem lui-même.

3. On fait qu'avant Charles quint il y avoit très peu de communication entre les savans des différentes nations. Les écrivains 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 étoient inconnus, et les savans étoient obligés de voyager pour voir de leurs propres yeux les progrès de leurs voisins. L'Italie étoit le centre des arts et de ce qu'on appelloit science dans ce tems-là. Les fréquens voyages des empereurs d'Allemagne à Rome leur donna la facilité de connoître des gens de merite, et de les placer dans les différentes universités

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 scrap-  
ing

de l'empire. C'est à cette circonstance que l'on doit attribuer les grands progrès que les Allemands ont faits surtout dans les mathématiques, depuis le XIV. jusqu'au XVI. siècle; 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 différens princes avec les puissances étrangères, leur donnoient une grande facilité de rassembler, dans leurs archives, les pièces originales des événemens les plus importans de l'Europe. C'est à cet esprit de critique et de recherche qu'est due en grande partie la réformation de Luther, et on ne peut se dissimuler, que, surtout dans le XV. siècle, 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 impériales des détails sur une expedition, projeté sur les bords du Tage par un Allemand, par un homme très considéré dans son pays, et dont par conséquent toutes les démarches devoient intéressantes.

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 découvertes les plus importantes, il faut en chercher le motif dans sa cupidité, et non dans son désir 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 chev. 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 chev. 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, tandis que 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 très 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'exécution 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 témoins. Ce qui doit nous confirmer dans cette opinion, c'est que *Mariana* dit lui-même (livre 26. chap. 3.) qu'un *certain* bâtiment 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

lumbus 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 Vespuccius, 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 général, que ce savant avoit eu quelques renseignemens sur des terres occidentales, mais ils ne nous en parlent que d'une manière très vague. L'expédition du chevalier Behem nous explique ce mystère.

6. Cet astronome ne pouvoit être jaloux des découvertes 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, ou pouvoit croire que le pays trouvé par Colomb n'avoit aucun rapport avec celui de Behem.

Quoiqu'il en soit, il paroît certain que Behem a découvert 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 premières fondateurs, ils leur ont élevé des autels; pourquoi Behem, Christophe Colomb et Vespucc ne meritoient-ils pas des statues sur les places publiques des villes Americaines? Ces monumens précieux transmettoient à la posterité la reconnaissance que doivent inspirer les noms de ces bienfaiteurs du genre humain. Sans le savoir ils ont jeté les fondemens du bonheur de plusieurs millions d'habitans; et Sesostris, Phul, Cyrus, Theseus, 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.

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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.*

**H**AVING 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 mat-

ter 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.

## D I A B E T E S.

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

## T H E R E M I T T E N T F E V E R.

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



fician, 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.

#### B E L L Y - A C H.

The belly-ach with inflammatory symptoms has frequently occurred 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 foetid 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 foetid. 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 diarrhoea 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 diarrhoea 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,

S I R,

Your most humble servant,  
WILLIAM WRIGHT.

N<sup>o</sup> 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. DUCHE.

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 fits 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 stomach and bowels braced up, and, by the interposition of opiates, the spasms were removed.

Having sent several specimens of the bark for a trial to different parts of the continent of America, and particularly to my worthy friend Doctor Hall of Peterburgh Virginia, I impatiently wait the result of your trials, and will esteem myself particularly obliged by your communication. If you chuse, I shall send you some of the young trees planted in tubs, with some of the seeds.

Should it be found to answer my expectation, the pleasure resulting from the thoughts of having communicated something useful, will be to me ample enough recompense. I have the honor to be,

With the utmost respect,

Your most obedient humble servant,

GEORGE DAVIDSON.

P. S. Dr. Wright of Jamaica (in fifth vol. of medical commentaries,) describes a species of Cinchona, with only one flower on a footstalk; the same was likewise found at the Havanna. It differs in that particular from the old bark, which resembles the St. Lucia bark, in having several flowers on each footstalk.

*The following is a Description of the CINCHONA CARIBÆA SANCTÆ LUCIÆ.*

The tree is commonly found in ravines, near springs, under the shade of a larger tree. It delights in places well shaded, and defended from the north-east trade wind. The soil is commonly a stiff red earth with a clayey substratum; quantities of small beautiful crystals, of a regular angular form, are found intermixed.

The tree is about the size of the cherry tree; seldom exceeding the thickness 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.

**Y**OUR 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 F F. 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 I.

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 E E E, 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-

menfions of the great fail, and expofing a quarter of its furface to the wind, would give a quarter of the force; fo that the whole force obtained from the wind would be the fame, while the refiftance from the air would be nearly reduced to the fpace between the pricked lines *ab* and *cd*, before the foremoft fail.

It may perhaps be doubted whether the refiftance from the air would be fo diminifhed; fince poffibly each of the following fmall fails having alfo air before it, which muft be removed, the refiftance on the whole would be the fame.

This is then a matter to be determined by experiment. I will mention one that I many years fince made with fuccefs for another purpofe; and I will propofe another fmall one eafily made. If that too fucceeds, I fhould think it worth while to make a larger, though at fome expence, on a river boat; and perhaps time and the improvements experience will afford, may make it applicable with advantage to larger veffels.

Having near my kitchen chimney a round hole of eight inches diameter, through which was a constant fteady current of air, increafing or diminifhing only as the fire increafed or diminifhed, I contrived to place my jack fo as to receive that current; and taking off the flyers, I fixed in their ftead on the fame pivot a round tin plate of near the fame diameter with the hole; and having cut it in radial lines almoft to the centre, fo as to have fix equal vanes, I gave to each of them the obliquity of forty-five degrees. They moved round, without the weight, by the impreffion only of the current of air, but too flowly for the purpofe of roafting. I fufpected that the air ftruck by the back of each vane might poffibly by its refiftance retard the motion; and to try this, I cut each of them into two, and I placed the twelve, each having the fame obliquity, in a line behind each other, when I perceived a great augmentation in its velocity, which encouraged me to divide

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.

AB is a long boom, from which are hoisted seven jibs, a, b, c, d, e, f, g, each a seventh part of the whole dimensions, and as much more as will fill the whole space when set in an angle of forty-five degrees, so 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 should be brought at right angles with the keel, by means of the sheet ropes CD, and all the sails hauled flat to the boom.

These positions of boom and sails 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 sails trimmed according as the wind is more or less against your course.

It seems to me that the management of a shallow so rigged would be very easy, the sails being run up and down separately, so that more or less sail may be made at pleasure; and I imagine, that there being full as much sail exposed to the force of the wind which impells the vessel in its course, as if the whole were in one piece, and the resistance of the dead air against the fore-side of the sail being diminished, the advantage of swiftness would be very considerable; besides that the vessel would lie nearer the wind.

Since we are on the subject of improvements in navigation, permit me to detain you a little longer with a small relative observation. Being, in one of my voyages, with ten merchant-ships under convoy of a frigate at anchor in Torbay, waiting for a wind to go to the westward; it came fair, but brought in with it a considerable swell. A signal was given for weighing, and we put to sea all together; but three of the ships left their anchors, their cables parting just as the anchors came a-peak. Our cable held, and we got up our anchor; but the shocks the ship felt before the anchor got loose from the ground, made me reflect on what might possibly have caused the breaking  
ing

ing of the other cables; and I imagined it might be the short bending of the cable just without the haufe-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 haufe-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 haufe-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 haufe-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 cruise 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 cruisers, 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 overfetting 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 packet 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 opposed to each other perfectly parallel, though the other sides are formed as in common thus, figure 10.

The building of a double ship would indeed be more expensive in proportion to her burthen; and that perhaps is sufficient to discourage the method.

The accident of fire is generally well guarded against by the prudent captain's strict orders against smoking 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 sufficient to abolish; that of carrying store-spirits to sea in casks. Two large ships, the *Seraphis* and the *Duke of Athol*, one an East-Indiaman, the other a frigate, have been burnt within these two last years, and many lives miserably destroyed, by drawing spirits out of a cask near a candle. It is high time to make it a general rule, that all the ship's store of spirits should be carried in bottles.

The misfortune by a stroke of lightning I have in my former writings endeavoured to show a method of guarding against, by a chain and pointed rod, extending, when run up, from above the top of the mast to the sea. These instruments are now made and sold at a reasonable price by *Nairne and Co.* in London, and there are several instances of success attending the use 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 gust.

Of the meeting and shocking with other ships in the night, I have known two instances in voyages between London and America. In one both ships arrived though much damaged, each reporting their belief that the other must have gone to the bottom. In the other, only one got to port; the other was never afterwards heard of. These instances happened many years ago, when the commerce between Europe and America was not a tenth part of what it is at present, ships of course thinner scattered, and the chance of meeting proportionably less. 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 circumference

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 inertiae* 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 propose the addition of another such L pipe, and that they should stand back to back in the boat thus, figure 13. the forward one being worked as a pump, and sucking in the water at the head of the boat, would draw it forward while pushed in the same direction by the force at the stern. And after all it should be calculated whether the labour of pumping would be less than that of rowing. A fire-engine might possibly in some cases be applied in this operation with advantage.

Perhaps this labour of raising water might be spared, and the whole force of a man applied to the moving of a boat by the use of air instead of water; suppose the boat constructed in this form, figure 14. A, a tube round or square of two feet diameter, in which a piston may move up and down. The piston to have valves in it, opening inwards to admit air when the piston rises; and shutting, 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 piston is forced down, and let the air pass out at E, which striking forcibly against the water abaft must push the boat forward. If there is added an air-vessel F properly valved and placed, the force would continue to act while a fresh stroke is taken with the lever. The boatman might stand with his back to the stern, and putting his hands behind him, work the motion by taking hold of the cross bar at B, while another should steer; or if he had two such pumps, one on each side of the stern, with a lever for each hand, he might steer himself by working occasionally more or harder with either hand, as watermen now do with a pair of sculls. There is no position in which the body of a man can exert more strength than in pulling right upwards.

To obtain more swiftness, greasing the bottom of a vessel is sometimes used, and with good effect. I do not know that any writer has hitherto attempted to explain this. At first sight one would imagine, that though the friction of a hard body sliding on another hard body, and

the resistance occasioned by that friction, might be diminished by putting greafe between them, yet that a body sliding on a fluid, such as water, should have no need of nor receive any advantage from such greasing. But the fact is not disputed. And the reason perhaps may be this. The particles of water have a mutual attraction, called the attraction of adhesion. Water also adheres to wood, and to many other substances, but not to greafe: On the contrary they have a mutual repulsion, so that it is a question whether when oil is poured on water, they ever actually touch each other; for a drop of oil upon water, instead of sticking to the spot where it falls, as it would if it fell on a looking-glass, spreads instantly to an immense distance in a film extremely thin, which it could not easily do if it touched and rubbed or adhered even in a small degree to the surface of the water. Now the adhesive force of water to itself, and to other substances, may be estimated from the weight of it necessary to separate a drop, which adheres, while growing, till it has weight enough to force the separation and break the drop off. Let us suppose the drop to be the size of a pea, then there will be as many of these adhesions as there are drops of that size touching the bottom of a vessel, and these must be broken by the moving power, every step of her motion that amounts to a drop's breadth: And there being no such adhesions to break between the water and a greased bottom, may occasion the difference.

So much respecting the motion of vessels. But we have sometimes occasion to stop their motion; and if a bottom is near enough we can cast anchor: Where there are no foundings, we have as yet no means to prevent driving in a storm, but by lying-to, which still permits driving at the rate of about two miles an hour; so that in a storm continuing fifty hours, which is not an uncommon case, the ship may drive one hundred miles out of her course; and should she in that distance meet with a lee shore, she may be lost.

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 purpose have occurred to me, which though not so simple as the above, I imagine would be more effectual, and more easily manageable. I will endeavour to describe them, that they may be submitted to your judgment, whether either would be serviceable; and if they would, to which we should give the preference.

The first is to be formed, and to be used in the water on almost the same principles with those of a paper kite used in the air. Only as the paper kite rises in the air, this is to descend in the water. Its dimensions will be different for ships of different size.

To make one of suppose fifteen feet high; take a small spar of that length for the back-bone, AB, figure 16, a smaller of half that length CD, for the cross piece. Let these be united by a bolt at E, yet so as that by turning on the bolt they may be laid parallel to each other. Then make a sail of strong canvas, in the shape of figure 17. To form this, without waste of sail-cloth, sew 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 so as to place its broad part opposite to that of the piece G, and the piece H in like manner opposite to I, which when all sewed together will appear as in figure 17. This sail is to be extended on the cross of figure 16, the top and bottom points well secured to the ends of the long spar; the two side points d, e, fastened to the ends of two cords, which coming from the angle of the loop (which must be similar to the loop of a kite) pass through two rings at the ends of the short spar, so as that on pulling upon the loop the sail 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 ballast 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 surface;  
this

this rope long enough to permit the kite's descending into the undertow, or if you please lower into still water. It should be held by a hauser. To get it home easily, a small loose rope may be veered out with it, fixed to the keg. Hauling on that rope will bring the kite home with small force, the resistance being small as it will then come endways.

It seems probable that such a kite at the end of a long hauser would keep a ship with her head to the wind, and resisting every tug, would prevent her driving so fast as when her side is exposed to it, and nothing to hold her back. If only half the driving is prevented, so as that she moves but fifty miles instead of the hundred during a storm, it may be some advantage, both in holding so much distance as is saved, and in keeping from a lee shore. If single canvas should not be found strong enough to bear the tug without splitting, it may be doubled, or strengthened by a netting behind it, represented by figure 20.

The other machine for the same purpose, is to be made more in the form of an umbrella, as represented, figure 21. The stem of the umbrella a square spar of proper length, with four moveable arms, of which two are represented C, C, figure 22. These arms to be fixed in four joint cleats, as D, D, &c. one on each side of the spar, but so as that the four arms may open by turning on a pin in the joint. When open they form a cross, on which a four square canvas sail is to be extended, its corners fastened to the ends of the four arms. Those ends are also to be stayed by ropes fastened to the stem or spar, so as to keep them short of being at right angles with it: And to the end of one of the arms should be hung the small bag of ballast, and to the end of the opposite arm the empty keg. This on being thrown into the sea, would immediately open; and when it had performed its function, and the storm over, a small rope from its other end being pulled on, would turn it, close it, and draw it easily home to the ship. This machine seems more simple 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 north-easter 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.  $40^{\circ}$  north, to a place in lat.  $50^{\circ}$  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

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\* Since this paper was read at the Society, an ingenious member, Mr. Patterfon, 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 hammered,



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. Ruffs. 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 sloping, 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 cabin 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 wholesome 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 risked 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	SSE	WbS		37 39	60 38	Much gulph weed; saw a whale.
12			64	SW	WNW		37 13	62 29	Colour of water changed.
13			65				37 48	64 35	No gulph weed.
14			65						Sounded, no bottom.
14			70	N	W	34	37 26	66 0	Much light in the water last nig.
27	8A.M.	60	60	NE	WbS	44			Water again of the usual deep sea colour, little or no light in it at night.
28	6P.M.	70	67	NW	WbS	57			Frequent gulph weed, water continues of sea colour, little light.
29	8A.M.	63	71	NW	WbN	69			Much light.
	5P.M.	65	72	NW	WbN	24	37 20	68 53	Much light all last night.
	11 dit.	66	66	NE	EbS	43			Colour of water changed.
30	8A.M.	64	70	NE	WbN	60			
	12	62	70	ESE					
	6P.M.	64	72	S					
	10 dit.	65	65						
	7A.M.	68	63						
May 1	12	65	56	SSW	WNW	44	38 13	72 23	
	4P.M.	64	56	SW	WbN	21			
	10 dit.	64	57		WNW	31			
	8A.M.	62	53			18	38 43	74 3	
2	12	62	53	WSW	NW	18			
	6P.M.	64	55	NW	WSW	15			
	10 dit.	65	55	NbW	WbN	10			
	7A.M.	62	54			30	38 30	75 0	Much light. Thunder-gulf.

Observations of the warmth of the sea-water, &c. by Fahrenheit's thermometer; with other remarks made on board the Reprisal, 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.	Course.	Distance.	Lat. N.	Long. W.	Remarks.
Octo. 31	10	4	76	70	SSE	E b S	135	38 12	70 30	Left the capes Thursday night, October 29, 1776.
Nov. 1	10	4	71	71	WSW	E $\frac{1}{2}$ N	109	No ob.	68 12	
2	8	4	71	78	N					
3	8	4	67	76	NW	ESE $\frac{1}{2}$ E E b S	141	ditto.	65 23	Some sparks in the water these two last nights.
4	9	4	70	76		N b E	194	37 0	62 7	
5	8	4	68	76		NE	163	35 21	58 8	Ditto.
6	8	4	75	75		S 50 E	75	35 33	53 52	Ditto.
7	8	4	77	77	E b N	N 30 W	108	36 6	52 46	
8	9	4	75	77	S b E	N 49 E	175	38 2	50 1	
9	9	4	75	77	SW	N 33 E	175	39 39	46 55	



Observations made on board the Reprifal, continued.

Date.	Hour A. M.	Hour P. M.	Temp. of Air.	Temp. of Water.	Wind.	Course.	Distance.	Lat. N.	Long. W.	Remarks.
Novem. 9	8	4	70	71	E	N 17 E	64	40 39	46 27	
10	12			64						
11	8			63						
12	12			61	SE	N 8 E	41	41 19	46 19	
12	8		56	59	NNW	N 80 E	120	41 39	43 42	
13	all day	4		69	E	S 82 E	69	41 29	42 10	
14	8	Noon	70	70	ESE	N 74 E	111	42 0	39 57	
15	8	4	61	71	WSW	N 70 E	186	43 3	35 51	
16		Noon	65	69	SW	N 67 W	48	43 22	34 50	
17	8	4	63	67	ESE	N 19 E	56	44 15	34 25	
18	all day		65	63	S b W	N 75 E	210	45 6	29 43	
19	Noon	Noon	65	64	SW	N 80 E	238	45 46	24 2	Some gulph weed.
20	8		62	64	N	S 80 E	155	45 19	20 30	
21	9	4	60	60	S	N 88 E	94	45 22	18 17	
22	10		60	62	SSW	S 89 E	133	45 19	15 19	
23		Noon		61	WSW	S 86 E	194	45 6	10 35	
24	do.	do.		60	NNE	N 78 E	191	45 46	6 10	
25	do.	do.	56	60	NE	S 76 E	125	45 4	3 23	
26	do.	do.	58	60	E	N 73 E	31	45 13	2 20	Soundings off Bellise.
27	do.	do.	54	58						
28	do.	do.	54	56						



## 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.  $32\frac{1}{2}$  deg. St. Mary's S W  $\frac{1}{2}$  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 faw gulph weed.

—21. Southerly current.

—22. Again faw 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 beds of rock-weed; and we were surpris'd 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 foundings, 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 foundings 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.*

## N° XXXIX.

*Two Hearts found in one Partridge.*

**A**LL 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<sup>a</sup>. 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.

**T**OUS les faits de la nature sont liés les uns aux autres, et forment un tout, dans l'immensité duquel nous n'apercevons que quelques points, qui nous paroissent isolés parce que ceux qui les unissent nous sont cachés. Il résulte de cet enchaînement 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 paroît le plus futile doit être recueilli comme les autres; en le saisissant on doit être assuré qu'on tient une chaîne ou le temps découvrira des chaînons précieux<sup>a</sup>. Si ceux qui manquent y laissent des lacunes, ces chaînons intermédiaires nous sont tous les jours présentés par la main du hazard, et celle du naturaliste les met à leurs places. Offrons

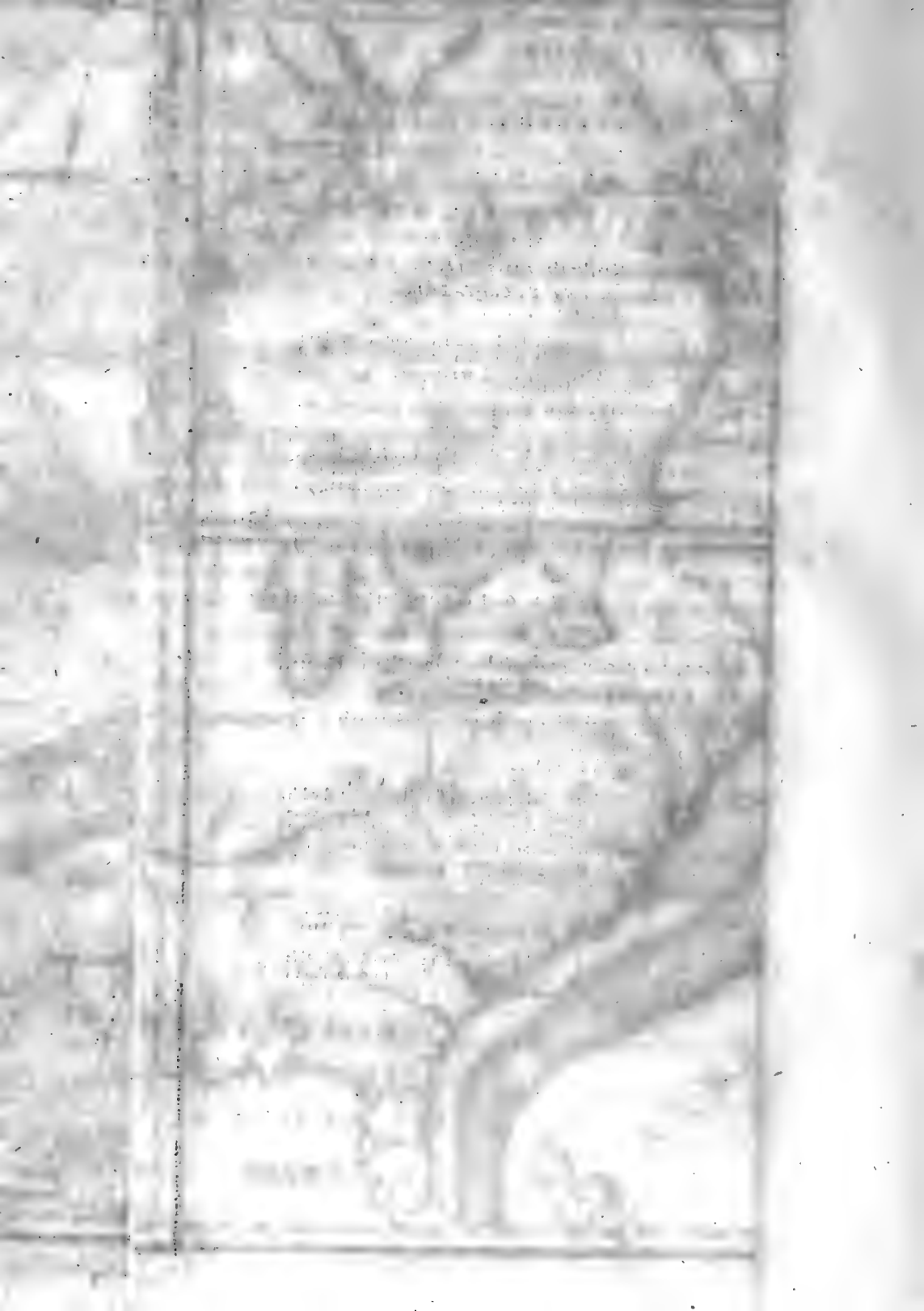
(a) We here see women sit quiet in their houses whilst thunder is rumbling over their heads; would they, at this day, enjoy this happy security, if a man had not observed, some thousand years ago, that a piece of amber when rubbed attracted light bodies which are near it. It is he who put into the hands of modern philosophers the chain in which Franklin was to find the link, from which his imagination took the hint of his conductor.

<sup>a</sup> Nous voyons ici les femmes tranquilles dans leurs maisons lorsque la tonnerre gronde sur leur tête. Jouiraient elles aujourd'hui 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 légers qui l'avoisinent? C'est lui qui a mis dans les mains des physiciens modernes la chaîne ou Franklin devoit trouver le chaînon dont il est parti pour imaginer son conducteur.











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 chevalier 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 faucet which contained the entrails; his dog who had followed, supposing it was a mess for himself, soon emptied the faucet.

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 magasin le plus précieux à l'homme, magasin qui appartient à toutes les nations et à tous les siècles. Ne pas rapporter à ce dépôt 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 chez 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 cœurs 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 favois que ce phénomène intéresseroit. Mais ayant voulu auparavant rentrer chez lui, et ne pouvant d'une main ouvrir le cadenas qui ferme sa porte, il posa à terre la foucoupe sur la quelle étoient ces entrailles. Son chien, qui l'avoit suivi, crut que c'étoit pour lui, et eut bientôt vidé la foucoupe. Mr. le Chevr. de Chastellux vint une heure après chez moi, voir un opossum mâle, dont je lui avois parlé à cause de la singularité des organes de la génération de*

account of the singularity of his organs of generation<sup>b</sup>. 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 faucer. 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 sent ce que je lui voulois dire: J'envoyai demander au chirurgéon 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'il 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 être 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'applaudir aux sentimens qui rendent M. Vergé si scrupuleux. Mais en me piquant 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 forme 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 étoient 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 queue 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.



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 première pouvoit en avoir un vrai et un faux. M. de Voltaire, qui croyoit que la nature s'amuse à imiter des concha veneris, auroit pu croire qu'elle s'amuse aussi à contrefaire des cœurs; je ferai 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'équipage d'artillerie de l'armée du roi en Amérique; déclare, que le dix de ce mois ayant retiré les entrailles du corps d'une perdrix, il me parut qu'il s'y trouvoit deux cœurs. Mais pendant le peu de temps que j'ai pu les considérer, j'étois tellement ébloui d'un phénomène aussi surprénant, que je crains que mes yeux ne m'aient trompé, et n'ose affirmer ce que je crois avoir vu. Je fus sur le champ montrer cette merveille à un marchand qui loge à côté de chez moi, et à M. d'Aboville; ce dernier vient de me lire l'exposé qu'il a écrit de ce phénomène, tel qu'il s'est pré-

senté

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 mē 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 vu.

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.*

**W**ITH respect to water-spouts what I am about to consider is whether water ascends or descends in  
 U u 2 these

these bodies? A question which it is reasonable to think should be determined by facts, and the nature of things; and concerning which, if we wish to attain to any certainty, we must be careful not to be misled by such appearances and imaginations, as have hitherto commanded the general belief.

Agreeable to this method of inquiry, I shall in the first place produce the observations of three or four persons, in whom I can confide for simplicity and honesty of intention.

The first is that of captain Melling, formerly of Boston, who informed me that in a voyage from our West-India islands, in the month of August, in a warm day just at evening, a spout fell close by the vessel, and in two or three seconds of time came across the stern where he then was. A flood of water, as he expressed it, poured upon him and almost beat him down, so that he was obliged to lay hold of what was nearest to him, to prevent being washed overboard, which in his fright he was apprehensive of. But the spout immediately passed off with a roaring noise into the sea. I asked him if he tasted the water? Taste it said he! I could not help tasting it, it ran into my mouth, nose, eyes and ears. Was it then fresh or salt? as fresh, said he, as ever I tasted spring water in my life.

The next account I had was from captain John Wakefield, also of Boston, which was, that being just within the straits of Gibraltar, a spout fell close by his ship with a great roaring which he heard as he was setting in the cabin, the men upon deck immediately crying out for him to come up, which he instantly did, and saw it travelling away before the ship, so near that he plainly saw the water descend. His men assured him that it did so from the beginning. He told me the wind was very small during the operation of it.

Captain John Howland, of the same town, told me that in passing the calm latitudes, a spout fell so 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 expeuce, 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 course of the air, as the bend is to two different ones at different heights.

The next thing proposed to be considered, was the great roar that attends a complete spout while it lasts; and it is the same as that in cataracts or falls of water from great eminencies. This kind of roar could not exist in any way of ascent, being very different from that of a whirlwind, which is no other than that of any other strong wind.

Mr. Stuart's figures of the great spouts are drawn with the appearance of a bush round their bases: The case is such, that great falls of waters must make a proportionable spray; so that the appearance is natural, and indeed a necessary consequence. It rises up from the foot of the spout and falls back in a parabolic manner into the sea. As was said of the roar just now, so it may be said of this, that it could not have existed in any conceivable way of ascent; while on the contrary it is perfectly agreeable to nature on the principle of descent. It continues the whole time of a large spout, increasing and diminishing as that does.

The appearance of a break or partition in the trunk of the spout, at the top of the bush, is a very curious phenomenon: It is not real but apparent, and could not have happened without the bush; it being caused by a refraction of rays from the drops that constitute the top of the bush; whence a divergency and so much loss of vision.

In great spouts there is also a pillar-like appearance, being a part of the trunk within the bush, and by another refraction through the side of the bush; by which it appears much bigger than it is, and limited in altitude by the break. The three last are agreeable to the laws of optics; and all the five particulars being attendants on the greater or the smaller spouts, are to me undeniable evidences of the universal descent of waters in these bodies. I pass from Mr. Stuart's figures to that of Mr. Maine, which is not less 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, is virtually to admit of real and essential miracle, without sufficient proof; and contrary to every idea we can form, of a divinely wise intention.

Tornados and hurricanes I take to be of the same general nature, although differing in some circumstances and appearances.

By the term tornado, or wind-spout, I mean a violent wind which has been observed in these northern colonies a few times since they were discovered and settled by our people. But perhaps no part of the terraqueous globe is entirely free from something of the like kind, as the atmosphere is every where liable to similar commotions.

The Spanish term of tornado, seems to have been chiefly used for a violent storm at sea, of larger extent than what I am about to explain, which is of a more contracted nature, and confined to a narrow sphere of action; so that it requires a particular and significant name, such as wind-spout, till a more suitable one is found for it.

Description of one. It begins of a sudden; more or less of clouds having been drawn together, a spout of wind coming from it strikes the ground in a round spot of a few rods or perches diameter, with a prone direction, in the course of the wind of the day, and proceeds thus half a mile or a mile. The proneness of its descent makes it rebound from the earth, throwing such things as are moveable before it, but some sideways from it. A vapour, mist or rain descends with it, by which the path of it is marked and wet.

I shall produce the instance of that at Leicester, a town about fifty miles from Boston, a few years since, which being more violent than usual, may give some 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 westward and coming over head, a sudden motion of their running together in a point being observed, immediately a spout of wind struck the ground at the western end of a house

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 forcibly 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 forcibly 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 concerning them.

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 rarifications, 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 strata 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 re-  
volves

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 intertropical 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.

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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.

**I**T 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/ 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 cocoon, have eaten their way through their natural inclosure, (which is generally about four days after the cocoon 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 disposed vertically, as against 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 she dies, as does the male; this their second 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 bigness of a common pin's head, and of a straw colour; by degrees they become black, assume more solidity, losing at the same time part of their bulk.

When they are arrived at this point, you must separate them from the cloth; to effect which, you must dip them into a large pan filled with one half water and the other half wine, rather more than lukewarm; when your cloth has soaked in this liquor a little while, you may separate them from the cloth with a silver spoon and dry them in a sunny place, and take them away when they begin to be whitish.

When you have thus detached your eggs, you must keep them till the next year in a cool damp place to preserve them from hatching during the great heat, which would ruin the project.

On the arrival of the spring, you must observe when the mulberry tree begins to put forth its leaves, which must be your signal to expose your eggs in a very warm place, that they may all hatch at once, otherwise 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 case the pains required to separate their different classes would be excessive, not to say impossible. To hatch your eggs you must carry them about you nine or ten days, keeping them in your bosom, or other parts near the body; in the night you may put them between the matresses of the bed. You may likewise hatch them by the heat of an oven, but this method is dangerous, because you may possibly burn the worm

contained in the egg, and thereby destroy 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 otherwise could not contain him. He preserves this black colour eight or nine days. After your worms are hatched you must put them on wicker shelves, which are covered first with paper and afterwards with a bed of the youngest and most tender mulberry leaves; you may place several ranges of them in the same chamber, one above another, provided you leave at least a foot and a half between each range; that the scaffolding be in the middle of the room, and that your wicker shelves be not too broad, but just so as to reach on each side conveniently to the middle. By degrees the worm grows and requires more room. It must be your care to thin them, and keep those of the same size as near as you can on one row; for which reason you must always leave some shelves vacant for that purpose.

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 first sickness, which consists in a kind of lethargic sleep for three days together, during which space he changes his skin, still preserving the same bulk.

This sleep being over, he begins to eat again during five days, at which term he is grown to the size of seven lines in length, after which follows a second sickness\*, in every respect 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 sickness; which over, he continues to eat again five days more, which are followed by his fourth sickness, at which time he is arrived at his full growth, i. e. about fourteen lines in length and two in diameter.

He:

\* You must observe that these sicknesses are much longer, and last seven 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 cocoon.

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 cocoon. 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 reason you must avoid offending their delicate organs.

In many parts of Italy, amongst others Romagna and La Marche of Ancona, they have two silk racoltas, or harvests. They keep the eggs in very cool places, and when the mulberry tree begins to bud again (for during the racolta it is stripped of its leaves for food for the worms) they expose their eggs to hatch. Sometimes they give rose leaves to the young worms, when there are no young mulberry leaves. The cocoons of this second racolta are rather inferior to those of the first. The silk worm is generally fourteen lines in length and two in diameter and six and two-sevenths in circumference. He is either of a milk or pearl colour or blackish: these last are the best. His body is divided into seven rings, to each of which are joined two very short feet. He has a small point like a thorn, exactly above the anus. The substance which forms the silk, is in their stomach, which is very long; wound up as it were on two spindles and surrounded with a gum, commonly yellow, sometimes white, not often greenish. When the worm spins his cocoon, he winds off a thread from each of his spindles, and joins them, afterwards, by means of two hooks which are placed in his mouth; so that the cocoon is composed of a double thread. Having opened a silk worm you may take out the spindles which are folded up in three plaits, and on stretching them out and drawing each extremity, you may extend them to near two ells in length. If you then scrape the thread so stretched out with your nail, you will scratch off the gum, which is very much like bees-wax, and performs the same office to the silk it covers, as a gold leaf does to the ingot of silver it surrounds, when drawn out by the wire-drawer; the silk then remains of a pearl colour. This thread which is extremely strong and even is about the thickness of a middling pin.

Three things very remarkable in this insect, are,

1. They

1. They describe a femicircle in eating.
2. Their excrement has perfectly the form of a mulberry.
3. They have no sex before their metamorphosis.

## CHAP. II. *Of the Cocons.*

I. IT is almost a general rule to wait six or seven days after all the cocons seem 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 some air to the room in which you have kept them, in order to dissipate a considerable dampness which the worms exhale on their mounting, (when they have not been well fed and kept, for when they have been properly nursed this dampness is not to be found) and which is of great detriment to the cocons, either by rotting them, rendering them soft, or covering them with spots.

The cocons may be divided into two general classes, the white and the yellow, in the yellow you meet with all the shades from a bright yellow diminishing at last to white, some few are of a pale green. We reckon nine sorts of cocons, viz.

1. The good cocons are those which are brought to their perfection, strong and little, and not at all spotted.

2. The pointed cocons are those, one of whose extremities rises up in a point. After having afforded a little silk, the point, which is the weaker part, breaks or tears, and it is impossible to continue winding that cocon any longer, because when the thread comes round to the hole it is of consequence broke.

3. The coccalons are a little bigger than the other, yet they do not contain more silk, because the contexture is not so strong. In winding they are to be separated from the rest, because they require to be wound in cooler water, otherwise they furze out in winding.

4. The

4. The dupions, or double 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 afunder from the rest; they make the silk we call dupions.

5. The foufflons are cocons very imperfect, whose con-texture 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 cocoons 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 cocoons that are bought, there ought to be neither soufflons, nor perforated cocoons; 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 cocoons 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 cocoons 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 cocoons 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 cocoon 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 cocoon 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 cocoon 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 cocoon, from five hundred to six hundred ells in length.

IV. The worm or chrysalis, as he is inclosed in his cocoon 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 feed. 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 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. 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 coccalons; 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 filk 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 handfuls 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 Battue*.

When the threads are quite clear, you must pass four of them (if you will wind fine filk) through each of the holes in a thin iron bar that is placed horizontally at the edge



edge of your bafon; afterwards you twist the two ends (which confift of four cocons each) twenty or twenty-five times, that the four ends in each thread may the better join together in croffing one another, and that your filk may be plump, which otherwife would be flat.

Your windfter muft always have a bowl of cold water by her, to dip her fingers in, and to fprinkle very often the faid 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 poft-rampin, moves it from the right to the left, and confequently the thread that is upon it; fo that your filk is wound on the reel crofs-ways, and your threads form two hanks of about four fingers broad.

As often as the cocons you wind are done, or break or diminifh only, you muft join frefh ones to keep up the number requifite, or the proportion; I fay the proportion, becaufe as the cocons wind off, the thread being finer, you muft join two cocons half wound to replace a new one: Thus you may wind three new ones and two half wound, and your filk is from four to five cocons.

When you would join a frefh thread, you muft 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 reft. You muft not wind off your cocons too bare or to the laft, becaufe when they are near at an end, the bairré, as we call it, that is the hufk, joins in with the other threads and makes the filk foul and gouty.

When you have finifhed your firft parcel, you muft clean your bafons, taking out all the ftriped worms, as well as the cocons, on which there is a little filk; which you firft open and take out the worm and then throw them into a bafket by you, into which you likewife caft the loofe filk that comes off in making the battüe. You

You then proceed, as before, with other two or three handfuls of cocoons; you make a new battüe; you purge them, and continue to wind the same number of cocoons 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 basin 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 cocoon 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 basin four times a day, for your dupions and choquette, and twice only for good cocoons 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 cocoons 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 cocoon to a thousand; but it is difficult to wind more than thirty in a thread. The nicety, and that in which consists

consists the greatest difficulty, is to wind even, because as the cocon winds off, the end is finer, and you must then join other cocons to keep up the same size. This difficulty of keeping the silk always even is so great, that (excepting a thread of two cocons, which we call fuch) we do not say a silk of three, of four, or of six cocons, but a silk of three to four, of four to five, of six to seven cocons. If you proceed to a coarser silk you cannot calculate so nicely as to one cocon more or less. We say for example, from twelve to fifteen, from fifteen to twenty, and so on.

It is easy to conceive, that it is more difficult to wind a coarse silk even, than a fine one, because it is harder to keep a great number of cocons always to the same size, than a small one.

The dupions which you design for rondelette, or ordinary sewing silk, are to be wound from fifteen to twenty. The rest you may wind as coarse as possible, i. e. from forty to fifty: they serve to cover and fill up in coarse stuffs, and may likewise be used for some sort of sewing silk.

The good choquette is to be wound according to the uses to which you intend to apply it; however not finer than from seven to eight. The bad choquette you may wind from fifteen to twenty cocons.

In winding the good cocons, you will always meet with some defective, which will not wind off, or are full of gouts and nubs. These you must take out of your bason and keep by themselves. They are called *bassinats*. They are to be wound apart as coarse as you can. They make a foul, dirty silk. To have a good silk, you must wind in fine weather. If the wind be high it shakes your silk, and prevents its lying smooth on the reel, forms strings of threads, which make it very difficult to wind on bobbins. If the weather is rainy the silk is damp, and has not that lustre it ought to have, or which it has when it dries, as it goes upon the reel. You must mind not to hank it when damp, but let it dry on the reel; otherwise 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 *morefques*. 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 *fattinades* 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\*.

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\* 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 repositories 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 Monf. 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 substance, and leaves them entirely naked and perfectly exposed to the eye. I once shewed a preparation of the vessels of a kidney I had thus executed at Paris, to a meeting of the French academy of surgery in the year 1764, who allowed it to be curious and quite new to them. I think none of the members present at that meeting, except *Monf. Morand*, secretary of that academy, who had been in England and was acquainted with doctor *Hunter*, alledged their having ever seen a similar preparation. At their request I presented a memoir on the subject, and since that time *Monf. Süe* has bestowed one entire section in treating expressly 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 *Pennsylvanian* by birth, or native of *America*, which have led some into mistakes concerning the author of that piece. The reason may be that *Americans* before the revolution, being but little considered in any other light than as colonists, their nation was seldom taken notice of, and I was introduced to him first as a graduate of the university of *Edinburgh*, and known to him afterwards as a member of the royal college of physicians of that place.

These preparations are, since that period, become common in *France*, and the art is now well known, and cultivated successfully by *Monf. Süe* and others; but it was unknown there till I communicated it, first at *Paris*, and afterward in the south of *France*; where I had the honour of explaining it to the illustrious *Monf. Imbert*, chancellor of the university at *Montpelier*, and to *Monf. Bourgelas*, principal of the *Ecole veterinaire*, or academy established at *Lyons* for the improvement of the science of horsemanship, justly celebrated for his very elegant preparations of the anatomy of horses, &c. But what gave me equal pleasure and surprize, was the admiration excited on my presenting



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.

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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 corrosions, 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 fuet, or of wax and oil; the fuet 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

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eleven or twelve ounces, to procure a proper consistence to the injection.

The method of ascertaining the due consistence and the necessary firmness of the injection, is by taking up any quantity of it, whilst melted, with a small wooden spatula, and then letting it fall drop by drop on the surface of cold water. This immediately spreads and forms into a thin plate. By rolling it between your thumb and finger first moistened, or in the palm of one hand with the fingers of the other, both previously made wet to prevent sticking, turn it into the shape of a cylinder or small blood-vessel, then throw it into a basin of cold water, and let it remain till it is quite cold. If it is then of such a consistence as not to yield to a very slight force, when pressed between your thumb and finger, and yet so soft as to be capable of bending readily without breaking, it has the due medium of firmness and flexibility which is desired. If it appears to be too soft, a further quantity of wax and rosin are to be added in the above mentioned proportion, till it acquires the wished for consistence. If, on the other hand, it is too hard, a proportionably larger quantity of the Venice turpentine is to be added. The same precaution is to be observed, if we make use of rosin, wax and spirit of turpentine, as directed in the first receipt.

Operators seldom are at the trouble of weighing the ingredients; they generally judge of the respective weights and proportions of each by the eye. This method of determining them will answer very well for persons who have acquired experience; for the different season of the year when the injection is made, and the different consistence or purity of the wax and rosin, with other little circumstances which sometimes happen, occasion some little variation. In general there is not so great danger of spoiling the preparation, by making the composition a little softer than is required, rather than harder, because it grows some what harder by time, and also by steeping the

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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 effervescence, which often happens when there is too great a fire, or when all the ingredients are mingled together before the colouring powders are added, especially the two last. The quantity is judged of by trial.

*Of the proper MENSTRUUM.*

I now pass to the third head, viz. to consider what substances will answer, for consuming all the parts surrounding the injection, and leaving the matter in the vessels themselves untouched.

The best I have yet tried is the concentrated fuming acid of marine salt, which comes over in the distillation employed in the process for making glaubers salt; it should not be diluted with water, nor be dulcified, otherwise it becomes too weak to answer the purpose, or at least the time required for completing the corrosion is thereby protracted beyond what is needful.

The concentrated acids of vitriol and of nitre, are no less powerful to destroy all the animal substance, surrounding the injected vessels, but the objection to which, from several trials, they appear to be liable, is that they are supposed to crisp the vessels; at least the spirit of sea-salt has been most used, and concluded to be the least exceptionable menstruum for this operation.

Such were the sentiments I communicated, in my memoir to the royal academy of surgery at Paris in the year 1764, since which, Monf. Süe, royal professor of anatomy in the schools of surgery, and in the royal academy of painting and sculpture at Paris, having honoured my communication to the abovementioned academy, with a section in his treatise entitled *Anthropotomie*, chap. 2. sect. 25. from page 70 to page 84; after acknowledging that he received the art of making those preparations from me, thus adds. "Since that time my nephew and I, having worked at them a great deal, and with abundant success,

ces, 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.

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\* Monf. Morgan, Docteur en médecine de la faculté d'Edinbourg, en a donné une description exacte à l'Académie 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 découvert qu'on pouvoit changer le menstrue, sans préjudicier en rien à la préparation, 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 conservés. 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 forte ne coûte tout au plus que 2 liv, et que la quantité est la même pour l'effet. J'ai employé aussi avec beaucoup de succès l'esprit de nitre. *Anthropotomie*, pag. 83. 84.

The substance 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 fast, upon coming into contact with the parts into which it is impelled, nor ought it to be so great, on the other hand, as to burn or crisp the vessels, or prove troublesome to the operator in handling the syringe or pipes, whilst he is making the injection.

The injection should be thrown into the part to be prepared, at one uniform impulse, made slowly and evenly, with a steady hand, and with so little force as not to endanger a rupture of the vessels in the soft parts. When the injection is finished, the pipes should be corked or otherwise closed, and the parts injected should be suffered to cool by degrees. If they are plunged at once into cold water, before the substance of them has acquired a certain degree of hardness and firmness, a contraction in the elastic coats of the vessels may be produced, sufficient to occasion a rupture, especially in the capillaries, which will always be followed by an extravasation of the injected substance.

Having exposed 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 vessel and absorbing the water from their surface with a sponge gently applied, or suffering it to run off by draining, they should be put into a sufficient quantity of the menstruum to cover the preparation entirely.

The next consideration is how to make use of the menstruum for corroding the parts to be dissolved and removed from the vessels. For this purpose the operator should be furnished with a china bowl or a stone vessel, on which the menstruum can make no impression; or what will answer still better, a glass vessel with a mouth sufficiently 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 concentration 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.

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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.*

“**W**HEN 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.

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MANIERE DE FAIRE LES PREPARATIONS EN CIRE.

**L**ORSQU'ON veut faire quelque preparation en cire, il faut commencer par mouler la partie que l'on veut imiter avec du platre frais et bien fin, ayant l'attention de bien huiler 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 sécher 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 représenter, avec du carmin, ou quelque autre couleur; ou bien, si la partie a plusieurs parties de couleur différente, alors on ne colore point la piece. Et on met la couleur sur chaque partie après que la piece a été fondue. Pendant que la cire fonde on prépare le moule, qu'on a le soin de bien huiler avec un pinceau. Ensuite on remble 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 fentes ou les jointures des pieces du moule, on y met sur les jointures extérieurement, de la terre glaisse. Le moule préparé ainsi, et ayant eu l'attention de y laisser une ouverture, on verse la cire dans le moule, et l'on a le soin de tourner le moule en tout sens, pour que la cire se repande également par tout l'intérieur du moule, jusques à ce qu'elle soit refroidie. Si vous jugez que la piece ne soit pas assez épaisse, vous remettez de nouveau de la cire, et vous faites comme ci dessus. Après quoi on laisse refroidir la piece dans le moule, et ensuite on tire avec beaucoup de precaution les pieces du moule, l'une après l'autre. La piece étant entièrement dépouillée; on la repare. C'est à dire qu'on ôte de des-

the joinings of the pieces of the mould, which being entirely repaired, we then colour the different parts which compose the piece with colours suitable to each part, that the arteries may be coloured with vermillion; the veins with Prussian blue; the muscles with carmine; and so of the other parts; *which finishes the preparation.*

“*Note, When we wish to make the wax less brittle, we must add some spermaceti to it; and sometimes a little of the finest Venice turpentine that can be procured.*”

Les portions excédentes de cire qui se font glissées entre les jointures des pièces du moule. La pièce étant entièrement réparée, on colore les différentes parties qui composent la pièce avec des couleurs convenables à chaque partie, en sorte que les artères seront colorées avec du vermillon; et les veines avec du bleu de Prusse; les muscles avec du carmin; ainsi des autres.

Nota que quand on veut rendre la cire moins cassante on y adjoute le blanc de baleine, quelque fois aussi un peu de terebentine de Venise, tout ce qu'il y a de plus fin.

### N° XLIII.

*Of a living Snake in a living Horse's Eye, and of other unusual Productions of Animals. By JOHN MORGAN, M. D. F. R. S. London, Professor of the Theory and Practice of Physic, Philadelphia.*

Read June  
5, 1782.

**W**HETHER there is such a thing in nature as equivocal generation, by which is to be understood the production of any new animal independent of a parent stock of the same kind, has been a subject of controversy amongst philosophers; some asserting the reality of this doctrine, whilst others, as the celebrated Harvey and his followers as strenuously reject it. The latter, which is now deemed the orthodox side of the question, affirm that the young of all are produced from an egg, furnished by the female, and fœcundated by a male animal. From the light thrown upon this subject, by the deep researches of Hippocrates, Galen and Aristotle,

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 philosophy, which had no solid foundation, the race of sceptics I refer to deem it manly not only to withhold their assent from truths they do not understand, but to disown and dispute the reality of them. They do not consider that, by such conduct, they endeavour to divest themselves and others of their rational faculties, and of that natural curiosity implanted in man by his Creator, for the wisest purpose, as a guide for investigating facts, in order to lead him to knowledge, which has given birth to discoveries of the greatest importance to mankind.

In answer to the cavils of minute philosophers, I would briefly remark the first step to new discoveries, is an exact attention to the phenomena of nature, unbiassed by preconceived hypotheses, and that it is as much a mark of a defective understanding to admit too little for truth, upon evidence, as to believe too much from credulity.

I have been led into the above train of observations from a singular phenomenon that may be now seen in this city, and which is worthy the inspection of the curious. It is advertised in the public newspapers, viz. the Pennsylvania Gazette, May 23d, as worthy of the attention and critical inspection of all curious persons, whether philosophers or physicians, and particularly the latter, as it may, for what they know, if properly examined into, throw some useful light upon the functions and diseases of the animal body.

What I refer to is an horse with a snake in its eye, to be seen in Arch-street, between Sixth and Seventh streets, not only possessed of mere life, but endowed with a very brisk locomotive faculty. True philosophers will not treat the assertion as idle, fictitious or romantic, but see and judge for themselves.

The writer of this piece has undertaken the present task, on purpose to excite every class of people to satisfy themselves of the reality of the fact, that when recorded in the very place where all have it in their power to determine its existence, on the testimony of their own eyesight, they may

not

not plead ignorance and unbelief. He professes, for his own part, to be as little credulous or liable to impositions, from accounts of pretended miraculous appearances, as his neighbours, however learned. Indeed he has ever strenuously opposed, and thinks he ever shall, what he deems empty tales of visionary speculatists, bred by weak fancies, or raised by designing men, to amuse or deceive the vulgar; but he admires and reveres the unfearchable wisdom of the divine architect, who framed this spacious universe, teeming with myriads of animal beings, as well in those instances where his design and footsteps are visible, as in those which lay more remote from human comprehension. Upon the first relation of this curious history from others, unacquainted with the structure of the eye, and therefore more likely to pass a wrong judgment; and, till he had an opportunity to examine it himself, he believed the appearance to be some unusual disease, or a filamentary production on the crystalline humour, from a stroke or inflammation of the eye, and that a convulsion in the nerves of its coat might produce an irritation in that organ, and a tremulous motion, which might impose upon those who, not knowing how to account for the appearance, should content themselves with calling it a snake in the eye, merely from its resemblance, on first sight, to that animal. But from the closest ocular examination, with unwearied attention, repeated more than once, he conceives he is not mistaken, in asserting that there is a real snake in the eye; which, from the vivacity and briskness of its motion, exceeds that of any worm, and equals that of any kind of serpent he has ever seen.

To satisfy the public in general, as well those who have now an opportunity of seeing it, as such who may happen never to see it, I think it will not be amiss to describe its appearance, and to deliver what I have been able to collect of its history.

The



The horse in whose left eye this extraordinary *lufus nature* 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 phenomenon, 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 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 phenomenon, 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 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 found."

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 seen them in the human kidneys, and such as are more frequently met with in the kidneys of dogs. That worms have been sometimes 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 passages those animals or their eggs were insinuated into the interior parts of the body, is not easy to determine. It does not seem probable that they could reach the forementioned places through the pores of the skin or the organs of respiration; much less that their eggs were taken in at the mouth, and from thence proceeded to their respective places; nor yet is it likely they could remain entire in the stomach, where, in the process of chylification, there is a remarkable fermentation and breaking down the parts of the food. Nor can the chyliferous or lacteal vessels afford a passage to the eggs in their rout; and lastly, no one alledges that he has ever seen exactly similar worms out of the body.

Were I so disposed, or did the design of this paper require it, I could to those observations add many extraordinary instances, of strange, rare, and surprizing productions of animals in the human body, from the works of the celebrated Bartholine, physician to the king of Denmark, and from other grave and learned authors of unexceptionable repute. But here I pause, to reverence the hand that framed not only our bodies, but those of the meanest reptiles, with an exuberance of skill, which proclaims that they are not the effect of chance; and acknowledging I am lost in wonder, I leave the fuller explanation of the uncommon productions, to some happy genius that may arise, if ever it should please God to produce such an one into the world, who by tracing out the footsteps of the Creator, shall be able to throw clearer lights than we yet have, upon these abstruse subjects.

## 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 Monf. Le Vallois, Dentist of the King of France at Guadaloupe in the West Indies, as follows.*

**A** DELAIDE, 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 fontanelle. 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 interspersed with black spots. There are some smaller and more numerous about her knees than elsewhere.

Upon the large black spots there are also many smaller and blacker which are very glaring. Many of these spots divide into four, five and six rays, resembling a star, which are not observed but by a close inspection, and then they are very visible. In several parts those spots, being of different shades, give an exact picture of lunar eclipses, as they are commonly represented in the books of astronomy. 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 striking resemblance to gloves and to buskins.

The white that prevails over the breast, and over the belly, arms and thighs, has a lively appearance. The skin is soft, smooth and sleek.

Adelaide has fine features; we meet with few negroes of so beautiful a form. In her temper she is cheerful, gay and sportful, and as tall as children of her age generally are, and hath evidently a very delicate temperament, yet enjoys pretty good health, neither hath she eyes, nor ears, nor any particularity in her features, or external conformation, like what may be seen at the first inspection in those who are called white negroes, whose skin is altogether of a dead white colour, and whose woolly white hair and features resemble those of their negro parents.

From this detail we may remark that the alteration of the natural colour of Adelaide, takes place over the same parts of the body, for the most part, as over the body of Maria Sabina, of whom Mons. Buffon gives an account; and considering it as a well authenticated fact, from all the information that has been received of Adelaide, that she had a negro father and negro mother, we are led to believe, that the English account under the portrait of Maria Sabina is exact, and not asserted merely for the  
fake

fake 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 *Monf. 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 *Monf. le Vallois* has with him, legally authenticated by *Monf. Blin*, lieutenant judge, given from under the hand of *Monf. des Effart*, king's physician, and of *Monf. 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 phenomena and alteration of the natural colour of their skin, are left for others to investigate and explain.

Monf. 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.

## N° XLV.

*Extract of a Letter from BERNARD ROMANS, of Pensacola, dated August 20, 1773.*

**T**HE 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.

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*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. <i>Coste</i> ,	An analytic, etymologic, and argumentative treatise on the accent and pronunciation of the English language.
Dec. 15. Monf. <i>Chabot-laux</i> ,	His works de la felicité publique, 2 vol.
1781, Jan. 19. Monf. <i>de Marbois</i> ,	A pamphlet in French, containing an account of, and proposals for printing a work of great merit, entitled voyages métallurgiques, &c. Par M. Jars.
Feb. 16. Gen. <i>Sullivan</i> ,	Specimens of talc, from a large body of this fossil, lately discovered in New-Hampshire.
Mar. 16. <i>Sam. Adams</i> , 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 <i>Pennsylvania</i> ,	One hundred and fifty pounds.
May 2. Mr. <i>J. M. Henry</i> ,	A curious specimen of petrified pine.

Sept.

## D O N A T I O N S.

1783,	DONORS.	PRESENTS.
Sept. 26.	Dr. <i>Franklin</i> ,	Natural history of the South of France, by Monf. L'Abbé Sou-lavie, 2 vols.
Oct. 17.	Mr. <i>John Hyacinth de Magellan</i> ,	A collection of Philosophical tracts, &c.
	Mr. <i>Godfrey</i> ,	A collection of prints, representing some of the principal events during the late revolution in America.
	Abbé de <i>Fontana</i> ,	Four pamphlets on Philosophical subjects, written by himself.
Dec. 5.	Mr. <i>Van Berckel</i> ,	Six volumes of the Transactions of the Batavian Philosophical Society of Rotterdam.
	Monf. de <i>Etienne</i> ,	On a cement impenetrable by water.
1784, Jan. 16.	Mr. <i>John Felfted</i> ,	A collection of specimens of the several woods growing in the island of Jamaica.
Febru. 3.	Mr. <i>Warder</i> ,	A collection of animal calculi.
	Mr. <i>Mandrillon</i> ,	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.

# D O N A T I O N S.

1784. DONORS.	PRESENTS.
Feb. 20. <i>Samuel Vaughan</i> , Esquire,	<p>A <math>3\frac{1}{2}</math> 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.</p> <p>A Gunter's scale improved, for navigation and astronomy, by Mr. John Robertson, with a description thereof by Mr. Mountaine.</p> <p>A magazine microscope, consisting of a single, double, and solar microscope.</p>
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*Pere*

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