John Redman Coxe

TRANSACTIONS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY,

HELD AT

PHILADELPHIA,

FOR PROMOTING

USEFUL KNOWLEDGE.

VOLUME IV.

pp. 282—287 omitted in printing

PHILADELPHIA:

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1799.

ADVERTISEMENT.

THE following are the rules adopted for the government of 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 impor- " tance or ſingularity of the ſubjects, or the advantageous " manner of treating them, without pretending to an- " ſwer, or to make the ſociety anſwerable, for the cer- " tainty 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 reſpec- " tive 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."

In the Month of May 1796, *the Society, in order the more effectually to answer the ends of their institution, agreed to appropriate, annually, a part of their funds to be disposed of in Premiums to the Authors of the best performances, inventions, or improvements, relative to certain specific subjects of useful knowledge. The following premiums were thereupon proposed:*

I.

For the best system of liberal education and literary instruction, adapted to the genius of the government, and best calculated to promote the general welfare, of the United States; comprehending also a plan for instituting and conducting public schools, in this country, on principles of the most extensive utility——*A premium of one hundred dollars.*

Papers on this subject will be received, till the first day of January, 1797.

II.

For the most simple, easy and expeditious method of computing the longitude, from the common lunar observation,——*A premium of seventy dollars.*

The particular view of the society, in proposing this subject, is, that the solution of this most useful problem may, if possible, be rendered so plain and easy, as to be readily learned by every mariner, even of moderate capacity, who understands the common rules of arithmetic; and thus be introduced into general practice.

Papers on this subject will be received, till the first day of January, 1797.

III.

For the best construction or improvement of ship-pumps,——*A premium of seventy dollars.*

Improvements

Improvements which may be readily applied to the ſhip-pumps in common uſe, will be moſt likely to be adopted by ſeamen, and introduced into general practice.

Papers on this ſubject will be received, till the firſt day of January, 1797.

IV.

For the beſt conſtruction or improvement of ſtoves, or fire-places,——*A premium of ſixty dollars.* The principal end which the ſociety have in view, in propoſing this ſubject, is the benefit of the poorer claſs of people, eſpecially of ſuch as live in towns, or other places where fuel is dear. To anſwer this end, the ſtove ſhould be cheap, and of durable materials; ſhould afford the neceſſary degree of a ſalubrious and durable heat, with the leaſt expenſe of fuel poſſible; and ſhould be capable of being employed both for the purpoſe of warming the room, and cooking proviſions for the family.—The ſociety have been informed, that ſtoves made of brick are, in many reſpects, ſuperior to thoſe made of metal; eſpecially, in the ſaving of fuel, and preſerving a more equable degree of heat.

Papers on this ſubject will be received, till the firſt day of January, 1797.

V.

For the beſt method, verified by experiment, of preventing the premature decay of Peach-trees,——*A premium of ſixty dollars.*

Papers on this ſubject will be received, till the firſt day of January, 1798.

VI.

For the beſt experimental treatiſe on native American vegetable dies; accompanied with an accurate account of the vegetables employed——*A premium of ninety dollars.*

Papers

Papers on this ſubject will be received, till the firſt day of February, 1798.

VII.

For the beſt conſtruction or improvement of lamps; eſpecially, for lighting the ſtreets——*A premium of fifty dollars.*

Papers on this ſubject will be received, till the firſt day of April, 1797.

GENERAL CONDITIONS.

1. Every candidate, along with his performance, is to ſend to the ſociety a ſealed letter, containing his name and place of abode; which letter ſhall never be opened by the ſociety, except in the caſe of a ſucceſsful candidate.

2. No performance, invention or improvement, on any of the ſubjects propoſed, for which a patent or any other reward ſhall have been obtained, before preſenting it to the ſociety, ſhall be conſidered as entitled to the premium.

3. In lieu of the money which ſhall be awarded by the ſociety, as a premium, any ſucceſsful candidate ſhall have it in his option to receive a gold or ſilver medal, or piece of plate, with a ſuitable inſcription, of equal value.

4. The ſociety reſerve to themſelves the power of giving, in all caſes, ſuch part only of any premium propoſed, as the performance ſhall be adjudged to deſerve; or, of withholding the whole, if it ſhall appear to have no merit above what may have been already publiſhed on the ſubject. The candidates may, however, be aſſured, that the ſociety will always judge liberally of their ſeveral claims.

A number of papers have been received on the ſeveral ſubjects above ſtated. But the ſociety have as yet forborne the adjudication of any premium, except that offered for the

the beſt ſyſtem of liberal and literary inſtruction, adapted to the genius of the government, and beſt calculated to promote the general welfare of the United States.

On the fifteenth of December 1797, the ſociety, agreeably to ſpecial appointment, proceeded to conſider the ſeveral Eſſays on Education, which had been preſented. The analyſes of theſe Eſſays were read, when it was agreed to take the queſtion in this manner on each performance, viz. " Is this the beſt ſyſtem of liberal Education and literary Inſtruction, adapted to the genius of the government, and beſt calculated to promote the general welfare, of the United States?"

Whereupon it appeared to the ſociety that two of theſe Eſſays, I. An Eſſay with this motto, " *In Metii deſcendat Judicis aures.*" HORACE. II. An Eſſay with this motto, " *I call a complete and generous Education that which fits a man to perform juſtly, ſkilfully and magnanimouſly, all the offices, both private and public, of peace and war.*" MILTON; poſſeſſed a ſuperior degree of merit, and were worthy of publication. The premium was in conſequence adjudged to be equally divided between the authors of theſe two Eſſays.

The Preſident then opened the ſealed letters which accompanied theſe performances, when it appeared that the Reverend SAMUEL KNOX, A. M. of Bladenſburg, in Maryland, was author of the firſt, and SAMUEL HARRISON SMITH, A. M. of Philadelphia, was author of the ſecond Eſſay.

The ſociety then directed the publication of the two Eſſays.

MR. I. H. DE MAGELLAN, OF LONDON,

Having made a donation, to the ſociety, of *two hundred guineas*, to be veſted in a permanent fund; that the intereſt

intereſt ariſing therefrom may be diſpoſed of, in annual premiums, to the authors of the beſt diſcoveries or moſt uſeful improvements, relating to Navigation, or to Natural Philoſophy, mere Natural Hiſtory only excepted;—the following are the rules and conditions, adopted by the ſociety, for the diſpoſition of the propoſed premiums, in conformity to the intention of the Donor, viz.

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

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

III. 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.

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

V. 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, without a ſpecial order of the ſociety for that purpoſe.

VI. The

VI. The ſociety having previouſly referred the ſeveral communications, from candidates for the premium then depending, to the conſideration of the twelve counſellors and other officers of the Society, and having received their report thereon, ſhall, at one of their ſtated meetings, in the month of December, annually, after the expiration of this current year (of the time and place, together with the particular occaſion of which meeting, due notice ſhall be previouſly given, by public advertiſement) proceed to the final adjudication of the ſaid premium: and after due conſideration had, a vote ſhall firſt be taken on this queſtion, viz. "Whether any of the communications, then under inſpection, be worthy of the propoſed premium?" If this queſtion be determined in the *negative*, the whole buſineſs ſhall be deferred till another year: But if in the *affirmative*, the Society ſhall proceed to determine, by ballot, given by the members at large, the diſcovery, invention or improvement moſt uſeful and worthy. And that diſcovery, invention or improvement, which ſhall be found to have a majority of concurring votes in its favour, ſhall be ſucceſsful. And then, *and not till then*, the ſealed letter accompanying the crowned performance, ſhall be opened, and the name of the author announced as the perſon entitled to the ſaid premium.

VII. No member of the Society who is a candidate for the premium then depending, or who hath not previouſly declared to the Society, either by word or writing, that he has conſidered and weighed, according to the beſt of his judgment, the comparative merits of the ſeveral claims then under conſideration, ſhall ſit in judgment, or give his vote, in awarding the ſaid premium.

VIII. 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.

IX. The unſucceſsful performances ſhall remain under conſideration, and their authors be conſidered as candidates for the premium, for *five* years next ſucceeding the time of their preſentment; except ſuch performances as their authors may, in the mean time, think fit to withdraw: And the Society ſhall annually publiſh an abſtract of the titles, object or ſubject matter of the communications ſo under conſideration, ſuch only excepted as the Society ſhall think not worthy of public notice.

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

XI. In caſe there ſhould be a failure, in any year, of any communication worthy of the propoſed premium, there will then be two premiums to be 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.

XII. 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 a ſhort Latin motto ſuited to the occaſion——together with theſe words, *The premium of* I. H. De Magellan, *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.*——— *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.

Since

Since the publication of their laſt volume, the Society has had occaſion to deplore the loſs of their Preſident, DAVID RITTENHOUSE. He died June 26th, 1796.

At a meeting, convened by ſpecial order, on the firſt of July, the following motion was made, and unanimouſly adopted, viz. That this Society, deeply affected by the death of their late worthy Preſident, do reſolve, That an EULOGIUM, commemorative of his diſtinguiſhed talents and ſervices, be publicly pronounced before the Society by one of its members.

At the next meeting DR. BENJAMIN RUSH was appointed to prepare the Eulogium, and on the 17th of December following it was pronounced in the ſecond Preſbyterian Church before the Society. After which the Society, having returned to their Hall, directed the publication of the Eulogium.

On the ſixth of January 1797, at the annual election of officers, THOMAS JEFFERSON, was called to the chair of the Society.

In conſequence of this appointment, the Secretaries on the 7th of January addreſſed to Mr. Jefferſon the following letter.

Philadelphia, Jan. 7, 1797.

SIR,

We have the pleaſure of informing you that at the annual election of officers of the American Philoſophical Society for promoting uſeful knowledge, held at Philadelphia, on the 6th Inſtant, you were choſen Preſident of that reſpectable inſtitution.

The Society, Sir, cannot ſoon forget the loſs they ſuſtained by the death of the late worthy and ingenious D. Rittenhouſe; but after expreſſing their grief on this melancholy occaſion, they look forward with this conſoling reflection, that in the ſame chair, from which two American philoſophers have ſucceſſively inſtructed them and the world, a third is now ſeated, by whoſe genius and knowledge, our national name will preſerve a diſtinguiſhed place in the annals of ſcience.

Permit us, Sir, on this occaſion, to expreſs our ſatisfaction in this pleaſing event, and in being the organs by which the Society announce their choice.

We are,

With Sentiments of Eſteem and

Reſpect, Sir,

Your obedient Servants,

Samuel Magaw,
Jonathan Williams,
William Barton,
John Bleakley,
} Secretaries of the American Philoſophical Society of Philadelphia.

Thomas Jefferson, Eſq.

To this letter, Mr. Jefferſon, on the twenty-eighth of January, 1797, replied as follows:

Monticello, Jan. 28, 1797.

Gentlemen,

I have duly received your favor of the 7th inſt. informing me that the American Philoſophical Society have been pleaſed to name me their Preſident. The ſuffrage of a body, which comprehends whatever the American world has of diſtinction in philoſophy and ſcience in general, is the moſt flattering incident of my life, and that to which I am the moſt ſenſible. My ſatisfaction would be complete, were it not for the conſciouſneſs that it is far beyond my titles. I feel no qualification for this diſtinguiſhed poſt, but a ſincere zeal for all the objects of our inſtitution, and an ardent deſire

desire to see knowledge so disseminated through the mass of mankind, that it may at length reach even the extremes of society, beggars and kings. I pray you, gentlemen, to testify for me to our body, my sense of their favor, and my dispositions to supply by zeal what I may be deficient in the other qualifications proper for their service, and to be assured that your testimony cannot go beyond my feelings.

Permit me to avail myself of this opportunity of expressing the sincere grief I feel for the loss of our beloved Rittenhouse. Genius, science, modesty, purity of morals, simplicity of manners, marked him as one of nature's best samples of the perfection she can cover under the human form. Surely no society, till ours, within the same compass of time, ever had to deplore the loss of two such members as Franklin and Rittenhouse: Franklin, our Patriarch, the ornament of our age and country, whom Philosophy and Philanthropy announced the first of men, and whose name will be as a star of the first magnitude in the firmament of heaven, when the memory of his companions of the way will be lost in the abyss of time and space. With the most affectionate attachment to their memory, and with sentiments of the highest respect to the Society, and to yourselves personally, I have the honor to be, Gentlemen,

Your most obedient,

And most humble Servant,

TH. JEFFERSON.

Messrs. SAMUEL MAGAW, JONATHAN WILLIAMS, WILLIAM BARTON, JOHN BLEAKLEY, } Secretaries of the American Philosophical Society.

LIST OF THE OFFICERS
OF THE
AMERICAN PHILOSOPHICAL SOCIETY,
For the Year 1799.

PATRON. The Governor of the Commonwealth for the time being—Thomas Mifflin.

PRESIDENT. Thomas Jefferſon, L. L. D.

VICE-PRESIDENTS.
- Caſper Wiſtar, M. D.
- Benjamin Ruſh, M. D.
- Robert Patterſon, A. M.

CURATORS.
- Charles Wilſon Peale.
- Benjamin S. Barton, M. D.
- Nicholas Collin, D. D.

COUNSELLORS.
- Robert Blackwell, D. D.
- Thomas M'Kean, L. L. D.
- James Davidſon, A. M.
- Adam Kuhn, M. D.
- Andrew Ellicott.
- Tench Coxe.
- James Abercrombie.
- Jonathan B. Smith, A. M.
- William Smith, D. D.
- William Currie, M. D.
- Samuel Wheeler.
- Jonathan Williams.

SECRETARIES.
- Thomas C. James, M. D.
- Adam Seybert, M. D.
- James Woodhouſe, M. D.
- Samuel H. Smith, A. M.

LIST

LIST of MEMBERS of the AMERICAN PHILOSOPHICAL SOCIETY, elected ſince January 1, 1794.†

AMERICAN MEMBERS.

JAMES Abercrombie.
Dr.———Bedford, Pittſburg.
Iſaac Briggs, Maryland.
Samuel Blair, D. D.
William Bache, M. D.
Tench Coxe.
Dr. Iſaac Cathrall.
Charles Caldwell, M. D.
Dr.———Deveze.
James Greenway, M. D. Virginia.
Dr.———Graſſe.
John Heckevelder, Bethlehem.
William Hamilton.
Dr. Hugh Hodge.
Thomas C. James, M. D.
Robert Leſlie.
Valentine Melſcheimer, Hanover, Pennſylvania.
Alexander Martin, North Carolina.
John F. Miſflin.
John Newnan, M. D. North Carolina.
John Nancarrow.
William Dandridge Peck, New Hampſhire.
Timothy Pickering.
Thomas Pinkney, South Carolina.
Thomas Mann Randolph, Virginia.
Richard Peters Smith. *
Adam Seybert, M. D.
Samuel Harriſon Smith.
John Stewart, Virginia.
Samuel Wheeler.
James Woodhouſe, M. D.
James Wilkinſon.

FOREIGN

† All thoſe whoſe places of abode are not ſpecified are of Pennſylvania.
* Deceaſed.

FOREIGN MEMBERS.

James Anderſon, L. L. D. Scotland.
M. Adet, Paris.
Earl of Buchan, P. S. S. A. Scotland.
John Frederick Blumenback, M. D. F. R. S. Goettingen.
Guſtaf Von Carleſon, Sweden.
M. F. H. Le Comte, Paris.
Joanne Baptiſta Cunat, Doctor of Civil Law, Valencia.
Earl of Dundonald, Scotland.
Louis Etienne Duhail, M. D. France.
Cypriano Riberio Friere.
John Guillemard, A. M. England.
Jacques Marie le Feſſior de Grandpre.
A. E. Van Braam Houckgeeſt.
Don Joſeph de Jaudennes, Valencia.
Alexander Leribours, France.
A. J. Laroque.
M. Mozard, France.
Julien Niemcewicz, Poland.
M. Talleyrand Perigord, France.
M. la Rochefaucault Liancour, France.
Edward Stevens, M. D. F. R. S. St. Croix.
James Edmund Smith, M. D. F. R. S.
J. B. Scandella, M. D. Venice.
Don Luis de Urbina, Valencia.
M. Volney, France.
E. A. W. Zimmerman, Brunſwick.
Franciſco de Zach, Saxa Gotha.

Donations received by the American Philoſophical Society ſince the Publication of their Third Volume of Tranſactions, with the Names of the Donors.

DONORS.	PRESENTS.
1793, Dec. 6. The Author,	Specimen Zoologiæ Geographicæ, quadrupedum domicilia et migrationes ſiſtens, &c. 4to. by Dr. Zimmerman, Brunſwick.
The Author,	Traité de l'elaſticité de l'eau, et d'autres fluides, 8vo. by Dr. Zimmerman.
1794, Feb. 21. Citizen *Genet*,	A Pamphlet in French, with a tranſlation, on a reform in the French Calendar.
The Author,	A Diſcourſe delivered before the Grand Lodge of Pennſylvania, by *Sam. Magaw*, D. D.
Mar. 7. Mr. *John Vaughan*,	The ceremonies and religious cuſtoms of the various nations of the known world, in ſix volumes Folio, by Bernard Picart.
do.	Swammardam's Hiſtory of Inſects, Folio.

1794. Donors.	Presents.
Mr. *John Vaughn*,	Hiſtory of the world, by Sir Walter Raleigh, Folio.
March 21. *J. C. Rediger*,	Hiſt. Eccles.Gentis Angl. Libri 5 a venerabili Beda, Folio.
do.	Acta Eruditor. Lipſiens. 1ſt. 2d. and 4th. volumes, 4to.
do.	Solom. Van Til. Theol. Dord. 4to.
May 2. The Author,	Geographical and hiſtorical deſcription of the United States of America, in German, by Profeſſor Ebeling of Hamburg, vol. 1. 8vo.
The Author,	American Biography, vol. 1. 8vo. by J. Belknap of Boſton, D. D.
Author,	A Treatiſe on the Synochus Icteroides, by Dr. Currie of *Philadelphia*.
Author,	Carey's Short Account of the late Malignant Fever in Philadelphia, 8vo.
May 16. Dr. *Nich. Collin*,	Hiſtoire des Celtes, &c. Par Simon Pelloutier, 8 vols. 8vo.

May

1794. DONORS.	PRESENTS.
May 30. Mr. *W. Roxburgh*, at Calcutta.	Sundry Asiatic plants, preserved at the country seat of Will. Hamilton, Esq. near Philadelphia.
Mr. *George Turner*,	Collection of Shells in the Territory North West of the Ohio.
June 20. Author,	Collection of State Papers, and other authentic documents, for an history of the United States of America, 2d. vol. 4to. by E. Hazard.
July 18. Author,	Glazing earthen vessels with lead, as a cause of many diseases, in German, by G. A. Ebell, Aulic counsellor, of Hanover, 8vo.
August 15. Author,	A Discourse delivered in the African church on the opening thereof, by Sam. Magaw, D. D.
Author,	Observations on the late Epidemic disease in Philadelphia, by Jean Deveze, M. D.
September 19. Author,	Stirpes Novæ Descriptionibus et Iconibus illustratæ, Folio, by C. L. Heritier, Reg. Consil.

1794. DONORS.	PRESENTS.
Oct. 24. Citizen *Fauchet*,	On Weights and Measures, by Citizen Dombey.
Nov. 7. Author,	A Systematical Treatise of arithmetic, by John Vinall, Boston, 8vo.
Author,	A Concise history of the human muscles, &c. 12mo. by Thom. Wright, Licent. of Roy. Coll. of Surgeons, Dublin.
Dec. 5. Mr. *Ebenez. Hazard*,	Act of Incorporation, laws, and circular letter of the Massachusetts historical society, 8vo.
Dec. 26. Author,	Experimental Researches concerning the philosophy of permanent colours, &c. by Edward Bancroft, M. D. F. R. S. 8vo.
Author,	A Treatise on Magnetism, with a description and explanation of a meridional and azimuth compass, &c. by Ralph Walker of *Jamaica*, 8vo.
1795.	
Jan. 16. M. *St. Mery*.	Four wooden locks of different kinds used by the country people of St. Domingo.

A Ball

1795. DONORS.	PRESENTS.
Jan. 16. M. *St. Mery*,	A ball of hair found in the ſtomach of a mule.
do.	A ſtone found in the ſtomach of a cow.
do.	An oriental *Bezoar*.
do.	A piece of ebony wood from Hiſpaniola.
do.	Some petrifactions of wood from Martinique.
do.	A little tooth of a whale.
do.	A cocoa-nut.
do.	A piece of copper ore from the Pyrenees.
do.	A piece of iron ore from do.
do.	Three ſpecimens of cochineal from Hiſpaniola.
do.	Fruit of courberit tree.
do.	An idol of the natives of Hiſpaniola.
do.	An inſect, called dragon from Martinique.
do.	A vegetable fly from St. Domingo.

A medal

1795. Donors.	Presents.
Jan. 16. M. *St. Mery*,	A medal ſtruck on the occaſion of Lewis XVI coming into the Aſſembly of the Electors of Paris, July 17. 1789.
Earl of *Buchan*,	Tranſactions of the ſociety of Scots Antiquaries, 1ſt vol. 4to.
do.	Duplicate of the minute book of the ſame ſociety, 1ſt vol. do.
do.	A caſt repreſenting the Earl of Buchan.
Feb. 6. Mr. *Cherachie*,	Buſt in marble of David Rittenhouſe, executed by Mr. Cherachie.
M. *St. Mery*,	Silver Medal of Lewis XV. ſtruck on the occaſion of the peace of 1763.
Mr. *John Beckley*,	Two elegant ſpecimens of printing in gilt letters, one a part of Magna Charta on ſattin, the other the Dream of Scipio on deep green ſattin.
April, 17. The Society,	Firſt part of vol. II. of the Memoirs of the American Academy of arts and ſciences, 4to.

Reports

1795. DONORS.	PRESENTS.
April 17. Mr. *Rich. P. Smith*,	Reports presented to the National Convention of France, with a Decree on the ſubject of weights and meaſures.
May, 15. Earl of *Buchan*,	A Box of Yew, on the lid of which is a correct picture of Copernicus, and on the inſide a drawing in pencil of Napier, by Brown.
Author,	Natural Principles of Rectitude for the conduct of man in all ſtates and ſituations of life. By Dan. Groſs, D. D. 8vo. New York, 1795.
Academy of Turin,	Vol. V. of the Memoirs of the Royal Academy of Sciences of Turin. 4to.
June, 19. Author,	Inaugural Diſſertation on the diſeaſe occaſioned by the bite of a mad dog, by James Meaſe, M. D.
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The Society having appointed a Committee to collect information respecting the past and present state of this country, the Committee during the last year addressed the following letter to such persons as were likely, in their opinion to advance the object of the Society.

[CIRCULAR.]

PHILOSOPHICAL HALL, PHILADELPHIA.

SIR,

THE American Philosophical Society have always considered the antiquity, changes, and present state of their own country as primary objects of their research; and with a view to facilitate such discoveries, a permanent committee has been established, among whose duties the following have been recommended as requiring particular attention.

1. To procure one or more entire skeletons of the Mammoth, so called, and of such other unknown animals as either have been, or hereafter may be discovered in America.

2. To obtain accurate plans, drawings and descriptions of whatever is interesting, (where the originals cannot be had) and especially of ancient Fortifications, Tumuli, and other Indian works of art: ascertaining the materials composing them, their contents, the purposes for which they were probably designed, &c.

3. To invite researches into the Natural History of the Earth, the changes it has undergone as to Mountains, Lakes, Rivers, Prairies, &c.

4. To

4. To inquire into the Cuſtoms, Manners, Languages and Character of the Indian nations, ancient and modern, and their migrations.

The importance of theſe objects will be acknowledged by every Lover of Science, and, we truſt, ſufficiently apologize for thus troubling you: for without the aid of gentlemen who have taſte and opportunity for ſuch reſearches, our means would be very confined. We therefore ſolicit your communications, now or in future, on theſe ſubjects; which will be at all times thankfully received, and duly noticed in the publications of the Society.

As to the firſt object, the committee ſuggeſt to Gentlemen who may be in the way of inquiries of that kind, that the Great Bone Lick on the Ohio, and other places where there may be mineral ſalt, are the moſt eligible ſpots for the purpoſe; becauſe animals are known to reſort to ſuch places.

With reſpect to the ſecond head, the committee are deſirous that cuts in various directions may be made into many of the Tumuli, to aſcertain their contents; while the diameter of the largeſt tree growing thereon, the number of its annulars and the ſpecies of the tree, may tend to give ſome idea of their antiquity. If the works ſhould be found to be of Maſonry; the length, breadth, and height of the walls ought to be carefully meaſured, the form and nature of the ſtones deſcribed, and ſpecimens of both the cement and ſtones ſent to the committee.

The beſt methods of obtaining information on the other ſubjects will naturally ſuggeſt themſelves to you; and we rely on a diſpoſition favourable to our wiſhes.

The

The Committee conſiſt of the following Gentlemen, viz.

THOMAS JEFFERSON, Preſident of the American Philoſophical Society, at Monticello in Virginia.

JAMES WILKINSON, Commander of the Army at Head Quarters.

GEORGE TURNER, of the Weſtern Territory, near Cincinnati.

Dr. CASPAR WISTAR, Vice Preſident of the A. P. S.
Dr. ADAM SEYBERT, Secretary of do.
C. W. PEALE, and
JON. WILLIAMS.
} in Philada.

Your communications may be addreſſed to any one of the Committee, but the articles you may think proper to furniſh ſhould be ſent to this place.

In behalf of the Committee,

I am reſpectfully,

Sir, your obedient ſervant,

——— ——— Chairman.

To ——— ———

CONTENTS

OF

VOLUME IV.

N°. Page.

I. *EXPERIMENTS and observations relating to the Analysis of Atmospherical air, by the* Rev. Dr. J. Priestley, 1

II. *Farther Experiments relating to the Generation of Air from Water, by* Rev. Dr J. Priestley, 11

III. *To determine the true Place of a Planet, in an Elliptical Orbit, directly from the mean Anomaly, by Converging Series, by* David Rittenhouse, L. L. D. *President* A. P. S. 21

IV. *On the Improvement of Time-keepers, by* David Rittenhouse, L. L. D. *President of the Society,* 26

V. *On the Expansion of Wood by Heat, in a Letter from* David Rittenhouse, L. L. D. *President of the Society,* 29

VI. *A Letter from* Mr. Andrew Ellicott, *to* Robert Patterson; *in Two Parts,* 32

VII. *Of the Aberration of the Stars, Nutation of the Earth's Axis, and Semi-annual Equation, by* Andrew Ellicott, 51

VIII. *A Letter from* Mr. Andrew Ellicott, *to* Mr. Robert Patterson. *A Method of Calculating the Eccentric Anomaly of the Planets,* 67

IX. *Method of raising the common Logarithm of any Number immediately, by* David Rittenhouse, *President of the Society,* 69

X. *Experiments on Evaporation, by* C. Wistar, M. D. 72

XI. *A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake, and other American Serpents. By* Benjamin Smith Barton, M. D. 74

XII. *Some account of an American Species of* Dipus, *or* Jerboa. By Benjamin Smith Barton, M. D. 114

XIII. *A Letter from* Mr. John Heckewelder, *to* Dr. Barton, *giving some account of the remarkable instinct of a bird called the Nine-Killer,* 124

XIV. *An Enquiry into the causes of the Insalubrity of flat and Marshy Situations; and directions for preventing or correcting the Effects thereof, by* William Currie, 127

XV. *Description of a Machine for saving Persons from the upper Stories of a House on fire, by* Nicholas Collin, D. D. *the Inventor; with a Drawing from the Model,* 143

No. Page.

XVI. *A Disquisition on wool-bearing Animals, by* Dr. James Anderson, *of North Britain, in a letter dated 6th December* 1794, 149

XVII. *Later Communication by the Author on this Subject, with a Sample taken from the Fleece of a Sheep brought from Jamaica to England,* 153

XVIII. *An easy and accurate Method of adjusting the Glasses of Hadley's Quadrant, on Land for the Back-Observation, by* Robert Patterson, *in a Letter to* Dr. David Rittenhouse, *President of the Society,* 154

XIX. *An Essay tending to improve intelligible Signals, and to discover an Universal Language. From an anonymous Correspondent in France, (probably the Inventor of the Telegraph) translated from the French,* 162

XX. *Memoir on the Subject of a new Plant, growing in Pennsylvania, particularly in the Vicinity of Philadelphia, by* Mr. Beauvois, 173

XXI. *A Letter from* Colonel Winthrop Sargent, *to* Dr. Benjamin Smith Barton, *accompanying Drawings and some Account of certain Articles, which were taken out of an ancient Tumulus, or Grave, in the Western-Country,* 177

XXII. *A Drawing of some Utensils, or Ornaments, taken from an old Indian Grave, at Cincinnati, County of Hamilton, and Territory of the United-States, North-west of the River Ohio, August 30th* 1794. *By* Colonel Winthrop Sargent. *Communicated by* Benjamin Smith Barton, M. D. 179

XXIII. *Observations and Conjectures concerning certain Articles which were taken out of an ancient Tumulus, or Grave, at Cincinnati, in the County of Hamilton, and Territory of the United-States, North-West of the River Ohio: in a letter from* Benjamin Smith Barton, M. D. *to the* Reverend Joseph Priestley, L. L. D. F. R. S. &c. 181

XXIV. *Barometrical Measurement of the Blue-Ridge, Warm-Spring, and Alleghany Mountains, in Virginia, taken in the Summer of the year* 1791, 216

XXV. *Miscellaneous Observations relative to the Western Parts of Pennsylvania, particularly those in the Neighbourhood of Lake Erie. By* Andrew Ellicott, 224

XXVI. *Observations made on the Old French Landing at Presqu' Isle, to determine the Latitude of the Town of Erie. In a Letter from* Andrew Ellicott, *to* Robert Patterson, *Secretary of the Society,* 231

XXVII. *Hints relative to the Stimulant Effects of Camphor upon Vegetables. By* Benjamin Smith Barton, M. D. 232

XXVIII. *Supplementum Indicis Floræ Lancastriensis. Auctore* Henrico Muhlenberg. Communicated by Dr. Barton, 235

XXIX. *On the Mode most easily and effectually practicable of drying up the Marshes of the maritime Parts of North America. By* Thomas Wright, *Licentiate of the College of Surgeons in Ireland, and Teacher of Anatomy,* 243

XXX. *A Memoir on the Discovery of certain Bones of a Quadruped of the Clawed Kind in the Western Parts of Virginia. By* Thomas Jefferson, *Esq.* 246

XXXI. *A Letter from* Mr. John Heckewelder, *to* Benjamin Smith Barton, M. D. *containing an Account of an Animal called the Big Naked Bear,* 260

XXXII. *Ex-*

N°. Page.

XXXII. *Experiments and Observations on Land and Sea Air. By* Adam Seybert, M. D. 262

XXXIII. *Translation of a Memoir on a new Species of Siren. By* M. de Beauvois, 277

XXXV. *Observations intended to favour a supposition that the Black Color (as it is called) of the Negroes is derived from the* Leprosy. *By* Dr. Benjamin Rush, 289

XXXVI. *An Improvement in Boats, for River-Navigation, described in a Letter to Mr.* Robert Patterson, *by* Nicholas King, 298

XXXVII. *General Principles and Construction of a Sub-marine Vessel, communicated by D. Bushnell of Connecticut, the inventor, in a letter of October,* 1787, *to* Thomas Jefferson *then Minister Plenipotentiary of the United States at Paris,* 303

XXXVIII. *The Description of a Mould-board of the least resistence, and of the easiest and most certain construction, taken from a letter to Sir John Sinclair, President of the board of agriculture at London,* 313

XXXIX. *Experiments upon Magnetism. Communicated in a letter to* Thomas Jefferson, *President of the Philosophical Society, by the Rev.* James Madison, *President of William and Mary College,* 323

XL. *Thermometrical Observations made at Fort Washington, Commencing June* 1790, *and Ending April* 1791. *By* Daniel Britt *and* G. Turner. *To which are added, for some time, the Rise and Fall of the Ohio. Communicated by* G. Turner, 329

XLI. *Calculations relating to Grist and Saw Mills, for determining the quantity of Water necessary to produce the desired effect when the Head and Fall are given in order to ascertain the dimensions of a new invented Steam Engine, intended to give motion to Water-wheels in places where there is no Fall, and but a very small Stream or Spring. By* John Nancarrow, 348

XLII. *Memoir on Amphibia. By* M. de Beauvois, 362

XLIII. *An Appendix to the two Articles in this Volume, (p.* 1.*) by* Dr. Priestley, *in a letter to* B. S. Barton, M. D. 382

LXIV. *An Inquiry into the comparative effects of the Opium Officinarum, extracted from the Papaver Somniferum or White Poppy of Linnæus; and of that procured from the Lactuca Sativa, or common cultivated Lettuce of the same author. By* John Redman Coxe, M. D. An Honorary Member of the Philadelphia Medical Society; and a Senior Member of the Chemical Society of Philadelphia, 387

LXV. *Experiments and observations, on the atmosphere of marshes. By* Adam Seybert, M. D. 415

LXVI. *An account of a Kettle for boiling Inflammable Fluids.—In a letter from* Thomas P. Smith, *to* Robert Patterson, 431

LXVII. *An Essay on a new Method of treating the Effusion which collects under the Scull after Fractures of the Head. By* J. Deveze, *Officer of Health, of the first class, in the French Armies,* 433

LXVIII. *Memoir on the Sand-hills of Cape Henry in Virginia. By* B. Henry Latrobe, *Engineer,* 439

LXIX. *Supplement to* Mr. Latrobe's *Memoir,* 444

LXX. *Ac-*

N°. Page.

LXX. *Account of* Chryſtallized Baſaltes *found in Pennſylvania—By* Thomas P. Smith, 445

LXXI. *Obſervations for determining the Latitude and Longitude of the Town of Natchez—By* Andrew Ellicott, *Eſq. Commiſſioner on the part of the United States, for running the line of Demarkation between them and the Spaniſh Territory. Communicated to the Society by* R. Patterſon, 447

LXXII. *An Anſwer to* Dr. Joſeph Prieſtley's *Conſiderations on the Doctrine of Phlogiſton, and the Decompoſition of Water; founded upon demonſtrative Experiments.* By James Woodhouſe, M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, &c. 452

LXXIII. *Philological View of ſome very Ancient Words in ſeveral Languages. By the* Rev. Nicholas Collin, D. D. Rector of the Swediſh Churches in Pennſylvania, 476

LXXIV. *Memoir on the Extraneous Foſſils, denominated* Mammoth Bones: *principally deſigned to ſhew, that they are the remains of more than one ſpecies of non-deſcript Animal. By* George Turner, Member of the A. P. S. Honorary and Correſponding Member of the Bath and Weſt of England Society, &c. 510

LXXV. *Deſcription of a Speedy Elevator. By the Inventor,* Nicholas Collin, D. D. *with two drawings from a model, repreſenting it folded and wound up,* 519

LXXVI. *A Deſcription of the Bones depoſited, by the Preſident, in the Muſeum of the Society, and repreſented in the annexed plates. By* C. Wiſtar, M. D. Adjunct Profeſſor of Anatomy, &c. in the Univerſity of Pennſylvania. 526

TRANS-

TRANSACTIONS

OF THE

American PHILOSOPHICAL SOCIETY, *&c.*

No. I.

Experiments and Obſervations relating to the Analyſis of Atmoſpherical air, by the Rev. Dr. J. PRIESTLEY.

Read Feb. 5, 1796.

IT is an eſſential part of the antiphlogiſtic theory, that in all the caſes of what I have called the *phlogiſtication of air* there is ſimply an abſorption of the dephlogiſticated air, or, as the advocates of that theory term it, the *oxygen* contained in it, leaving the phlogiſticated part, which they call *azote*, as it originally exiſted in the atmoſphere. Alſo, according to the principles of this ſyſtem, *azote* is a ſimple ſubſtance, at leaſt not hitherto analyſed into any other: They therefore ſuppoſe that there is a determinate proportion between the quantities of oxygen and azote in every portion of atmoſpherical air, and that all that has hitherto been done has been to ſeparate them from one another. This proportion they ſtate to be twenty ſeven parts of oxygen and 73 of azote, in 100 of atmoſpherical air.

 But

But in every caſe of the diminution of atmoſpherical air in which this is the reſult, there appears to me to be ſomething emitted from the ſubſtance, which the anti-phlogiſtians ſuppoſe to act by ſimple abſorption, and therefore that it is more probable that there is ſome ſubſtance, and the ſame that has been called phlogiſton, or the *principle of inflammability* (being common to all bodies capable of combuſtion, and transferable from any one of them to any other) emitted, and that this phlogiſton uniting with part of the dephlogiſticated air forms with it part of the phlogiſticated air which is found after the proceſs; and in ſome caſes there is more of this, and in others leſs. Alſo, in ſome caſes fixed air is the reſult of the union of the ſame conſtituent principles.

A mixture of iron filings and ſulphur, which, with a little water, has been commonly made uſe of to diminiſh and phlogiſticate air, and probably many other ſubſtances which produce the ſame effect, if they be continued in the air after the diminution has advanced to its *maximum*, occaſion an increaſe of the quantity, by an addition of inflammable air. This mixture I find to have the ſame effect when it is long confined in nitrous air, or in fixed air; and therefore it is probable that the ſame would be the caſe if it were confined in any other kind of air, or in vacuo. It therefore, ſeems natural to infer that the ſame principle which conſtitutes inflammable air was from the firſt exhaling from the mixture, but that it did not actually form inflammable air till there was no more dephlogiſticated air for it to unite with, and thereby form phlogiſticated air. The experiments from which this concluſion is drawn are recited in my former publications, and I have lately repeated them with particular attention, and the ſame reſult. I have alſo lately obſerved that by heating bones made black by burning without acceſs of air, in atmoſpherical air, there is, after the period

period of greateſt diminution, an increaſe of the quantity, and that it is then found to contain a mixture of inflammable air.

That ſomething is really emitted from the iron filings and ſulphur, when it is in a ſtate of diminiſhing air is evident from the ſtrong and offenſive *ſmell* which at that time this mixture has. Flowers alſo, and eſpecially thoſe which have the ſtrongeſt ſmell, I have obſerved to phlogiſticate air. Moreover, the iron filings and ſulphur when nearly dry, emit a viſible denſe vapour, which appears by its ſmell to be vitriolic acid air, which I have obſerved to have the power of diminiſhing and phlogiſticating air; owing no doubt, in part to its imbibing the dephlogiſticated part of it, and with it forming common vitriolic acid; but at the ſame time part of its phlogiſton may unite with another part of the dephlogiſticated air, and with it form phlogiſticated air.

Iron filings and ſulphur, as well as phoſphorus, and moſt of the other ſubſtances which have been generally uſed for the purpoſe of phlogiſticating atmoſpherical air, do likewiſe imbibe the dephlogiſticated air contained in it, and thereby gain an addition of as much weight as the air has loſt. But this is not the caſe with *black bones* heated in air, which by this means become white; and as nothing in them is volatile, except that which conſtitutes their blackneſs, I thought they would be a very convenient ſubſtance with which to make theſe experiments.

Theſe bones gained no addition of weight in the proceſs, and when they are uſed, the diminution of the air is by no means ſo great as in the other caſes, though the air that is left is completely phlogiſticated. This is probably in conſequence of the fixed air (formed by the union of the dephlogiſticated air with the phlogiſton emitted from the bones) not being readily imbibed by the water, or any other ſubſtance with which it is then in contact; ſo that a better opportunity is given to the phlogiſton emitted from the bones to unite with that air in a different manner, and form phlogiſticated air, which is therefore after the proceſs found in a greater proportion than in the other caſes, to which alone a due attention has hitherto been given. When theſe bones are heated over lime water, there is a copious precipitation of the lime. Here I would obſerve that the phlogiſton neceſſary to form this fixed air could only come from the bones in becoming white, as they had been calcined in as great a degree of heat as I could produce, ſo that no kind of air could have been expelled from them while excluded from acceſs of air.

Having by means of a burning lens heated 140.5 grains of well burned black bones in 23.75 ounce meaſures of air, it was reduced to 20 ounce meaſures, completely phlogiſticated, without any mixture of fixed or inflammable air in it. According to this experiment, the quantity of pure air in 100 ounce meaſures of atmoſpherical air was only 15.78 parts inſtead of 27.

Heating 267 grains of theſe bones in 30 ounce meaſures of air, it was reduced to 25.5 ounce meaſures completely phlogiſticated, which was in the proportion of 15 parts of dephlogiſticated air in 100 of atmoſpherical. In theſe experiments with bones there is ſometimes a ſmall loſs of weight, owing, I doubt not, to ſomething beſides phlogiſton being expelled from them by the intenſe heat of the lens; and during the proceſs I could perceive a ſlight vapour riſing from them. When I managed the heat ſo that it was not more than neceſſary to whiten the bones, they neither gained nor loſt any weight; at leaſt the loſs was very inconſiderable.

I had ſimilar reſults from experiments made with ſmall poliſhed *ſteel needles*. For when they were heated ſo as

only

only to become blue, and were not melted, they gained very little, if any, weight, and diminiſhed the air only in about the ſame proportion with the black bones.

Having by means of a burning lens heated 200 grains of the poliſhed needles in 24 ounce meaſures of air (in conſequence of which they became of a dark colour) they neither gained nor loſt any weight, and the air was reduced to 19.5 ounce meaſures, almoſt completely phlogiſticated. I heated the ſame quantity of theſe needles in 16.75 ounce meaſures of air, when it was reduced to 13.5 ounce meaſures, completely phlogiſticated without any mixture of fixed or inflammable air in it; ſo that the diminution was in the proportion of 19.4 parts in one hundred. In another experiment 24.75 ounce meaſures of air were reduced to 20.25 ounce meaſures nearly phlogiſticated. It is evident, therefore, from theſe experiments, that more phlogiſticated air is found after this proceſs, than after that with the iron filings and ſulphur.

Leſt, by heating the needles over a quantity of water they might attract moiſture, which cannot be expelled from them without ſome difficulty, I heated 200 grains of the ſame needles in the open air, till they had aſſumed exactly the ſame appearance with thoſe that had diminiſhed the air in the preceding experiments, and found that they had neither gained nor loſt any ſenſible weight. The ſame was the reſult of whitening a quantity of black bones in the open air. But to make this experiment with accuracy, the bones ſhould be calcined with the greateſt degree of heat, and made white with the leaſt.

In one experiment with very thin pieces of malleable iron (viz. iron turnings) 38.5 ounce meaſures of air were reduced to 31.5 meaſures, wholly phlogiſticated, which is in the proportion of the loſs of 19.5 parts in 100. I could not perceive that the iron had gained or loſt

loſt any weight; whereas, if it had imbibed the air that had diſappeared, or the water, of which, as I have ſhewn, the air principally conſiſts (as it would have done if it had been melted in the proceſs) it ought to have gained 4.2 grains.

There was not, however, the ſame certainty in the experiments with the needles, and ſtill leſs with the iron, as in thoſe with the bones. They generally gained a little weight, and diminiſhed the air more than the bones. The reaſon of this uncertainty might be that they were ſometimes heated too much; and ſometimes fine ſcales were thrown from them, which were indeed ſometimes viſible when, in floating about within the veſſel, they croſſed the ſun beams, and both in the experiments with the needles and thoſe with the bones a vapour viſibly roſe from them. When the needles were heated over lime water, a thick cruſt was formed upon it; but there was not ſuch a precipitation of the lime as in the experiments with the bones.

That the phlogiſtication of nitrous acid is owing, in ſome caſes, to its *imbibing* ſomething, and not always to its *parting* with any thing, which the antiphlogiſtians maintain is evident from its becoming phlogiſticated by imbibing nitrous air. This I have obſerved that it does with the greateſt rapidity, leaving in ſome caſes not more than one 18th part of the original quantity. M. Fourcroy ſuppoſes (*Philoſophie Chymique*, p. 76) that the converſion of the common nitrous acid into the phlogiſticated is always occaſioned by its parting with oxygen. That this is ſometimes the caſe I have demonſtrated in my experiments with heating it in long glaſs tubes; but in the preſent caſe it is not poſſible that the acid ſhould have parted with any thing, and leaſt of all with *oxygen*, ſince the ſmall reſiduum of nitrous air is pure *azote*. I ſhall here obſerve, what I did not before, that the abſorption of of nitrous air by nitrous acid is attended with a confiderable degree of heat.

That phlogifticated air, or azote, is not a fimple fubftance, but confifts of phlogifton (or whatever is the proper element of inflammable air) and of dephlogifticated air, is probable from feveral experiments that feem to have been overlooked by the antiphlogiftians, fuch as the following. A mixture of dephlogifticated and inflammable air being kept a long time was found by M. Metherie to contain a confiderable portion of phlogifticated air, as appeared by the difference of the refiduums after exploding a part of the mixture when firft made, and another part fome time afterwards. I had alfo found that a mixture of dephlogifticated and inflammable air fuffers a confiderable diminution in a courfe of time, though they will not wholly incorporate. But I have lately found that thefe two kinds of air unite completely by being confined fome time together in a moift bladder.

Having mixed equal quantities of thofe kinds of air, I put them into a bladder, which I left floating in a trough of water, and found, after about a fortnight, that the quantity was confiderably diminifhed; and examining it, I found it to be almoft wholly phlogifticated, though there was fomething flightly inflammable in it. On this I put equal meafures (but omitted to note the quantity) of each of the kinds of air into another bladder, and after about three weeks, found it reduced to 12.5 ounce meafures, all pure phlogifticated air, without any mixture of fixed or inflammable air that I could perceive.

I have likewife hit upon another method of forming phlogifticated air by the union of dephlogifticated and inflammable air, viz. by expofing the latter to a furface of rufted iron, which is known to become fo by imbibing pure air. Twenty ounce meafures of inflammable air were

were confined in a phial containing pieces of rufted iron from the 18th of Auguft to the 6th of October, when it was reduced to 9 ounce meafures, and was but flightly inflammable. The iron, from being red, was then become of a very dark colour. Another quantity of inflammable air treated in the fame manner from, I believe, the 6th of October, was in the 2d of December found to be completely phlogifticated. In thefe experiments the iron and the air were confined by water. Afterwards, putting 7 ounce meafures of inflammable air to pieces of rufted iron confined by mercury, it was, in about a week, almoft wholly abforbed. I then filled up the veffel again with inflammable air, and when the diminution proceeded no farther, I examined it, and found 5 ounce meafures of air completely phlogifticated.

Charcoal, as well as phlogifticated air, I have no doubt, contains the element of dephlogifticated air, as well as phlogifton, fince by its union with fteam it takes the form of fixed air, as well as that of inflammable air, and one element in the compofition of fixed air is dephlogifticated air. And when I made hot charcoal imbibe inflammable air by introducing pieces of it into jars of this air confined by mercury, and afterwards expelled it by plunging the charcoal in water, that which came out of it was phlogifticated air. Yet I think I recollect that the refult of this experiment has fometimes been inflammable air, the fame that the charcoal had imbibed.

I know of no cafe of the fimple abforption of air, but which, like that by water, refpects all kinds of air, though with a preference of that which is dephlogifticated; but not fo as to take this kind *only*, and leave all the phlogifticated air that was mixed with it. Otherwife it would have been in our power to afcertain with exactnefs the real proportion of both the kinds of air in the atmoſphere. For want of this the neareſt approximation that we can make appears to me to be by the uſe of nitrous air.

Since when two meaſures of pure nitrous air are mixed with one meaſure of pure dephlogiſticated air, they both, as nearly as poſſible, diſappear, and form nitrous acid, which is imbibed by the water in which the mixture is made, it is evident that little or no phlogiſticated air is neceſſarily formed in this proceſs; and when it is conducted properly, there will appear to be a much greater proportion of dephlogiſticated air in the atmoſphere than has been ſuppoſed, and enough to be converted into phlogiſticated air in the proceſs above mentioned. But a conſiderable *time* is neceſſary for this purpoſe; becauſe the diminution continues much longer than has been hitherto imagined.

The difference between the degree of diminution of atmoſpherical air by a mixture of nitrous air, with, or without, *agitation*, is very conſiderable. In general, without agitation, equal meaſures of each will occupy the ſpace of 1.25 meaſures, but with agitation only 1.01; and if the computation be made from this laſt *datum*, it will give the proportion of dephlogiſticated air to be 27 parts in 100, and conſequently that of the phlogiſticated air 73. But by keeping the mixture a longer time, the diminution will proceed to about 0.6 of a meaſure which will give 46.6 for the proportion of dephlogiſticated air, and 53.4 for that of the phlogiſticated air in the atmoſphere.

This diminution in the mixture of nitrous and atmoſpherical air, which is effected in the courſe of time, is various, depending, no doubt, on ſeveral circumſtances which I have not yet been able to aſcertain. What I have actually obſerved is as follows,

On the 21ſt of July I mixed equal quantities of nitrous and atmoſpherical air; when, with agitation, they oc-

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cupied the ſpace of 1.01. Examining the mixture at different times, I obſerved that the diminution kept advancing till ſome time before 24th of Auguſt, when it occupied the ſpace of only 0.545. Another mixture made in the ſame manner was 0.54, and another 0.65. At the ſame time I found other mixtures made without agitation, which at firſt occupied the ſpace of 1.25, were in one caſe 0.75, another 0.72 and another 0.65.

The reaſon why I apprehend the diminuton goes on ſo long is, that time is requiſite for the action of the phlogiſton in the nitrous air upon the dephlogiſticated part of the atmoſpherical air, in order to the converſion of the whole of it into nitrous acid, in conſequence of this part being intimately diffuſed through the phlogiſticated part, by which it is, as it were, protected from its action, which is ſimilar to many other chemical proceſſes. It is for the ſame reaſon that the diminution is much greater with agitation than without it, as the parts diſpoſed to unite are thereby brought into better contact.

When atmoſpherical air is exploded together with inflammable air, the diminution never proceeds ſo far as when nitrous air is mixed with it; becauſe in this caſe phlogiſticated air, as well as nitrous acid, is formed by their union; and, as I have ſhewn, the greater is the proportion of the inflammable air employed, the greater will be the proportion of phlogiſticated air in the reſiduum. This mixture, however, will go on diminiſhing for ſome time, though not ſo far as that with the nitrous air; becauſe part of this produce being nitrous acid, as I have ſhewn in a former courſe of experiments, it will require time to be formed, as well as when the nitrous air is employed.

Having made a mixture of equal parts of inflammable and atmoſpherical air, and exploded them on the 3d of Auguſt, I obſerved that it then occupied the ſpace of

1.35

1.35 meafures, and on the 2d of September, when I perceived that the diminution would proceed no farther, it was 1.14 which, though confiderable, was far fhort of the diminution produced by an equal bulk of nitrous air.

Though, in the experiments recited above with the *calcined bones*, and the *fteel*, neither of thefe fubftances appeared to have loft any weight that I was able to afcertain, it does not follow that nothing was emitted from them. For *light* and *heat* are almoft univerfally allowed to be *fubftances*, though no perfon has been able to weigh them. Befides the quantity of the materials that I made ufe of might be too fmall for the purpofe. What is moft important in the experiments is that, fince the diminution of the air was effected by heating thofe fubftances, and they did not *gain* any weight in the procefs, the phlogiftication of air is not the abforption of any part of it by the fubftance which produces that effect, as the antiphlogiftic theory fuppofes.

N°. II.

Farther Experiments relating to the Generation of Air from Water, by Rev. Dr. J. Priestley.

Read Feb. 19, 1796. IN a late publication, containing an account of fome experiments relating to *the generation of air from water*, I mentioned three different proceffes in which air was produced from the fame water, without any perceivable limit.

The firft procefs was converting the whole of a quantity of water into fteam, in the common method of boiling; when I found that, though the water had been boiled ever ſo long, or ever ſo often, air continued to be produced from it.

In order to obviate the objection to the water having imbibed the air from the atmoſphere, in a ſecond proceſs I put the water on which I operated into long glaſs tubes, over a column of mercury; and after producing air by keeping the upper part of the tube containing the water a long time in the form of vapour, I let out the air ſo procured under mercury, by which means the water never came into any contact with the air of the atmoſphere, and yet it continued to yield air whenever the proceſs was repeated, without any perceivable diminution, or limit.

In the third proceſs, no heat was uſed, but the water was put into a glaſs veſſel conſiſting of a large bulb, connected with a tube the full length of a barometer, a quantity of mercury ſufficient to fill the tube being put into it along with the water, and then inverted, and placed in a baſon of mercury. By this means the preſſure of the atmoſphere was removed from the water, and thus the air naturally contained in it eſcaped, and lodged on the ſurface of the water; and by inverting the veſſel again, it was thrown out into the open air. This proceſs I kept repeating with the ſame water more than a year, and yet, as in the former proceſſes, I found freſh air always produced from it, and ſeemingly in an equable manner.

It has been ſaid that, in this proceſs, the water, deprived of all air, inſtantly ſeizes upon ſome the moment that the newly extricated air is thrown out, the ſurface of the water in the tube being then, though but for a moment, expoſed to the atmoſphere. But this ſuppoſed eager attraction of air by the water would have made it to abſorb the newly produced air, if not in its rarified

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ſtate on the ſurface of the water, yet when it was condenſed, on inverting the tube, during the time that it was paſſing the whole length of the tube, as readily as freſh air from the atmoſphere. Beſides, it requires a conſiderable time before the water thus deprived of all air will abſorb that which has been produced, or extricated, from it, when the veſſel is inclined, and conſequently the preſſure of the atmoſphere removed.

Alſo, in order to obviate this objection, I kept the extremity of the tube carefully covered with my finger all the time that it was inverted till the moment that the air muſt be let out, and mercury put in, ſo that it was not expoſed to the atmoſphere ſo much as the tenth of a ſecond; and yet I found repeatedly, that the air was produced as readily as when it had been expoſed to the atmoſphere (as I ſometimes purpoſely did it) ſeveral minutes.

I would farther obſerve, that, in this proceſs, if the veſſel containing the water and mercury be inverted, and a vacuum appear, as it inſtantly will, in the form of a bubble, for ever ſo ſhort a time, a perceivable bubble of permanent air will be produced. I do not therefore ſee but that, by means probably of heat, air is producible from the ſame water without any limit.

In order to make any quantity of water as free from air as poſſible, *agitation* is neceſſary. But when by the frequent repetition of this proceſs the greateſt effect has been produced, and the air, or vapour, has remained long upon the water, agitation will diminiſh it, part of the newly generated air being imperfectly formed, and more readily imbibed by the water than that which had been a longer time in the ſtate of air. This diminution of the bulk of a bubble of air by agitation appears to be the moſt certain teſt of as perfect an extrication of air from water as can be attained. But even after this, whenever the bubble of air is let out, and the veſſel is inverted, another bubble is inſtantly formed, ſometimes indeed ſo ſmall as not to be viſible to the naked eye, but always by means of a magnifying glaſs, and this very ſmall quantity will not be abſorbed by the water till the veſſel has been laid in an inclined poſition ſome hours. If the veſſel be placed perpendicularly, the bubble will come to be of a conſiderable ſize. Still however it will not increaſe beyond a certain quantity, though it remain in that poſition ever ſo long.

I have tried every method that I could think of to deprive water of its power of producing air, but without effect. *Heat* I found of no uſe but to aſſiſt in expelling the air originally contained in it, and freezing had no more effect than heat.

When I publiſhed the pamphlet above mentioned I had not procured from water any other kind of air than ſuch as was, in a greater or leſs degree, purer than that of the atmoſphere, and therefore I imagined that this might have been the origin of all the air in the atmoſphere. But I have ſince found that though the firſt quantity of air that is expelled from water is much purer than that of the atmoſphere, the next is leſs pure, and at laſt it is wholly phlogiſticated. This I could not diſcover while I made uſe of ſmall bulbs; but when I uſed large ones, containing from fifty to an hundred ounces of water, it was aſcertained with the greateſt certainty. From this fact it may be inferred, either that the air produced from water is not that which had been imbibed from the atmoſphere, or that, though it imbibes moſt readily that which is the pureſt, it retains with the greateſt obſtinacy that which is leaſt pure, which is analogous to other chemical affinities. If the air thus produced be really generated from the water, or rather vapour, it muſt be wholly phlogiſticated, and afterwards purified by the

proceſs

proceſs of vegetation; or the phlogiſticated part alone of the atmoſphere may have had that origin, and the dephlogiſticated part have come from vegetation.

I once thought that a very ſmall quantity of any of the *acids* enabled water to yield more air than it would do without them, and while I uſed only ſmall bulbs, I continued to think ſo; but when I uſed the larger veſſels above mentioned, I could not perceive any ſenſible difference in the reſults in conſequence of this circumſtance.

The quantity of air extricated from diſtilled water before the production becomes equable is about one fortieth of its bulk.

Wiſhing to leave nothing undone that I was capable of doing with reſpect to this courſe of experiments, I have, ſince the publication of the tract above mentioned, endeavoured to convert the whole of a ſmall quantity of water into air, but it has been without effect. Having provided barometer tubes with bulbs connected with them, from one inch to three inches in diameter, I firſt put into them a ſmall quantity of water, and then filling them with mercury, left them ſome time with the orifices of the tubes upwards, in order to give the water an opportunity to riſe to the top. I then inverted them, when after ſome time a very ſmall quantity of water would be viſible on the ſurface of the mercury in the tube, and the vapour ariſing from it in *vacuo* would, of courſe, be diffuſed through the whole of the bulb above it. After this, inclining the veſſel, and making it lean over a fire, that ſmall quantity of water was wholly converted into vapour, ſo as to cauſe the mercury to deſcend, and leave both the whole of the bulb, and part of the tube, filled with hot vapour, and in this ſtate I kept it ſeveral hours. After this I always found a quantity of air produced, and this I threw out, by inverting the veſſel.

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Then exposing it again to the heat, I never failed to ge more air; and having done this, in some cases, not less than twenty or thirty times, I was satisfied that even the smallest quantity of water will never cease to yield air, and in several cases I have by this means procured more air than the bulk of the water.

As some water would necessarily insinuate itself between the mercury and the glass, I exposed almost the whole of the tube containing the mercury to the heat; by this means converting that water into vapour, and making it ascend to the top of the mercury; then throwing out both the water and the air produced from it again and again, I at length found nothing but air above the mercury. Still, however, the whole of the water was not converted into air. For when, by means of heat, the mercury was made to descend, the water which had been confined between the mercury and the glass made its appearance, though by the ascent of the mercury it would again disappear.

I have also found that when there was any sensible quantity of water above the mercury, and have exposed it to heat day after day, the quantity of air, in this case as well as the preceding without heat, came to a *maximum*, and no repetitions of the process would increase it. This induces me to conclude that the longest continuance of any quantity of water in the state of vapour would not convert it into air. It may, however, be worth while, if there should be an opportunity of doing it without much expence, to make the experiment.

The purest distilled water should be used in these experiments. Instead of this, I once used pump water; but found that, after the production of air was advanced to its maximum, it began to yield a considerable quantity, at least ten times more than it had done before, at the same time becoming a little turbid. But when it was clear, it ſtill yielded much more air than diſtilled water. Probably ſome calcareous matter diſſolved in the water was decompoſed in this proceſs, and the air contained in it had increaſed the bulk of that which had been produced by means of the water.

Having, in the manner above mentioned, found an eaſy method of expelling from a quantity of water all the air contained in it, I wiſhed to know what would be the reſult of making it imbibe different kinds, and various mixtures, of air. I had before found that water deprived of its air by boiling would imbibe any kind of air, and that when this air was again expelled by heat, the quality of it was not changed; but I could now both expel the air more effectually, and make it imbibe any particular kind of air with more certainty and expedition. For this purpoſe, having firſt expelled the air, by removing the preſſure of the atmoſphere in the manner deſcribed above, I inclined the veſſel, laying it in a poſition nearly horizontal, with the end of the tube immerſed in a baſon of mercury; and then having introduced the air that I wiſhed it to imbibe, I gently agitated the veſſel, and the preſſure of the atmoſphere being now removed, the water would pretty ſoon ſaturate itſelf with the air. After this, the veſſel being placed upright, the air which it had imbibed was preſently diſcharged, without any application of heat.

In this method, beginning with atmoſpherical air, which conſiſts of a mixture of dephlogiſticated and phlogiſticated air, I found that water imbibes the former in preference to the latter, but not wholly unmixed with it. Having made 45 ounces of diſtilled water free from air, I put to it $2\frac{3}{4}$ ounce meaſures of atmoſpherical air, of which, by agitation, it imbibed three fourths of a meaſure, when the remaining two ounce meaſures were found to be of the ſtandard of 1.15 inſtead of 1.01

which was the ſtandard of the air before the proceſs; that is, when one meaſure of this air was mixed with one meaſure of nitrous air, it occupied that ſpace. When the air that had been imbibed was expelled from the water, it was of the ſtandard of 0.75. Both mixed together were exactly of the ſtandard of atmoſpherical air.

I had thought that, though deplogiſticated and inflammable air will not unite while they retain their aerial form, without a red heat, they might do ſo when they were both deprived of that form, by being combined with water, and make phlogiſticated air; I therefore made a quantity of water deprived of all air imbibe a mixture of equal quantities of thoſe two kinds of air. But when this mixed air was expelled from the water, it was fired with an exploſion, ſo that no union had been formed between them. I then made a quantity of water imbibe the two kinds of air one after the other, but there was no difference in the reſult. The air that was expelled from the water was ſtill fired with one exploſion.

But dephlogiſticated and nitrous air, which unite without heat in their aerial form, did the ſame when they were combined with water. Having expelled all its air from a large quantity of diſtilled water, I firſt made it imbibe as much as it could of nitrous air, and after that of dephlogiſticated air, and obſerved that what remained of each, not abſorbed by the water, was very little changed. Then, expelling the air from the water thus doubly impregnated, the firſt quantity procured was dephlogiſticated, though not ſo pure as before; the ſtandard of the proceſs with two equal quantities of nitrous air being 0.6, whereas before it had been 0.2. The ſtandard of the ſecond expulſion of air was 0.4. Afterwards it was 0.8, then 1.0; and thus it would, no doubt, have proceeded, till it had been wholly phlogiſticated; but no part of it had the property of nitrous air. This kind

kind of air that had been imbibed muſt have united with as much of the dephlogiſticated air contained in the water as it could ſaturate, and thus have formed nitrous acid, which remained in the water, while the ſuperfluous dephlogiſticated air had been expelled in the proceſs.

I then firſt ſaturated the water with the dephlogiſticated air, and after that with the nitrous air, which it imbibed very readily; and expelling the air afterwards, found it to be purely nitrous, there having been more nitrous air employed at this time than was ſufficient to ſaturate the dephlogiſticated air.

Having made the preceding experiments with *water*, I wiſhed to extend them to other liquid ſubſtances, and began with *ſpirit of wine*, which I had before found to be convertible into inflammable air by a red heat, and alſo by the electric ſpark. I now find that ſo great a degree of heat is by no means neceſſary for this purpoſe.

If I fill one of the bulbs above mentioned with the ſpirit, and by means of a column of mercury take off the preſſure of the atmoſphere, a very great quantity of inflammable air is immediately diſcharged from it, and by a repetition of the proceſs a ſmaller quantity never fails to be produced, and as far as I ſee without limits.

If in this ſtate I expoſe the ſpirit to a degree of heat ſufficient to convert it into vapour, a very great proportion of it is preſently converted into air, and in a few minutes the quantity produced will be ten or twenty times the bulk of the liquid. This is the caſe repeatedly with the ſame ſpirit, ſo that I have no doubt but that, in time, the whole of it would be ſo converted, juſt as if it had been expoſed to a red heat in paſſing in the form of vapour though a red hot earthen tube.

Having expelled a very great quantity of air from one of the bulbs filled with ſpirit of wine, of the ſpecific gravity

gravity of 682.5, I expofed it to the atmofphere, after which it yielded as much as before, viz. about one third or one fourth of its bulk, all ftrongly inflammable. I had the fame refult in the fubfequent procefs. After another, the air was exploded like a mixture of inflammable and atmofpherical air, and the next produce burned with a lambent flame. Being then examined, its fpecific gravity was 692.4; fo that it had acquired fome weight by imbibing atmofpherical air.

Having, in like manner, expelled air which I found to be inflammable from a quantity of *fpirit of turpentine*, I made it imbibe atmofpherical air, and expelling it again, found it to be not inflammable, but phlogifticated. This fluid had alfo gained fomething in its fpecific gravity by the procefs.

The only objection that, after giving much attention to the fubject, I think can be made to the conclufion that I firft drew from thefe experiments, viz. that air is actually produced from water, is the very fmall quantity that is produced in proportion to the bulk of the water, after the air naturally contained in it is wholly expelled. But if it fhall appear, after a long courfe of time, that this fmall production of air from the fame water is conftant, and equable, I do not fee how the conclufion, extraordinary as it may be thought, can be difputed. This air being wholly *phlogifticated* is a fufficient proof that the air fo produced is not abforbed from the atmofphere in the courfe of the procefs. For then it would have been dephlogifticated, or at leaft purer than that of the atmofphere, which water always feizes upon in preference to that which is impure,

No. III.

To determine the true Place of a Planet, in an Elliptical Orbit, directly from the mean Anomaly, by Converging Series, by DAVID RITTENHOUSE, L. L. D. *President* A. P. S.

Read Feb. 5, 1796.

LET $x =$ the eccentricity, y the mean anomaly in the arch of a circle the radius whereof is 1. And a, an arch required.

For the upper half of the orbit, let $\frac{x}{x+1}, = n$, and $\frac{y}{x+1}, = z$.

Then $a = z + \frac{n}{6}z^3 + \overline{\frac{nn}{12} - \frac{n}{120}}\,z^5 + \overline{\frac{nnn}{18} - \frac{nn}{90} + \frac{n}{5040}}\,z^7 + \overline{\frac{55n^4}{1296} - \frac{11n^3}{864} + \frac{41nn}{60480} - \frac{n}{362880}}\,z^9$ &c.

Find the log. of the natural cosine of a, and the log. of the same cosine $+ x$, and add the difference of these two logarithms, and likewise the complement of the log. of the conj. semidiameter, and the log. cotang. of a, together, the sum will be the log. cotang. of the true anomaly.

For the lower half of the Orbit.

Let y, be the mean anomaly from the lower apsis, $\frac{x}{1-x} = n$, and $\frac{y}{1-x} = z$.

Then $a = z - \frac{n}{6}z^3 + \overline{\frac{nn}{12} + \frac{n}{120}}\,z^5 - \overline{\frac{nnn}{18} + \frac{nn}{90} + \frac{n}{5040}}\,z^7 + \overline{\frac{55n^4}{1296} + \frac{11n^3}{864} + \frac{41nn}{60480} + \frac{n}{362880}}\,z^9$ &c.

Take the difference between the log. of the nat. cosine of a, and the log. of the same cosine $- x$, and subtract this diff. — the comp. abovementioned from the log. cotang. of a, the remainder is the log. cotang. of the true anomaly, counted from the lower apsis.

If the co-efficients prefixed to the powers of z, be computed for any particular orbit, and their logarithms used instead of the numbers themselves, the calculation will afterwards be very simple for any degree of mean anomaly in that orbit, as will appear by the following example.

In the very elaborate tables of Mr. Zach, published in 1792 the eccentricity of the Earth's orbit is assumed .0167923, consequently log. of the lesser semidiameter will be — 1.9999387, its complement .0000613, and the log. of $n = -2.2178779$ and the series for the upper half of the orbit

will be, $a = a + -3.4397266.\ z^3$
$- -4.0603053.\ z^5$
$+ -7.6959472.\ z^7$
$- -8.9982252.\ z^9$ &c.

The negative sign prefixed to these logarithms affects the index only.

For the lower half, $a = z - -3.4543136.\ z^3$
$+ -4.2217638.\ z^5$
$- -6.8392607.\ z^7$
$+ -7.4939405.\ z^9$ &c.

For the logarithm of the distance in any part of the Orbit.

To the log. sine of a, add the comp. of the log. sine of the true anomaly — the comp. of the log. of the conjugate semidiam. the sum will be the true log. of the distance.

Example of the Calculation.

The Sun's mean anomaly being II^s 6° 30′ required the true anomaly or equation.

Arch of 66° 30′ = 1.16064395 =
y, log. = .0646990
Sub. $1 + x$ log. .0072323
log z = .0574667 z = 1.1414758

z^3 = .1724001
$+ -3.4397266$
$-3.6121267 = + .0040938.01$

z^5 = .2873335
$+ -4.0603053$
$-4.3476388 = - .0002226.58$

z^7 = .4022669
$+ -7.6959472$
$-6.0982141 = + .0000012.54$

z^9 = .5172003
$+ -8.9982252$
$-7.5154255 = - .0000003.27$

$+ 1.1455708.55$
$- .0002229.85$

$a = 1.1453478.7 = 65°\ 37'\ 24''.96$

Log.

Nat. cofine of a = .4127292.5 log. = — 1.6156652
Eccent. + .0167923.

.4295215.5 log. = — 1.6329849

Diff. log. = .0173197
Comp. of conj. femidiam. log. = . 613
Cotang. of a, 65° 37′ 24″.96 = 9.6562166

Cotang. of true anomaly 64° 45′ 0″.8 = 9.6735976

Log. fine a = 9.9594487
Log. fine 64° 45′ 0″.8 comp = .0436122

10.0030609
Comp. of log. conj. — 613

Log. diftance = 10.0029996

Hence the equation is 1° 44′ 59″.2. In Zach's tables it is 1° 44′ 59″.25.

The feries above given converge flowly when the mean anomaly is near 3 S. or 9 S. In this cafe the true anomaly may be obtained with great accuracy by a feries derived from that which expreffes the cofine in terms of its correfpondent arch, as follows,

Subtract the eccentricity from the mean anomaly and call the remainder R. Let the difference between R and 90° be = z,

Then $R + \frac{x}{2} zz \pm \frac{xx}{2} z^3 - \overline{\frac{x}{24} - \frac{5xxx}{8}}\, z^4 \mp \overline{\frac{xx}{8} - \frac{7x^4}{8}}\, z^5 + \overline{\frac{x}{720} - \frac{7xxx}{24} + \frac{21x^5}{16}}\, z^6$ &c. $= a$

And a will be the true anom. counted from the upper apfis.

For the Earth's Orbit.

$a = R + —3.9240802.\ zz$
$\pm —4.1491904.\ z^3$
$\mp —4.8430581.\ z^4$
$\mp —5.5462722.\ z^5$
$+ —5.3413007.\ z^6$
$\pm —6.4890406.\ z^7$

The uppermoft of the figns prefixed to the 3d 5th and 7th powers of z muft be ufed when R exceeds 90° and the lower fign when R is lefs than 90°.

Examples of Calculation.

Sun's mean anomaly 60° 50′ required the true anom. and equation.

60° 50′ = 1.06174196
— x = .0167923
R = 1.04494966
90° = 1.57079633
z = .52584667

Log. z = — 1.7208591
zz = — 1.4417182
\+ — 3.9240802
— 3.3657984 +

z^3 = — 1.1625773
\+ — 4.1491904
— 5.3117677 —

z^4 = — 2.8834364
\+ — 4.8430581
— 5.7264945 —

z^5 = — 2.6042945
\+ — 5.5462722
— 6.1505667 +

z^6 = — 2.3251546
\+ — 5.3413007
— 7.6664553 +

z^7 = — 2.0460137
\+ — 6.4890406
— 8.5350543 —

R = 1.04494966
\+ 232166
\+ 141
\+ 46
1.04727319
Sum of — 7380
a = 1.04719939 = 60° 0′ 0″.38

Nat. cosine = .4999984 Log. = .6989686
\+ x .0167923
.5167907 Log. = .7133147
Diff. = .0143461
\+ 613
Cotang. 60° 0′ 0″.38 = 9.7614376
Cotang. true anomaly 59° 10′ 13″.1 = 9.7758450
Equation — 1° 39′ 46″.9.

The

The Sun's mean anomaly 120° 50′ required the true anomaly and equation.

$$
\begin{aligned}
120^\circ\ 50' &= 2.10893951 \\
-x &= .0167923 \\
R &= 2.09214721 \\
90^\circ &= 1.57079633 \\
z &= .52135088 \\[1em]
\text{Log. } z &= -1.7171301 \\
zz &= -1.4342602 \\
+ &- 3.9240802 \\
&- 3.3583404 = +.00228213 \\[1em]
z^3 &= -1.1513903 \\
+ &- 4.1491904 \\
&- 5.3005807 = +.00001998 \\[1em]
z^4 &= -2.8685204 \\
+ &- 4.8430581 \\
&- 5.7115785 = -.00005147 \\[1em]
z^5 &= -2.5856505 \\
+ &- 5.5462722 \\
&- 6.1319227 = -.00000135 \\[1em]
z^6 &= -2.3027806 \\
+ &- 5.3413007 \\
&- 7.6440813 = +.00000044
\end{aligned}
$$

Sum of + = .00230255
Sum of — = — 5282
+ .00224973

$$
\begin{aligned}
R &= 2.09214721 \\
+ &\quad\ \ 224973 \\
&2.09439694 = 120^\circ\ 0'\ 0''.38
\end{aligned}
$$

Comp.

Log.

Comp. to $180° = a = 59° \ 59' \ 59''.62$ cos. .5000015.9 = .6989714

$-x =$ .0167923.

.4832092.9 = .6841352

diff. = .0148362

Cotang. 59° 59' 59″.62 = 9.7614412

9.7466050

\+ 613

Tang. 29° 9' 48″ = 9.7466663

\+ 90

True anomaly 119° 9' 48″

Mean anom. 120 50 00

Equation — 1° 40' 12″

If the 1ſt and 6th 60° of mean anomaly in the Earth's orbit be computed by the firſt ſeries, the 3d and 4th 60° by the ſecond ſeries, and the 2d and 5th by the laſt ſeries, no more than the firſt 3 terms containing powers of z, need be uſed, for the equation cannot be had true to $\frac{1}{100}$ of a ſecond without tables of logarithms carried farther than to 7 places.

N°. IV.

On the Improvement of Time-keepers, by DAVID RITTENHOUSE, L. L. D. *Preſident of the Society.*

Read Nov. 7, 1794.

THE invention and conſtruction of time-keepers may be reckoned amongſt the moſt ſucceſsful exertions of human genius. Pendulum clocks eſpecially, have been made to meaſure time with aſtoniſhing accuracy; and if there are ſtill ſome cauſes of inequality in their motions, the united efforts of mechaniſm, philoſophy and mathematics will probably in time remove them.

The laſt and leaſt of thoſe cauſes, which perhaps may be worthy of notice when all others of more importance are

are removed, is that arising from the unequal density of the air, which by varying the actual weight of the pendulum will accelerate or retard its motion The effects arising from this cause will indeed be found very small, for if we suppose the greatest range of the barometer to be three inches, which indicates a change of density in the air of about one tenth of the whole; and supposing lead, of which pendulums are generally made, to be 8,800 times heavier than air, the variations of the actual weight of a pendulum may be one-88000th part of its whole weight, and consequently the change in its rate of going one-176000th part. And, as there are 86,400 seconds in a day, the clock may vary in its rate of going, from this cause, about $\frac{1}{2}$ a second in 24 hours. Mentioning the barometer seems naturally to point out a remedy for this cause of irregularity by means of that instrument. But my design is at present to describe a very different and extremely simple method, which though only a matter of curiosity at present, may at some future time perhaps be found useful; especially as the variation above mentioned is governed solely by the actual density of the surrounding air, and the barometer can only give the weight of an intire column, which does not strictly correspond with the density of its base; whereas the method I propose depends on the real density of the air surrounding the pendulum, and nothing else.

Let AB (Plate I. Fig. 9.) be a pendulum vibrating on the point A, and removed from the perpendicular line DE. Let the inflexible rod be continued from BA to C, and let a body C, of equal dimensions with the pendulum B, but hollow and light as possible, be fixed on the rod, making AC equal to AB. Now it is evident that B will be pressed upwards by a force equal to the weight of its bulk in air, and its descent retarded. But the body C, will be equally pressed upwards, and consequently the motion of the pendulum

pendulum towards the perpendicular will be as much accelerated. Theſe two forces therefo e will deſtroy each other, and the pendulum will perform its vibrations in equal times, whether the air be light or heavy, denſe or rare.

I have for greater perſpicuity deſcribed the moſt ſimple caſe, but perhaps not the moſt eligible, for if we can enlarge the veſſel or body C, in any proportion, the diſtance of its center from A may be diminiſhed at the ſame rate.

However plauſible the above may appear in theory, no doubt difficulties will occur when we attempt to reduce it into practice. But I am perſuaded they will not be found inſuperable.

The only experiment I have hitherto made on this ſubject has been merely to ſhew that a pendulum can be made in this manner which ſhall vibrate quicker in a denſe medium than in one more rare, contrary to what takes place with common pendulums.

I made a compound pendulum on the principles above mentioned, of about one foot in its whole length. This pendulum, on many trials, made in the air 57 vibrations in a minute. On immerſing the whole in water it made 59 vibrations in the ſame time, ſhewing evidently that its motion was quicker in ſo denſe a medium as water than in the air. When the lower bob or pendulum only was plunged in water it made no more than 44 vibrations in a minute. The remaining 15, being ſolely the effect of the preſſure of the water againſt the upper veſſel C.

N°. V.

On the Expansion of Wood by Heat, in a Letter from DAVID RITTENHOUSE L. L. D. *President of the Society.*

May 15th, 1795.

IN the prefent ftate of experimental philofophy it is well known that bodies in general enlarge their dimenfions, or expand, on being heated, and contract in cooling. From fome experiments heretofore made, wood has been thought to make an exception to the general rule, and this opinion has fo far prevailed that many curious perfons have applied wooden pendulum rods to their timepieces, to prevent the variation in their rate of going, arifing from the expanfion and contracting of a metal rod. From my own obfervations, however, as well as thofe of fome of my friends, the wooden pendulum rod does not appear to anfwer the expectations formed on it. I had in my poffeffion for feveral years an excellent timepiece made for this fociety by an ingenious workman and worthy member of the fociety. The refult of my conftant attention to this clock was, that though its regular variations with heat and cold were probably much lefs than thofe of metal pendulums, it neverthelefs always went fafter in winter than in fummer, and was liable to very fudden and confiderable variations; arifing, no doubt, from the combined effects of heat and cold, moifture and drynefs. This determined me to make fome careful experiments with a pyrometer capable of receiving a piece of wood of the length of a fecond pendulum. Several years ago I made fome experiments of this kind, perfectly correfponding with thofe I have lately made, and which I now communicate to the fociety.

I took a ſtraight grained piece of white hiccory, green, for I could not procure any ſeaſoned, its length 39 inches, and about $\frac{3}{8}$ of an inch ſquare. This I placed in my pyrometer, and kept it fully extended by a weight faſtened to a ſtring, going over a pulley. To the pyrometer I applied the tube and glaſſes of a good compound microſcope, and a micrometer, the value of the ſmaller diviſions whereof I found to be nearly .00053 parts of an inch, each.

The rod of wood being placed in the pyrometer, I poured ſand all around it, heated to about 250 of Fahrenheit, which degree of heat I found the wood would bear without ſcorching. On pouring in the hot ſand, the rod expanded very much, but ſoon began to contract, even before the ſand was ſenſibly cooled, which I ſuppoſe aroſe from the hot ſand extracting the moiſture of the wood. It continued to contract as the whole grew cool, ſo that when the rod had acquired its firſt temperature it was near 30 of the above diviſions ſhorter than at firſt. I repeated the operation a ſecond and third time, and had then reaſon to conclude that the wood was nearly as dry as it would become by lying long in a dry air. I now let it cool to the temperature of the atmoſphere which was 75° and heating the ſand to 200 only, I poured it around the rod. In a few minutes it expanded 16 diviſions. In half an hour the ſand had cooled to 125, and the rod had contracted 11 diviſions. In an hour more the ſand was 80 and the rod ſhortened full 4 diviſions more, being nearly equal to its length when the ſand was firſt applied. On the whole I conclude that very dry wood expands with heat pretty regularly, though certainly in a much leſs degree than any of the metals or even glaſs.

DAVID RITTENHOUSE.

To the Philoſophical Society.

P. S.

May 12th 1796.

P. S. The rod above mentioned having been kept in a dry place for twelve months, I again tried it with the pyrometer, having fixed near one end of it a ſmall graduated ſcale of ivory, 360 diviſions whereof were equal to one inch. This ſcale was viewed with the microſcope, furniſhed with a croſs hair, and I thought this method preferable to the ſcrew micrometer uſed before.

The rod was placed in the pyrometer when the temperature of the air was about 60°. On pouring ſand around it, heated a little higher than boiling water perhaps, it immediately expanded $\frac{1}{2}$ a diviſion, but in leſs than a minute it began to contract, and continued to do ſo for an hour, when I drew off the ſand. It was now full 10 diviſions ſhorter than at firſt, ſo that it had imbibed a great deal of moiſture from the air which it again parted with to the heated ſand. Three hours afterwards when the rod was cool, I again poured the ſand on it, heated as before. It now continued to expand for about three minutes, when its length was encreaſed $3\frac{1}{4}$ diviſions, it then began to contract, and became full 3 diviſions ſhorter than when the ſand was poured on it. I cauſed the ſand to run off once more, and let the rod cool. Then heating the ſand 250° by a thermometer, I poured it on the rod, and in a few minutes it expanded $3\frac{3}{4}$ diviſions, it then began to contract ſlowly, and in 15 minutes it became $\frac{2}{3}$ of a divſion ſhorter than at firſt. On the whole I concluded that the expanſion of wood, in its length, will be irregular, correſponding partly to the warmth, and partly to the moiſture of the atmoſphere.

D. RITTENHOUSE.

N°. VI.

A Letter from Mr. ANDREW ELLICOTT, *to* ROBERT PATTERSON; *in Two Parts.*

Part first contains a number of Astronomical Observations.

Part second contains the Theory and Method of calculating the Aberration of the Stars, the Nutation of the Earth's Axis, and the Semiannual Equation.

PART FIRST.

Philadelphia, April 2d, 1795.

DEAR SIR,

Read April 3, 1795.

I HEREWITH present you with a considerable number of Astronomical Observations, which you will observe were generally made on some very important occasions. —The following immersions, and emersions, of the satellites of Jupiter, were observed at Wilmington on the Delaware, by Messrs. Rittenhouse, Page, Andrews, and Lukens; and at the western observatory by Messrs. Ewing, Madison, Hutchins, and myself, for the purpose of determining the western extension of the state of Pennsylvania.

Immersions observed at the Western observatory in 1784.

Day of the month.	Satellite.	Mean Time.	Observers.	Telescopes.*
July 17.	1	$12^{h}\,13'\,48''$	Ewing,	A
		12 13 20	Madison,	B
		12 13 25	Hutchins,	C
		12 13 25	Ellicott.	D

Immersions observed at Wilmington in 1784.

Day of the Month.	Satellite.	Mean Time.	Observers.	Telescopes.*
July 1.	1	$14^{h}\,17'\,33''$	Page,	E
		14 17 46	Rittenhouse,	G
		14 17 48	Lukens.	F
July 3.	2	13 18 58	Page,	E
		13 19 12	Rittenhouse,	G
		13 19 02	Lukens.	F
July 8.	1	16 11 10	Page,	E
		16 11 27	Rittenhouse.	G

* A a 4 feet acromatic, B 2½ feet reflector, C a 2 feet reflector, and D a 3 feet acromatic.

* G a 4 feet reflector, E a 2 feet reflector, F 3½ feet acromatic, and H a 2 feet reflector.

Aug. 3.

Immerſions obſerved at the Weſtern Obſervatory.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Aug. 3.	3	8h 54′ 56″	Ewing,	A
		8 55 16	Madiſon,	B
		8 55 23	Hutchins,	C
		8 55 6	Ellicott.	D
Aug. 9.	1	12 24 20	Ewing,	A
		12 24 25	Madiſon,	B
		12 24 15	Hutchins,	C
		12 24 31	Ellicott,	D
Aug. 10.	3	12 56 24	Ewing,	A
		12 56 29	Madiſon,	B
		12 56 24	Hutchins,	C
		12 56 8	Ellicott.	D
Aug. 16.	1	14 18 40	Ewing,	A
		14 18 13	Hutchins,	C
		14 19 1	Ellicott.	D
Aug. 19.	4	12 31 2	Ewing,	A
		12 30 57	Madiſon,	B
		12 31 15	Ellicott.	D

Immerſions obſerved at Wilmington.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Aug. 3.	3	9h 15′ 17″	Andrews,	H
		9 14 47	Page,	E
		9 15 37	Lukens,	F
		9 15 27	Rittenhouſe	G
Aug. 10.	3	13 16 10	Andrews,	H
		13 16 33	Lukens,	F
		13 16 38	Rittenhouſe.	G
Aug. 16.	1	14 38 51	Andrews,	H
		14 38 31	Page,	E
		14 38 37	Lukens,	F
		14 38 39	Rittenhouſe.	G
Aug. 19.	4	12 49 36	Andrews,	H
		12 49 46	Page,	E
		12 50 26	Lukens,	F
		12 50 21	Rittenhouſe.	G
Aug. 23.	1	16 32 11	Andrews,	H
		16 32 45	Lukens,	F
		16 32 49	Rittenhouſe.	G

Emerſions obſerved at the Weſtern Obſervatory, 1784.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Aug. 27		7h 26′ 0	Ewing,	A
		7 26 3[illegible]	Madiſon,	B
		7 26 15	Ellicott,	D
Aug. 2[illegible]		12 39 41	Ewing,	A
		12 39 58	Madiſon,	B
		12 40 21	Hutchins,	C
		12 40 8	Ellicott.	D

Emerſions obſerved at Wilmington, 1784.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Aug. 29.	2	13h 0′ 18″	Andrews,	H
		13 0 10	Page,	F
		12 59 43	Rittenhouſe.	G
Sept. 8.	3	8 43 23	Andrews,	H
		8 42 55	Page,	F
		8 42 45	Rittenhouſe.	G
Sept. 10.	1	11 36 6	Andrews,	H
		11 36 1	Page,	F
		11 35 48	Rittenhouſe.	G

Emerſion

Emerſions obſerved at the weſtern Obſervatory.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Sept. 15.	3	12h 22′ 55″	Ewing,	A
		12 23 2	Madiſon,	B
		12 23 31	Hutchins,	C
		12 22 49	Ellicott.	D
Sept. 19	1	7 38 56	Ewing,	A
		7 39 9	Madiſon,	B
		7 39 6	Hutchins,	C
		7 39 11	Ellicott.	D

Emerſions obſerved at Wilmington.

Day of the Month.	Satellite.	Mean Time.	Obſervers.	Teleſcopes.
Sept. 15.	3	12h 44′ 15″	Andrews,	H
		12 44 8	Lukens,	F
		12 43 45	Rittenhouſe.	G
Sept. 19.	1	7 59 12	Andrews,	H
		7 58 54	Lukens,	F
		7 59 6	Rittenhouſe.	G

Although the correſponding obſervations only, were admitted in the deciſion, the non-correſponding ones may nevertheleſs be uſeful for fixing the geographical ſituations of other places, where correſponding ones may have been made.

In drawing a concluſion from the foregoing obſervations, it was thought neceſſary to conſider what dependence ought to be placed in each ſatellite; becauſe their different velocities, will give different degrees of certainty. The firſt ſatellite is ſmall, but the rapidity of its motion is much more than a compenſation for this deficiency: Its luſtre is much ſooner loſt, or acquired, than that of the ſecond: On the ſame account, the ſecond is better than the third, and the third than the fourth.—The ſlow motion of the third and fourth ſatellites, will occaſion great uncertainty, if the atmoſphere ſhould be more hazy at one obſervatory, than the other, at the time of obſervation: this is manifeſt from the correſponding obſervations of Auguſt 19th and September 15th, both of which would have been rejected, had they not counteracted each other. The firſt ſatellite, being ſo much ſuperior, on account of certainty, to either of the others, we thought proper to put as much dependence upon it, as upon the others collectively, and that the mean of thoſe reſults, ſhould be deemed the aſtronomical diſtance between the eaſtern and weſtern obſervatories.

The correſponding obſervations on the firſt ſatellite, are thoſe of Auguſt 16th and September 19th.

Diff. of longitude by the correſponding obſervations of Auguſt 16th.	20′ 1″ 10‴	Immerſion 1ſt Satellite.
Ditto by do. Sept. 15th.	19 58 30	Emerſion 1ſt Satellite.
Diff. of longitude by 1ſt Satellite.	19 59 50 =	the Mean Longitude.

Diff.

Diff. of longitude by the correfponding obfervations of Auguft 3d.	20′ 6″ 45‴	Immerfion 3d Satellite.	
Do. by Do. of Auguft 10th	20 7 45	Do.	Do.
Do. by Do. of Auguft 19th	18 57 45	Do. 4th	Satellite.
Do. by Do. of Auguft 29th	20 1 40	Emerfion 2d	Satellite.
Do. by Do. of September 15th	20 58 10	Do. 3d	Satellite.
Longitude by the 2d 3d and 4th Satellites collectively.	20 2 25	= Mean Longitude.	
Do. by the 1ft Satellite	19 59 50		
	20 1 $7\frac{1}{2}$	Mean.	

Hence the diftance between the obfervatories exceeded 5 degrees of longitude, (being the extent of Pennfylvania weft from a point on the Delaware,) by 1″ 7‴.5.

After the determination, we completed the fouthern boundary of Pennfylvania; it being likewife the north boundary of Maryland, and a part of Virginia, and which had been carried on fome years before by Meffrs. Mafon, and Dixon, the diftance of 242 miles.*

On the 9th day of June 1785, the following obfervations were made at the weft end of the above line to trace a meridian north, for the weftern boundary of Pennfylvania, and the eaftern boundary of a part of Virginia.

Diff. in time between the paffage of *a* Libræ, and *β* Urfæ Min. over our line.		0^h 12′ 40″.5
Right Afcenfion of *β* Urfæ Min.	7^s 12° 54′ 6″	
Do. *a* Libræ. -	7 9 46 0	
Diff. - -	3 8 6 in time=	0 12 29 .2
Error of the line in time - -		0 0 11 .3

By the above error of 11″.3 in time, it appears that our line was inclined to the weft 57″ in fpace, which was corrected on a bafe of 300 perches.

June 29th about 17 miles north from our firft ftation, we corrected our line by the following obfervations.

Diff. in time between the paffage of *a* Libræ, and *β* Urfæ Minoris over our line.		0^h 12′ 17″
Right Afcenfion of *β* Urfæ Min.	7^s 12° 53′ 50″	
Do. of *a* Libræ.	7 9 46 11	
Diff. - -	0 3 7 39 = in time to	0 12 30
Error of the line in time. - - -		0 0 13 diff.

The

* This line is in the parallel of 39° 43′ 18″ north latitude. My affociates in this part of the bufinefs were, Dr. Rittenhoufe, Dr. Ewing, Mr. Andrews, and Mr. Hutchins.

The above difference of 13″ in time, is equal to an angle of 1′ 5″ in ſpace, which in this caſe is the error of our line towards the eaſt, and was corrected on a baſe of 110 perches.

On the 16th of July, diſtant from our firſt poſition 29 miles, we examined the direction of our line by the following obſervations.

Diff. in time between the paſſage of μ Sagit. and γ Draco. over our line. - - - -			0h 9′ 20.″5
Right Aſcenſion of μ Sagit.	9s 0° 14′ 47″		
Do. γ Draco. -	8 27 54 59		
Diff. - -	2 19 48	= in time to	0 9 19
Error in time - - - -			0 0 1.5 diff.

From the above obſervation it appears that our direction is ſufficiently accurate, and the ſmall error if it can be called one, is to the eaſt.

September 3d we made the following obſervations to rectify the direction of our line.

Diff. in time between the paſſage of α Urſæ Majoris, and γ Cephi. over our line. - - -			0h 40′ 26″
Right Aſcenſion of γ Cephi.	11s 22° 40′ 53″		
Do. of α Urſæ Maj.	5 12 35 18		
Diff. - -	6 10 5 35		
Deduct 6s. -	6		
-	0 10 5 35	= in time to	0 40 22
Error in time - - - -			0 0 4 diff.

By this obſervation it appears that our line is directed too much towards the eaſt by an angle of 13″.

Diff. in time between the paſſage of α Urſæ Minoris, and ε Urſæ Majoris over our line - - -			0h 5′ 8″
Right Aſcenſion of α Urſæ Min.	0s 12° 34′ 13″		
Do. of α Urſæ Maj.	6 11 7 24		
	6 1 26 49		
Deduct - -	6		
Difference -	0 1 26 49	= in time to	0 5 47
Error in time - - - -			0 0 39 diff.

By this obſervation, it appears that our direction is too much eaſt by an angle of 23″.

Error of the line by α Urſæ Majoris, and γ Cephi.		0′ 13″
Do. by *a* Urſæ Min. and ε Urſæ Majoris.		0 23
		2)0 36
Mean error towards the eaſt	-	0 18

This correction of 18″ was made on a baſe of 24 perches.

The ſame night, we alſo took the greateſt deviation of the pole ſtar, (*a* Urſæ Min.) and the error diſcovered in the line by that method did not differ more than 1″ from a mean of the other obſervations.—It is alſo worthy of remark, that we had not corrected for ſomewhat more than 54 miles: from which a concluſion may be drawn very favourable to the method uſed in carrying on the line, otherwiſe the error muſt have been more conſiderable in ſuch a diſtance.

On the ſixth day of October, diſtant from our firſt ſtation 90 miles, the direction of our line was examined by the following obſervations.

Diff. in time between the paſſage of α Urſæ Min. and, ε Urſæ Maj. over our line. - - .			$0^h\ 4'\ 56''$
Right Aſcenſion of α Urſæ Min.	$0^s\ 12^\circ\ 35'\ 51''$		
Do. of ε Urſæ Maj.	6 11 7 39		
	6 1 28 12		
Deduct - -	6		
Diff. - -	0 1 28 12	= in time to	0 5 53
Error in time - - - -			0 0 57 diff.

The above error in time by *a* Urſæ Min and ε Urſæ Maj. is equal to an angle of 34″, which was to the weſt. This error was corrected on a baſe of 48 perches.

On the 17th of October, diſtant from our firſt poſition about 100 miles, we examined the direction of our line by the following obſervations.

Diff. in time between the paſſage of γ Capricorn, and β Cephi over our line - - - -			$0^h\ 2'\ 16''$
Right Aſcenſion of γ Capri.	$10^s\ 22^\circ\ 3'\ 9''$		
Do. of β Cephi.	10 21 27 17		
Diff. - -	0 0 35 52	= in time to	0 2 23
Error of the line in time. - . - -			0 0 7 diff.

This error in time, (by thoſe ſtars,) is equal to an angle of 46″ which is to the weſt.

Diff. in time between the paſſage of β Urſæ Maj. and Fomalhout over our line		$0^h\ 2'\ 56''$
Right Aſcenſion of β Urſæ Maj.	$5^s\ 12°\ 11'\ 4''$	
Do. of Fomalhout	11 11 26 34	
	6 0 44 30	
Deduct	6	
Diff.	0 0 44 30 = in time to	0 2 58
Error of the line in time		0 0 2 diff.

This error of 2″ in time, is equal to an angle of 10″ the error of the line towards the weſt.

Diff. in time between the paſſage of α Urſæ Min. and ε Urſæ Maj. over our line		$0^h\ 6'\ 34''$
Right Aſcenſion of α Urſæ Min.	$0^s\ 12°\ 35'\ 50''$	
Do. of ε Urſæ Maj.	6 11 7 41	
	6 1 28 9	
Deduct	6	
Diff.	0 1 28 9 = in time to	0 5 53
Error of the line in time		0 0 41 diff.

By this laſt obſervation, our direction appears to be inclined to the weſt, by an angle of 25″.

Error of the line by γ Capri. and β Cephi.		0′ 46″
Do.	by β Urſæ Maj. and Famalhout,	0 10
Do.	by α Urſæ Min. and ε Urſæ Maj.	0 25
		3)1 11
Mean error towards the weſt		0 23⅓

This correction of 23″⅓ was made on a baſe of 40 perches, which cloſed our operations that ſeaſon.*

The year following, (viz. in 1786,) the line was carried on about 55⅓ miles to Lake Erie by Andrew Porter, and Alexander Maclain: in that diſtance the direction was not corrected by any obſervations, neither could it appear very neceſſary, when we conſider how trifling, and unimportant all the errors were which had been diſcovered the preceding ſeaſon.—The line was run by

* Dr. Rittenhouſe, Joſeph Nevil, Andrew Porter, and myſelf were concerned in this line. Joſeph Nevil left us about the 21ſt of Auguſt, and Dr. Rittenhouſe about the 17th of September.

by a moſt excellent tranſit inſtrument, made by Mr. Bird, and which had been uſed by Meſſrs Maſon and Dixon, ſome years before in this country.

The magnetic variation was taken in many places on this line, and was found at our firſt ſtation at the end of the parallel of

latitude to be		1° 5′	East.
5 miles on the line it was		2 3	
11	Do.	2 10	
14	Do.	1 57	
16½	Do.	1 30	
19	Do.	1 25	
20	Do.	1 12.5	
26	Do.	1 17.5	
29	Do.	1 37	
37	Do.	1 7.5	
44	Do.	0 57	
47	Do.	0 40	
51	Do.	0 57.5	
53	Do.	0 50	
57	Do.	1 2.5	
63¾	Do.	0 57.5	
70	Do.	0 51	
75	Do.	0 27.5	
79	Do.	0 17.5	
90	Do.	0 19.5	
100	Do.	0 25	

The ſtate of Pennſylvania is bounded on the north by the 42° of north latitude. This line extends from a point on the Delaware, (which was fixed by Dr. Rittenhouſe and Captain Holland in the year 1774,) and extends weſt to Lake Erie: it was completed in the years 1786, and 1787. In order to carry on the parallel of latitude with as much expedition, and economy as poſſible, we diſpenſed with the method of tracing a line on the arc of a great circle, and correcting into the parallel, as purſued by Meſſrs Maſon and Dixon, in determining the boundary between this ſtate, and the ſtate of Maryland, and which we followed in completing their line in the year 1784. We commenced our operations by running a guide line weſt, with a ſurveying compaſs from the point mentioned on the

Delaware 20¼ miles, and there corrected by the following zenith distances taken at its western termination by a most excellent sector, constructed, and executed, by Dr. Rittenhouse.*

Face of the Sector East, 1786.

July	21st	Observed Z. distance	α Lyræ	3°	23′	46″.5	S
	22	Do.	do.	3	23	46	S
	23	Do.	α Cygni	2	31	52	N
	24	Do.	do.	2	32	1	N
	25	Do.	do.	2	32	1	N
		Do.	Capella	3	46	55	N
August	5	Do.	α Lyræ	3	23	37	S
		Do.	α Cygni	2	32	5	N

Face of the Sector West, 1786.

July	25th	Observed Z. distance	α Lyræ	3°	24′	31″	S
	26	Do.	Capella	3	45	17	N
	29	Do.	do.	3	45	15.5	N
		Do.	α Cygni	2	31	14.5	N
	31	Do.	do.	2	31	16	N
August	1	Do.	Capella	3	45	16	N
		Do.	α Cygni	2	31	18.5	N
	4	Do.	Capella	3	45	17.5	N
		Do.	α Cygni	2	31	19.5	N

Mean latitude deduced from the above observations	41° 59′ 52″.7
By which it appears that we were too far south by	7.3

The correction being made, the guide line was corrected back to the Delaware, and another guide line carried on west 19¼ miles from the corrected point north of our observatory, at the termination of which the following zenith distances were observed.

Face of the Sector East, 1786.

August	17th	Observed Z. distance	α Lyræ	3°	23′	39″.5	S
	18	Do.	do.	3	23	37.5	S
		Do.	α Cygni	2	32	10.5	N
	19	Do.	α Lyræ	3	23	36.5	S
		Do.	α Cygni	2	32	8	N
	20	Do.	α Capella	3	46	1.5	N

* At this station a number of observations were rejected, on account of their disagreement, which we fortunately discovered was owing to the atmosphere being affected by the numerous fires we kept up to keep off the flies, musketoes, and gnats, which are very troublesome in that part of the country.

† *Note* the letters N. S. signify north and south of the Zenith.

Mean

Face of the Sector West, 1786.

August 20th	Observed Z. distance	α Lyræ	3°	24′	22″	S
	Do.	α Cygni	2	31	23	N
21	Do.	Capella	3	45	16	N
	Do.	α Lyræ	3	24	23.5	S
	Do.	α Cygni	2	31	24	N

Mean latitude deduced from the foregoing observations 41° 59′ 53″
Hence our observatory too far south by 7

This correction being made, we proceeded as in the first case, and carried on our guide line 21¼ miles, at the termination of which we observed the following zenith distances.

Face of the Sector East, 1786.

September 1st	Observed Z. distance	α Cygni	2°	32′	0″	N
2	Do.	do.	2	32	0	N
3	Do.	α Lyræ	3	23	47	S
6	Do.	do.	3	23	44.5	S
	Do.	α Cygni	2	32	1	N
7	Do.	α Lyræ	3	23	45.5	S
	Do.	α Cygni	2	31	59	N

Face of the Sector West, 1786.

September 8th	Observed Z. distance	α Lyræ	3°	24′	31″	S
9	Do.	α Cygni	2	31	13	N
	Do.	α Lyræ	3	24	33	S
	Do.	α Cygni	2	31	15	N
10	Do.	do.	2	31	12	N

Mean latitude deduced from the above observations 42° 0′ 3.8″
Too far north by 3.8

The above correction of 3″.8 being laid off, we proceeded as formerly, and carried on our guide line 28¼ miles, and observed the following Z. distances at its termination.

Face of the Sector East, 1786.

September 22d	Observed Z. distance	α Lyræ	3°	23′	36″	S
	Do.	α Cygni	2	32	16	N
23	Do.	α Lyræ	3	23	34.5	S
	Do.	α Cygni	2	32	12	N
24	Do.	α Lyræ	3	23	35	S
	Do.	α Cygni	2	32	16.5	N

Face

Face of the Sector Weſt, 1786.

September 27th	Obſerved Z. diſtance	α Lyræ	3°	24′	23″.5	S
	Do.	α Cygni	2	31	23.5	N
28	Do.	do.	2	31	24	N
29	Do.	α Lyræ	3	24	22.5	S
	Do.	α Cygni	2	31	26.5	N
30	Do.	α Lyræ	3	24	24.5	S
	Do.	α Cygni	2	31	26.5	N

Mean latitude by the above obſervations	41° 59′ 55″.2
Too far ſouth by - -	4 .8

The correction being made and our guide line corrected back, we ceaſed our operations for that ſeaſon.

In June the year following we carried on our guide line 19¼ miles and at its termination made the following obſervations.

Face of the Sector Weſt, 1787.

June 19	Obſerved Z. diſtance	Capella	3°	45′	2″.5	N
20	Do.	α Lyræ	3	24	54	S
	Do.	γ Androm.	0	42	35	S
	Do.	Capella	3	45	2	N
	Do.	α Cygni	2	30	55.5	N
21	Do.	α Lyræ	3	24	53.5	S
	Do.	δ Cygni	2	36	30.5	N
	Do.	α Cygni	2	30	56	N
22	Do.	γ Androm.	0	42	35.5	S
	Do.	Capella	3	45	1.5	N
23	Do.	α Lyræ	3	24	50.5	S
	Do.	δ Cygni	2	36	31.5	N
	Do.	α Cygni	2	30	57.5	N
	Do.	Capella	3	45	0.5	N
24	Do.	α Lyræ	3	24	53	S
	Do.	δ Cygni	2	36	30	N
25	Do.	Capella	3	44	59.5	N

Face of the Sector Eaſt, 1787.

June 26th	Obſerved Z. diſtance	α Lyræ	3°	24′	9″.3	S
	Do.	α Cygni	2	31	37.3	N
	Do.	γ Androm.	0	41	52.5	S
	Do.	Capella	3	45	42	N
28	Do.	α Lyræ*	3	24	7	S
	Do.	δ Cygni	2	37	13	N
	Do.	α Cygni	2	31	39	N
	Do.	Capella	3	45	44	N
	Do.	α Lyræ	3	24	6.5	S

* Note the Zenith diſtances are entered according to the civil account, and therefore α Lyræ by ſiderial time gaining 3′ 56″ on mean ſolar time, was twice on the meridian that day.

June

June 29th	Obferved Z. diftance	δ Cygni	2°	37′	16″.5	N
	Do.	α Cygni	2	31	44.2	N
	Do.	γ Androm.	0	41	53.2	S
	Do.	Capella	3	45	44	N

Mean latitude by the foregoing obfervations	42°	0′	12.4″
Too far north by			12.4

The above correction being made we carried on our guide line $26\frac{5}{8}$ miles, and at its termination obferved the following Zenith diftances.

Face of the Sector Weft, 1787.

July 7th	Obferved Z. diftance	γ Androm.	0°	42′	40″.5	S
	Do.	Capella	3	44	54	N
	Do.	α Lyræ	3	24	47	S
8	Do.	Capella	3	44	52	N
9	Do.	α Lyræ	3	24	48	S
10	Do.	α Cygni	2	31	22	N
	Do.	Capella	3	44	54	N
11	Do.	γ Androm.	0	42	41	S
	Do.	Capella	3	44	53.7	N
	Do.	α Lyræ	3	24	47.5	S
12	Do.	δ Cygni	2	36	33	N
	Do.	α Cygni	2	30	58	N
13	Do.	δ Cygni	2	36	32	N
	Do.	α Cygni	2	31	1	N
	Do.	Capella	3	44	56	N

Face of the Sector Eaft, 1787.

July 13th	Obferved Z. diftance	α Lyræ	3°	24′	2″	S
14	Do.	γ Androm.	0	41	53	S
	Do.	Capella	3	45	37.9	N
	Do.	α Lyræ	3	24	1	S
15	Do.	α Cygni	2	31	45	N
	Do.	Capella	3	45	40.3	N
16	Do.	α Lyræ	3	24	2	S
	Do.	δ Cygni	2	37	20.5	N
17	Do.	α Cygni	2	31	45.2	N
	Do.	γ Androm.	0	41	54	S
	Do.	Capella	3	45	41	N
	Do.	δ Cygni	3	37	17.4	N
18	Do.	γ Androm.	0	41	54.2	S
	Do.	Capella	3	45	39	N
	Do.	δ Cygni	2	37	20	N
19	Do.	α Cygni	2	31	41.7	N
	Do.	Capella	3	45	40	N

Mean latitude of our Obſervatory	-	42° 0′ 15″
Too far north by	- - -	15

The above correction being made, we carried on the guide line 30½ miles, and at its termination obſerved the following Zenith diſtances.

Face of the Sector Weſt, 1787.

Auguſt 7th	Obſerved Z. diſtance	α Lyræ	3°	23′	53″.4	S
	Do.	α Cygni	2	31	55.5	N
8	Do.	Capella	3	45	38.7	N
	Do.	δ Cygni	2	37	30.1	N
	Do.	α Cygni	2	31	57	N
9	Do.	γ Androm.	0	41	49.5	S
10	Do.	α Lyræ	3	23	53.2	S
11	Do.	α Lyræ	3	23	53.8	S
	Do.	δ Cygni	2	37	33.6	N
	Do.	α Cygni	2	31	54.6	N
12	Do.	Capella	3	45	38.6	N
	Do.	α Lyræ	3	23	52.4	S
	Do.	α Cygni	2	31	57.2	N
13	Do.	γ Androm.	0	41	47.5	S
	Do.	Capella	3	45	36.5	N
	Do.	α Lyræ	3	23	51.8	S
	Do.	δ Cygni	2	37	31.3	N
	Do.	α Cygni	2	31	58.4	N
14	Do.	Capella	3	45	41.5	N

Face of the Sector Eaſt, 1787.

Auguſt 14th	Obſerved Z. diſtance	α Lyræ	3°	23′	8″.5	S
	Do.	δ Cygni	2	38	20.7	N
	Do.	α Cygni	2	32	45.6	N
15	Do.	γ Androm.	0	41	0.7	S
	Do.	α Lyræ	3	23	11	S
	Do.	δ Cygni	2	38	22.6	N
16	Do.	γ Androm.	0	41	2	S
	Do.	α Lyræ	3	23	10.5	S
	Do.	δ Cygni	2	38	23.7	N
	Do.	α Cygni	2	32	42.5	N

Mean latitude of the obſervatory	- -	41° 59′ 27″.5
Too far ſouth by	- -	32.5

Corrected as formerly, and carried on the guide line 28¼ miles, and obſerved the following Zenith diſtances.

Face

Face of the Sector West, 1787.

August 25th	Observed Z. distance	δ Cygni	2°	36′	38″.3	N
26	Do.	β Medusæ	1	53	12.5	S
	Do.	Capella	3	44	47.9	N
27	Do.	α Lyræ	3	24	45.7	S
	Do.	δ Cygni	2	36	39	N
	Do.	α Cygni	2	31	8.4	N
30	Do.	Capella	3	44	49.2	N
	Do.	α Lyræ	3	24	42	S
	Do.	α Cygni	2	31	6.5	N
31	Do.	γ Androm.	0	42	32.9	S
	Do.	β Meadusæ	1	53	15.9	S
	Do.	α Lyræ	3	24	44.9	S
	Do.	δ Cygni	2	36	41.5	N
	Do.	α Cygni	2	31	10.2	N
Sept. 2d	Do.	β Medusæ	1	53	11.3	S

Face of the Sector East, 1787.

September 2d	Observed Z. distance	α Lyræ	3°	23′	58″.2	S
	Do.	δ Cygni	2	37	24.5	N
	Do.	α Cygni	2	31	55.4	N
3	Do.	γ Androm.	0	41	50.6	S
	Do.	α Lyræ	3	23	59.2	S
	Do.	δ Cygni	2	37	27.5	N
	Do.	α Cygni	2	31	56.6	N
4	Do.	γ Androm.	0	41	47.4	S
	Do.	β Medusæ	1	52	26.8	S
	Do.	Capella	3	45	31.5	N
	Do.	α Lyræ	3	23	57.8	S
	Do.	δ Cygni	2	37	28.8	N
6	Do.	α Lyræ	3	23	58.4	S
	Do.	δ Cygni	2	37	29	N
7	Do.	β Medusæ	1	52	28.5	S
	Do.	α Cygni	2	31	56.4	N
8	Do.	β Medusæ	1	52	27.8	S
	Do.	Capella	3	45	31.1	N
9	Do.	do.	3	45	35	N

Mean latitude of the Observatory - - 42° 0′ 21″.3
Too far north by - - - 21.3

The above correction being made, we carried on the guide line 32½ miles, and observed the following Zenith distances.

Face of the Sector West, 1787.

September 21st	Observed Z. distance	α Lyræ	3°	24′	31″.5	S
	Do.	δ Cygni	2	36	54	N
	Do.	α Cygni	2	31	28.3	N

Sep. 22d

September 22d	Obſerved Z. diſtance	β Meduſæ	1°	52′	57″.9	S
	Do.	Capella	3	45	4.3	N
	Do.	α Lyræ	3	24	31.5	S
	Do.	α Cygni	2	31	28.3	N
23	Do.	γ Androm.	0	42	21.7	S
	Do.	α Lyræ	3	24	32.9	S
	Do.	δ Cygni	2	36	58.5	N
	Do.	α Cygni	2	31	25	N
24	Do.	Capella	3	45	1.2	N
	Do.	δ Cygni	2	36	55.5	N
	Do.	α Cygni	2	31	28.6	N
25	Do.	Capella	3	45	2.5	N
October 2	Do.	γ Androm.	0	42	19.5	S
	Do.	β Meduſæ	1	53	0.3	S

Face of the Sector Eaſt, 1787.

September 25th	Obſerved Z. diſtance	α Lyræ	3°	23′	46″.4	S
	Do.	δ Cygni	2	37	45.8	N
	Do.	α Cygni	2	32	14.5	N
26	Do.	α Lyræ	3	23	43.5	S
	Do.	δ Cygni	2	37	42.5	N
	Do.	α Cygni	2	32	14.9	N
27	Do.	α Lyræ	3	23	42.9	S
	Do.	δ Cygni	2	37	41.3	N
28	Do.	δ Cygni	2	37	41.3	N
	Do.	α Cygni	2	32	11.2	N
29	Do.	γ Androm.	0	41	39	S
30	Do.	γ Androm.	0	41	38.7	S
	Do.	β Meduſæ	1	52	12	S
	Do.	Capella	3	45	47.2	N
	Do.	α Lyræ	3	23	46.2	S
	Do.	δ Cygni	2	37	42	N
	Do.	α Cygni	2	32	13	N
October 1	Do.	γ Androm	0	41	35	S
	Do.	β Meduſæ	1	52	11.5	S
	Do.	Capella	3	45	45.6	N

Mean latitude of the Obſervatory - - - 42° 0′ 10″.8
Too far north by - - - - 10 .8

Corrected as formerly, and carried on our guide line 32½ miles, to Lake Erie, and obſerved the following Zenith diſtances.

Face of the Sector Weſt, 1787.

October 8th	Obſerved Z. diſtance	δ Cygni	2°	37′	12″.9	N
	Do.	α Cygni	2	31	40.5	N

Oct. 9th

October 9th	Observed Z. distance	γ Androm.	0°	42′	1″.9	S
	Do.	β Medusæ	1	52	43.8	S
	Do.	Capella	3	45	18.1	N
10	Do.	β Medusæ	1	52	44.5	S
	Do.	Capella	3	45	13.5	N
	Do.	α Lyræ	3	24	17.4	S
	Do.	δ Cygni	2	37	11.4	N
	Do.	α Cygni	2	31	41.3	N
11	Do.	γ Androm.	0	42	2.4	S
	Do.	β Medusæ	1	52	47	S
	Do.	α Lyræ	3	24	15.3	S
13	Do.	α Cygni	2	31	37	N
14	Do.	β Medusæ	1	52	42.8	S
	Do.	Capella	3	45	13.8	N
	Do.	α Lyræ	3	24	21.5	S
	Do.	δ Cygni	2	37	10.2	N
	Do.	α Cygni	2	31	41.5	N
15	Do.	γ Androm.	0	42	1.6	S
	Do.	β Medusæ	1	52	47.1	S
	Do.	Capella	3	45	17.6	N

Face of the Sector East, 1787.

October 15	Observed Z. distance	α Lyræ	2°	23′	34″.7	S
	Do.	δ Cygni	2	37	54.5	N
	Do.	α Cygni	2	32	25.4	N
16	Do.	γ Androm.	0	41	14.2	S
	Do.	β Medusæ	1	52	0.4	S
	Do.	Capella	3	45	58.5	N
17	Do.	β Madusæ	1	51	59.9	S
	Do.	α Lyræ	3	23	34.9	S
	Do.	δ Cygni	2	37	57	N
	Do.	α Cygni	2	32	27.6	N
18	Do.	Capella	3	45	58.2	N
	Do.	α Lyræ	3	23	31.2	S
	Do.	δ Cygni	2	37	55.2	N
	Do.	α Cygni	2	32	24.7	N
19	Do.	γ Androm.	0	41	13.2	S
	Do.	β Medusæ	1	51	58.4	S
	Do.	δ Cygni	2	37	51.1	N
	Do.	α Cygni	2	32	25.9	N
20	Do.	γ Androm.	0	41	13.3	S
	Do.	β Medusæ	1	51	57.4	S

Mean latitude of the Observatory by the above observations 41° 59′ 58″.7
Too far south by - - - 1 .3

The above correction being made, completed the Astronomical boundaries of the State of Pennsylvania.

My aſſociates in tracing the north boundary of Pennſylvania were Dr. Rittenhouſe, James Clinton, and Simeon De Wit, in the year 1786. The firſt of thoſe gentlemen left us in the beginning of September.—The year following my aſſociates were Andrew Porter, Abraham Hardenberg, and William Morris.

I have omitted the calculations, and given only the reſults, for the following reaſons, *firſt* they would have ſwelled this paper to a great length, *ſecondly* no difficulty can ariſe in making them, to any perſon moderately acquainted with practical aſtronomy, except in thoſe ſmall equations depending upon the effects of aberration and nutation, which from the preſent improved ſtate of this ſcience, have become abſolutely neceſſary; and *thirdly* becauſe I intend concluding this paper, with a ſhort eſſay deſigned to render eaſy ſo much of the calculations, as depend upon the effects of aberration and nutation.

The following emerſions of the 1ſt Satellite of Jupiter were abſerved in Baltimore, in the State of Maryland. The teleſcope which I uſed was acromatic, and magnified about 60 times.

January 1788,	2^d	8^h	$6'$	$23''$	hazy atmoſphere	Mean Time.
	9	10	0	14	very clear	
	18	6	23	57	do.	
	25	8	18	54	do.	

Obſervations made at Georgetown, in the diſtrict of Columbia on the annular eclipſe of the Sun in the year 1791.

The beginning of the eclipſe could not be obſerved, the ſun being below the horizon.

April 2d	18^h $39'$	$1^s.25$	annulas completed	Mean Time.
	18 43	15 .25	annulas broken	
	19 55	37 .75	end of the eclipſe	

From an uncommon undulation in the atmoſphere till towards the end of the eclipſe, I cannot pretend to be certain within two or three ſeconds of the completion, and breaking of the annulas; but the end may be relied on as correct. The lat. of Georgetown is about 38° 55′ N.

In the city of Waſhington lat. 38° 52′ 40″ N. I obſerved the following occultation of Aldebaran by the Moon.

Immerſion	January 1793	21^d	7^h	$55'$	$49''.5$	Apparent Time.
Emerſion		21	9	25	21 .5	

A number

A number of the eclipſes of the firſt Satellite of Jupiter, together with a great proportion of my notes relative to the city of Waſhington, were privately taken from my lodgings in Georgetown, otherwiſe they ſhould have appeared in this paper.

As the city of Waſhington from its ſhortly becoming the permanent ſeat of the government of the United States, muſt be an object of importance, I preſume it will not be unacceptable to give ſome account of the method uſed in laying out the ten miles ſquare, and executing the plan of the city.—Preparative to beginning the ten miles ſquare, a meridian was traced at Jones's Point on the weſt ſide of the Potomak: from this meridian an angle of 45° was laid off north-weſterly, and a ſtraight line continued in that direction ten miles; from the termination of this line making a right angle with it, a ſtraight line was carried north-eaſterly ten miles: from the termination of this ſecond line, a third making a right angle with it was carried ſouth-eaſterly ten miles; and from the beginning on Jones's Point, a fourth was carried ten miles to the termination of the third. Theſe lines were meaſured with a chain which was examined and corrected daily, and plumbed wherever the ground was uneven, and traced with a tranſit and equal altitude inſtrument which I conſtructed and executed in 1789, and uſed in running the weſtern boundary of the State of New York. This inſtrument was ſimilar to that deſcribed by M. Le Monnier in his preface to the French Hiſtoire Celeſte; except in being accommodated to a firm portable triangular frame. The tranſit and equal altitude inſtrument is of all others the moſt perfect, and beſt calculated for running ſtraight lines, and when the different verifications are carefully attended to, may ſafely be conſidered as abſolutely perfect. The lines of the ten miles ſquare were opened forty feet wide, and a mile-ſtone ſet up at the termination of each mile where the ground would admit of it, and marked with the magnetic variation at that particular ſpot.

In order to execute the plan of the city, a meridional line was drawn through the area intended to be occupied by the capitol, and croſſed at right angles by another line paſſing through the ſame area: theſe lines were continued to the extremities of the city, and became the baſis on which the moſt

conſiderable

confiderable part of the plan was executed.——I firft endeavoured to lay off the parallels with a chain; but from its great uncertainty, owing to its expanfion and contraction with heat and cold, and the bending and ftraightening of the links, was under the neceffity after making many trials of laying it wholly afide, and in its place made ufe of wooden meafuring rods, formed like a carpenter's fquare: thefe rods were truly graduated, and accommodated with plummets and fliders, by the due management of which, the meafurements were always horizontal.——After adopting the ufe of the rods I had but one difficulty for fome time to contend with, which was the tallies being fometimes returned erroneous for want of the neceffary care in the meafurers. The next difficulty was of a much more ferious nature; it was the points of interfection of fome of the leading avenues which fixed the pofition of other ftreets being moved. Upon making this difcovery I at firft fufpected that it had been done by fome perfon, or perfons through inadvertence; but from fubfequent events am inclined to think it was the effect of defign. I have mentioned this circumftance to fhew the neceffity of a conftant attention in thofe intrufted with the execution of fuch complicated plans to the pofition, and fituation of all the leading points.

After the principal avenues were fixed, great part of the work could be examined and corrected with mathematical exactnefs, and the fmalleft error in any of the meafurements detected with certainty.

The following are the the inclinations of feveral of the leading avenues to the meridian.

Maffachufetts avenue eaft of 1ft ftreet weft, and North Carolina and Georgia avenues, make an angle with the meridian of - - -	62° 29′ 32″
Virginia avenue eaftward from the place where the Equeftrian Statue of General Wafhington is to be placed, makes an angle with the meridian of - -	70 18 5
Pennfylvania and Maryland avenues, eaft of the capitol, make an angle with the meridian of - -	62 27 00
Kentucky and an avenue not yet named, make an angle with the meridian of : - -	33 00 00
Water ftreet between 7th and 12th ftreets weft, makes an angle with the meridian of . - -	44 49 50
New Jerfey and Delaware avenues, make an angle with the meridian of - - - -	15 43 24

Pennfylvania

Pennſylvania avenue between the capitol and Preſident's Houſe, and Maryland avenue weſt of the capitol make an angle with the meridian of - -	70° 30' 23"

All the lines of the city in which I have been concerned were traced with the ſame inſtrument which I uſed on the lines of the ten miles ſquare, but as the northern part was not finiſhed when I left that place, I cannot pretend to ſay what method has been ſince purſued.

This paper being already carried to a greater length than I at firſt intended, (but upon looking over my notes I find it is yet ſhort of what was originally deſigned for the ſociety,) I muſt therefore in conſequence of numerous avocations, reſerve the remainder for a future communication, and proceed to the ſubjects of aberration and nutation.

N°. VII.

Of the Aberration of the Stars, Nutation of the Earth's Axis, and Semiannual Equation, by ANDREW ELLICOTT.

PART SECOND.

Of the Aberration of the Stars.

Read April 3, 1795.

THE aberration of the ſtars is their ſmall apparent motion occaſioned by the velocity of the Earth in its orbit bearing a ſenſible proportion to the velocity of light. To give an idea of this effect, ſuppoſe an infinite number of particles of matter moving in the direction of A towards B (Fig. 1 Plate I.) at the ſame time ſuppoſe the tube *a* to be moving towards C and preſerving its paralleliſm; then if the velocity of the tube *a* towards C bears no ſenſible proportion to the velocity of the particles moving from A towards B, a particle which enters the centre of the tube at top will fall upon the centre at the bottom. But if the velocity of the tube towards C bears a ſenſible proportion to the velocity of the particles moving from A towards B, then the particles which fall into

into the centre of the tube at top will not fall upon the centre at the bottom, unlefs the tube ſhould be inclined towards the moving particles like the tube *b*, which inclination muſt be more or lefs as the velocity of the tube in croſſing the direction of the particles, is more or lefs fenſible when applied to their velocity. Now ſuppoſe theſe particles to be rays of light, iſſuing from a ſtar, the line DC a portion of the Earth's orbit, and the tube *a* a teleſcope, then from the theory it is manifeſt, that if the velocity of the Earth in its orbit, bears a ſenſible proportion to the velocity of light, the teleſcope muſt have a direction which will vary from the true place of the ſtar, in order to bring the light through the viſual axis of the inſtrument.

From the ratio of the velocity of the Earth in its orbit, to the velocity of light, a ſtar may poſſibly appear 20″ from its true place, which has alſo been confirmed by celeſtial obſervation, and is the full aberration; but this quantity in declination, and right aſcenſion, will only be had in ſtars particularly ſituated, as in the poles of the ecliptic for declination, and in the ſolſtitial colures for right aſcenſion. A ſtar ſituated in either pole of the ecliptic, will apparently deſcribe a circle round its true place, whoſe radius is 20″; and in the ecliptic apparently vibrate backward and forward in its plane, in a ſtraight line whoſe length is 40″. In whatever figure the ecliptic would be projected when viewed from a ſtar, that ſtar will apparently deſcribe a ſimilar one, which muſt be either a ſtraight line a circle, or an ellipſe.—A ſtraight line if the ſtar is in the ecliptic, a circle if in either pole of the ecliptic, and if in either of the intermediate ſpaces an ellipſe, whoſe ſemitranſverſe will be 20″, and ſemi-conjugate the ſine of the ſtar's latitude, making radius, or the ſine of 90° equal to 20.——ſo far for the theory.

It will be adviſible for thoſe not conſtantly in the habit of making the calculations, to begin by projecting the caſe, which may be done as follows. For an example take β Meduſæ, whoſe longitude is $1^s\ 23°\ 13'$, and latitude 22° 28′ north.—From any ſcale of equal parts take 20, and with that extent for a radius deſcribe the circle ABCD, (Fig. 2 Plate I.) through which at right angles to each other, draw the diameters AC, and BD: let BD be the tranſverſe diameter of the ellipſe. Then for the conjugate ſay

4 As

As rad. or ſine of 90° - -	Log.	10.00000
Is to 20 the equal parts contained in rad.	Log.	1.30103
So is the ſine of the lat. 22° 28′ -	Log.	9.58223
To 7.6 the equal parts cont. in the ſemi-conjugate	Log.	0.88326

From the ſame ſcale of equal parts take 7.6, and from the centre of the circle at E, apply this diſtance each way on the diameter AC: ſuppoſe thoſe points to be at F, and G, then will FG, be the conjugate diameter of the ellipſe BFDG apparently diſcribed by the ſtar. The ellipſe muſt be divided ſimilar to the ecliptic into ſigns, &c. to ſhew the Sun's place. This diviſion muſt begin from the longitude of the ſtar, for which the projection is made, which in the preſent caſe is 1^s 23° 13′ at the point F.—From the point A in the primitive circle lay off 23° 13′, (the exceſs of the ſtar's longitude above 1^s,) towards B, to the point *z*: then from the point *z*, draw the occult line *z*1 to the periphery of the ellipſe parallel to AC, and the place of the firſt ſign will be had—next from the point *z*, in the primitive lay off 30° or one ſign each way, and from thoſe points, as in the firſt caſe, draw parallels to AC, meeting the periphery of the ellipſe, and the poſition of 0^s, and 2^s will be had: In this manner the whole periphery of the ellipſe may be graduated into ſigns, and degrees if the projection ſhould be ſufficiently large.

The next requiſite is to draw the meridian of the ſtar through the centre of the projection. In order to do this, the angle made by the interſection of the circle of the ſtar's longitude, with the circle of its right aſcenſion, muſt be determined; which in the preſent caſe is about 18° 11′: this quantity muſt be laid off in the primitive from A to M, towards B*: then from M through the centre of the projection draw MEP cutting the ellipſe in the point *u*, and it will be the meridian required.

From a little conſideration it will be eaſy to conceive that the effect of aberration will always be found three ſigns behind the Sun's place—hence the aberration anſwering to 2 of the Sun's place, muſt be eſtimated at 11^s——and the occult line E 11,

* It may be obſerved for a general rule that when the right aſcenſion of the ſtar is leſs than 3^s and more than 9^s the meridian muſt be laid off from A towards B; when more than 3^s and leſs than 9^s from A towards D.

will be the apparent diſtance of the ſtar from its true place. From 11 draw 11 p perpendicular to the meridian of the ſtar, and that diſtance will be the aberration in right aſcenſion, which is always at right angles to the meridian, and the diſtance Ep, on the meridian will be the effect in declination.—The firſt meaſured on the ſcale by which the projection was made, will give 18″.62, and the latter about 7″.12: But the firſt muſt be reduced to the equator, which may be done various ways, but the moſt expeditious is by multiplying it into the natural ſecant of the ſtar's declination, which will give 24.34, the effect of aberration in right aſcenſion anſwering to 2^s and 8^s of the Sun's place; but with contrary ſigns of application*. If the projection ſhould be large, this method will anſwer for common purpoſes, but when great accuracy is required, the quantities muſt be determined by calculation. For this purpoſe, draw the diameter RS, at right angles to the meridian, and cutting the ellipſe in the point m. Then in the right angled ſpherical triangle mEu †, right angled at E, it will be neceſſary to find the arcs uF, Fm, and the angles muE, umE.—The angle muE muſt be firſt obtained by ſolving the right angled ſpherical triangle EFu, right angled at F.—the arc EF being 22° 28′ the latitude of the ſtar, and the angle FEu 18° 11′. From theſe data, the angle FuE will be found 73° 21′—the arc Fu 7° 9′—the angle FmE 28° 31′, and the arc Fm 49° 21.—To find the aberration in right aſcenſion anſwering to 2^s and 8^s,——3^s and 9^s,——4^s and 10^s &c. in the projection, add to the log. ſine of the angle EuF $=$ 73° 21′ the log. of 20, and from that ſum deduct 10 for a conſtant log. to the conſtant log. add ſeparately, the log. ſines of the arcs u2, u3, u4, &c. from each of theſe ſums, deduct 10, and the numbers anſwering to the log. remainders, will be the values of 2h, 3i, 10t, &c. Each of thoſe values being multiplied by the natural ſecant of the ſtar's declination, will give the effect in right aſcenſion, as in the following examples.

* The algebraic ſigns of + plus, and — minus.

† An ellipſe may be conſidered a circle in the orthographical projection of the ſphere, the ſemi-conjugate being the co-ſine of the circle's elevation above the primitive.

Angle

Angle E u F 73° 21′ Log. S.	9.98141		
Add 20 Log.	1.30103		
	1.28244	Conftant Log.	
Conftant - Log.	1.28244		
Add arc u 2^s = 14° 41′ Log. S.	9.40394		
	0.68638	= 4.86 = $2^s\ h$	for 2^s and 8^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 6″.35	but 5^s and 11^s of the Sun's Longitude
Conftant - Log.	1.28244		
Add arc u 3^s = 44° 41′ Log. S.	9.84707		
	1.12951	= 13.48 = $3^s\ i$	for 3^s and 9^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 17″.62	but 6^s and 6^s of the Sun's Longitude
Conftant - Log.	1.28244		
Add arc u 4^s = 74° 41′ Log. S.	9.98429		
	1.26673	= 18.48 = $10^s\ t$	for 4^s and 10^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 24″.15	but 7^s and 1^s of the Sun's Longitude
Conftant - Log.	1.28244		
Add arc u 5^s = 104° 41′ Log. S.	9.98750		
	1.26994	= 18.62 = $11^s\ p$	for 5^s and 11^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 24″.34	but 8^s and 2^s of the Sun's Longitude
Conftant - Log.	1.28244		
Add arc u 6^s = 134° 41′ Log. S.	9.85924	= 13.85 = $0^s\ y$	for 6^s and 0^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 18″.10	but 9^s and 3^s of the Sun's Longitude
Conftant - Log.	1.28244		
Add arc u 7^s = 164° 41′ Log. S.	9.44862	= 5.38 = $1^s\ x$	for 7^s and 1^s in the projection
Multiplied by nat. Sec^t.	40° 6′ =	× 1.307 = 7″.03	but 10^s and 4^s of the Sun's Longitude.

In this manner the calculations may be expeditioufly made for any degree of the Sun's place in the ecliptic.

The aberration in right afcenfion is additive, when a point 3^s behind the Sun's longitude falls on the left fide of the meridian of the ftar; the right afcenfion, or point M, being held from you; but negative when the point falls on the contrary fide of the meridian.

The foregoing equations when tabled will ftand as follows:

Sun's Long.				Sun's Long.
0^s	—	17″.62	+	6^s
1		24.15		7
2		24.34		8
3		18.10		9
4		7.03		10
5	+	6.35	—	11
6		17.62		0

To obtain the aberration in declination, the angle E*mu* is to be ufed in the fame manner the angle E*um* was in the cafe of right afcenfion; and the perpendiculars $3^s u$, $2^s o$, $1^s s$, $0^s v$, and $11^s r$, let fall upon the diameter at right angles to the meridian of the ftar, will be the equations required.

Angle E*mu* 28° 31′	Log. S. 9.67889		
Add 20 -	Log. 1.30103		
	0.97992 Conftant Log.		
Conftant -	Log. 0.97992		
Add arc m 3^s 11° 49′	Log. S. 9.31129		
	0.29121	= 1″.96 = 3 *u*	for 3^s and 9^s in the projection but 6^s and 0^s of the Sun's Longitude
Conftant -	Log. 0.97992		
Add arc m 2^s = 41° 49′	Log. S. 9.82396		
	0.80388	= 6″.36 = 2 *o*	for 2^s and 8^s in the projection but 5^s and 11^s of the Sun's Longitude
Conftant -	Log. 0.97992		
Add arc m 1^s = 71° 49′	Log. S. 9.97775		
	0.95767	= 9″.07 = 1 *s*	for 1^s and 7^s in the projection but 4^s and 10^s of the Sun's Longitude
Conftant -	Log. 0.97992		
Add arc m 0^s = 101° 49′	Log. S. 9.99070		
	0.97062	= 9″.35 = 0 *v*	for 0^s and 6^s in the projection but 3^s and 9^s of the Sun's Longitude
Conftant -	Log. 0.97992		
Add arc m 11^s = 131° 49′	Log. S. 9.87232		
	0.85225	= 7″.12 = 11 *r*	for 11^s and 5^s in the projection but 2^s and 8^s of the Sun's Longitude
Conftant -	Log. 0.97992		
Add arc m 10^s = 161° 49′	Log. S. 9.49424		
	0.47416	= 2″.98 = 10 *r*	for 10^s and 4^s in the projection but 1^s and 7^s of the Sun's Longitude.

The

The aberration in declination is negative, when a point 3 behind the Sun's longitude, falls on the ſame ſide of a diameter at right angles to the meridian of the ſtar, with the ſtar's right aſcenſion or point M; but the contrary is to be obſerved when the point falls on the oppoſite ſide. The foregoing equations when tabled will ſtand as below.

Sun's Long.					Sun's Long.
	0^s	+	1.96	—	6^s
	1	—	2.98	+	7
	2		7.12		8
	3		9.35		9
	4		9.07		10
	5		6.36		11
	6		1.96		0

Of Nutation.

THE nutation or libratory motion of the Earth's axis is occaſioned by the inclination of the Moon's orbit to the ecliptic, and the retrograde revolution of her nodes; which is performed in about 18 years and 7 months. On which account the action of the Moon on the equatorial, or longer diameter of the Earth, is not uniform, and muſt therefore from the principles of gravity produce a motion in the Earth's axis, which will be apparently in the ſtars. For the completion of this diſcovery, we are indebted to the very laborious, and ingenious Dr. Bradley.* This effect of the Moon has been ſettled by a ſeries of accurate obſervations, and therefore not to be conſidered as a ſpeculative argument in favour of the Newtonian Philoſophy; but an abſolute confirmation of it.

It muſt be evident from the theory, that the poles of the equator will complete a retrograde revolution about the mean poles, in the ſame period which completes a revolution of the Moon's nodes: But as the action of the Moon on the equatorial diameter of the Earth, will be ſomewhat varied in different ſituations of her nodes, this revolution of the poles will not be performed in a circle, but a ſmall ellipſe, with the tranſverſe diameter lying in the ſolſtitial colure, and amounting to 19.1,

* Vide his paper upon this ſubject in Vol. 45, N° 1. of the Tranſactions of the Royal Society.

and

and the conjugate in the equinoctial colure, and has been settled at 14″.2.—Let P (fig. 3 Plate I.) represent the mean northern axis of the earth.—AB a portion of the solstitial colure, and CD a portion of the equinoctial colure.—From P, each way on AB, lay off 9.55, suppose to F, and G, then from the same point P, lay off each way 7.1, suppose to E and H, through FHGE describe an ellipse, and it will represent the path described by the axis of the Earth. When the Moon's ascending node is in the beginning of ♈, the northern axis of the Earth will be at F, when the same node is in the beginning of ♈, the pole will be at H.—constantly 3ˢ before the Moon's ascending node. From these elements it is evident, that the obliquity of the ecliptic must be subject to a periodical change, being greater by 19″.1, when the Moon's ascending node is in ♈, than when in ♎: and the equinoctial points will also be subject to an equation, which will be a maximum when the Moon's ascending node is in the beginning of ♋ and ♑; this equation is common to all the stars.

As in the case of aberration, it will be proper to make the calculations from an orthographical projection.—From any scale of equal parts take 9.55, and with that distance for a radius describe a circle, which divide into twelve equal parts for the signs in right ascension; which designate by numerical letters; (as in Fig. 4, Plate I.) join III, and IX with a diameter, to represent a portion of the solstitial colure; and O, and VI, for a portion of the equinoctial colure, from the centre C, towards O, and VI, lay off 7.1, and designate those points by o, and 6, then through the points o, III, 6 and IX, describe an ellipse; which must be divided similar to the primitive answering to the places of the Moon's ascending node; and to prevent confusion in the explanation, it will be convenient to designate the signs by figures.

To apply β Medusæ to the projection,* lay off its right ascension 1ˢ 13° 43′ from 0ˢ, in the primitive, according to the order of the signs to the point A, then from A, through the centre C, draw the diameter AB for the meridian of the star; which cross at right angles by the diameter DE. This being done,

* This projection will serve for any star; on which account it differs from a projection for aberration.

suppose

ſuppoſe the place of the Moon's aſcending node to be at 1, and the pole of the Earth being 3ˢ before the Moon's aſcending node, will be at 4ˢ in the ellipſe: and the occult line 4 *a*, at right angles to the meridian of the ſtar, will be the nutation in right aſcenſion anſwering to 1ˢ, and 7ˢ, of the longitude of the Moon's aſcending node, but with contrary ſigns of application. The diſtance C *a*, or the occult line 4*b*, in the direction of the meridian will be the nutation in declination. The diſtance 4 *a*, meaſured on the ſame ſcale by which the projection is made, will give 8″.44, and the diſtance 4*b* will give 3″.15: But the firſt muſt be reduced to the equator, which is moſt conveniently done by multiplying it by the natural tangent of the ſtar's declination.

When great accuracy is required, recourſe muſt be had to calculation, which may be done in the ſame manner as purſued in aberration. It has already been obſerved that an ellipſe may be conſidered as a circle in the orthographical projection of the ſphere, and therefore the arc C *o*, which is the meaſure of the angle C 3 *o*, will be had by adding 10, to the log. of 7.1, and from that ſum deducting the log. of 9.55 the remainder will be the log. ſine of the arc C *o* which will be about 48° 2′. Then in the right angled ſpherical triangles C *o g*, and C *o e*, right angled at *o*, it is required to find the angles C *g o*, C *e o*, and the axis *o g*, *o e*, the angle *o* C *g* being the right aſcenſion of the ſtar, and the angle *o* C *e* its complement, and therefore both given. The angle C *g o* will be 62° 28′, the arc *o g* = 35° 25′, the angle C *e o* = 61° 6′, and the arc *o e* = 37° 52′. Theſe being the neceſſary requiſites, the nutation in right aſcenſion will be had as follows

To - 9.55	-	Log.	0.98000
Add angle C *g o* = 62° 28′	-	Log. S.	9.94780
			10.92780
Deduct - -	-		10
			0.92780 Conſtant Log.
Conſtant -	-	Log.	0.92780
Add arc *g* 1 = 5° 25′	-	Log. S.	8.97496
			9.90276

As

As radius cannot be deducted, the number will be a fraction, and the index of the log. being 9, the log. fraction will be - - $\bar{1}.90276 = .80 = 1\,m$ { for 1^s and 7^s in the projection

Multiply by nat. tang^t. of 40° 6′ = the ſtar's declination = × .842 = .67 { but 10^s and 4^s of the Longitude of Moon's node

Conſtant - - Log. 0.92780
4 Add arc $g\,o$ = 35° 25′ - Log. S. 9.76307
0.69087 = 4.91 = $0\,n$ { for 0^s and 6^s in the projection
Multiply by - - - × .842 = 4″.13 { but 9^s and 3^s of the Long. of Moon's node

Conſtant - - Log. 0.92780
Add arc g 11 = 65° 25′ - Log. S. 9.95873
0.88653 = 7.70 = $11\,p$ { for 11^s and 5^s in the projection
× .842 = 6″.48 { but 8^s and 2^s of the Long. of Moon's node
Multiply by - - -

Conſtant - - Log. 0.92780
Add arc g 10 = 95° 25′ - Log. S. 9.99850
0.92630 = 8.44 = $10\,q$ { for 10^s and 4^s in the projection
× .842 = 7″.11 { but 7^s and 1^s of the Long. of Moon's node
Multiplied by - - -

Conſtant - - Log. 0.92780
Add arc g 9 = 125° 25′ - Log. S. 9.91114
0.83894 = 6.40 = $9\,r$ { for 9^s and 3^s in the projection
Multiply by - - - × .842 = 5″.81 { but 6^s and 0^s of the Long. of Moon's node

Conſtant - - Log. 0.92780
Add arc g 8 = 155° 25′ - Log. S. 9.61911
0.54691 = 3.55 = $8\,s$ { for 8^s and 2^s in the projection
× .842 = 2″.96 { but 5^s and 11^s of the Long. of Moon's node
Multiply by - - -

In

In applying the nutation in right afcenfion, obferve this general rule, that when a point 3 before the longitude of the Moon's afcending node, falls on the right fide of the meridian of the ftar, the point A or right afcenfion being held from you, the nutation will be pofitive for ftars having north declination, but negative for fouth:—the contrary is to be obferved when a point 3ˢ before the Moon's afcending node, falls on the left fide of the meridian. Agreeably to thefe directions, the foregoing equations when tabled will ftand as follows.

Longitude of Moon's Afcending node				Longitude of Moon's Afcending node
0^s	—	5″.81	+	6^s
1		7.11		7
2		6.48		8
3		4.13		9
4		0.67		10
5	+	2.96	—	11
6		5.81		0

The next equation is that of the equinoctial points, which is common to all the ftars, and occafioned by the poles of the Earth inclining to, and receding from the celeftial equator.—Suppofe the Moon's afcending node to be at 9^s, then the pole of the Earth will be at o in the ellipfe, and the diftance C *o* will be its inclination towards ♈.—This inclination for any point in the ellipfe will be a perpendicular let fall upon the tranfverfe axis, which will be to the alteration of the equinoctial points, as the tangent of the obliquity of the ecliptic, is to radius;—hence thefe deviations from the tranfverfe axis of the ellipfe being multiplied by the nat. co-tangent of the obliquity of the ecliptic, will give the equations required.

The quantity C *o* = 7.1 { for 0^s and 6^s in the projection but
Mult. by nat. Co-tang. of 23° 28′ = × 2.3 = 16″.3 { for 3^s and 9 of the long. of ☽'s node.

For any other points in the ellipfe add the log. of 9.55, to the log. fine of the arc C *o*, and from that fum deduct 10 for a conftant log. to which add the log. fine of any arc from 3, or 9, and from that fum deduct 10, the remainder will be the log. of a perpendicular let fall from the termination of that arc to the tranfverfe axis.

To	9.55	Log.	0.98000
Add arc $C o = 48^{\circ}\ 2' =$	-	Log.	9.87130
			0.85130 Conftant Log.
to Conftant -	-	Log.	0.85130
Add arc $3^{s}\ 1^{s} = 60^{\circ}$	-	Log. S.	9.93753
			$0.78883 = 6.15 = 1\,c$
Mult. by nat. Co-tang^{t} of $23^{\circ}\ 28' =$			$\times\ 2.3 = 14''.14$

$\left\{\begin{array}{l}\text{for } 1^{s}\ 5^{s}\ 7^{s} \text{ and } 11^{s} \text{ in the projection but}\\ \text{for } 2^{s}\ 4^{s}\ 8^{s} \text{ and } 10^{s} \text{ of the Long. of Moon's node}\end{array}\right.$

Conftant -	-	Log.	0.85130
Add arc $3^{s}\ 2^{s} = 30^{\circ}$	-	Log. S.	9.69897
			$0.55027 = 3.55 = 2\,f$
Mult. by nat. Co-tang^{t} of $23^{\circ}\ 23'' =$			$\times\ 2.3 = 8''.16$

$\left\{\begin{array}{l}\text{for } 2^{s}\ 4^{s}\ 8^{s} \text{ and } 10^{s} \text{ in the projection but}\\ \text{for } 1^{s}\ 5^{s}\ 7^{s} \text{ and } 11^{s} \text{ of the Long. of Moon's node}\end{array}\right.$

Thefe equations are additive when a point 3^{s} before the longitude of the Moon's afcending node falls on the fame fide of the tranfverfe axis with 0^{s}, but the contrary when the point falls on the other fide, and when tabled will ftand as below.

Longitude of the Moon's Afcending node				Longitude of the Moon's Afcending node
0^{s}	—	$0''.0$	+	6^{s}
1		8.16		7
2		14.14		8
3		16.33		9
4		14.14		10
5		8.16		11
6	+	0 0	—	0

As theſe equations are neceſſary in every caſe relative to the right aſcenſion of the ſtars, (and common to them all,) it will be found very convenient for thoſe concerned in aſtronomical reſearches to make out a table for every degree of the quadrant.

For the nutation in declination, proceed as follows.

To the angle $Ceo = 61^\circ\ 6'$		Log. S.	9.94224		
Add	9.55	Log.	0.98000		
			0.92224	Conſtant Log.	
Conſtant		Log.	0.92224		
Add arc $e\ 11 =$	$7^\circ\ 52'$	Log. S.	9.13630		
			0.05854	$= 1''.14 = 11\ d$	for 11^s and 5^s in the projection but 8^s and 2^s of the Long. of Moon's node
Conſtant		Log.	0.92224		
Add arc $eo =$	$37^\circ\ 52'$	Log. S.	9.78804		
			0.71028	$= 5''.13 = 0\ v$	for 0^s and 6^s in the projection but for 9^s and 3^s of the Long. of Moon's node
Conſtant		Log.	0.92224		
Add arc $e\ 1 =$	$67^\circ\ 52'$	Log. S.	9.96676		
			0.88900	$= 7''.75 = 1\ t$	for 1^s and 7^s in the projection but for 4^s and 10^s of the Long. of the Moon's node
Conſtant		Log.	0.92224		
Add arc $e\ 2 =$	$97^\circ\ 52'$	Log S.	9.99589		
			0.91813	$= 8''.28 = 8\ u$	for 2^s and 8^s in the projection but for 5^s and 11^s of the Long. of Moon's node
Conſtant		Log.	0.92224		
Add arc $e\ 3 =$	$127^\circ\ 52'$	Log. S.	9.89732		
			0.81956	$= 6''.60 = 9\ x$	for 3^s and 9^s of the projection but for 6^s and 0^s of the Long. of Moon's node
Conſtant		Log.	0.92224		
Add arc $e\ 4 =$	$157^\circ\ 52'$	Log. S.	9.57607		
			0.49831	$= 3''.15 = 10\ y$	for 4^s and 10^s of the projection but for 7^s and 1^s of the Long. of Moon's node.

 In

In applying the equations for nutation in declination obſerve, that when a point 3^s before the longitude of the Moon's aſcending node falls on the ſame ſide of a diameter at right angles to the meridian of the ſtar with its point of right aſcenſion, the nutation will be additive for ſtars having north declination, but negative for thoſe having ſouth declination; the contrary is to be obſerved when a point 3^s before the longitude of the Moon's aſcending node falls on the other ſide of the diameter. The above equations for nutation in declination will be properly expreſſed in the following table.

Longitude of the Moon's Aſcending Node				Longitude of the Moon's Aſcending Node
0^s	+	6″.60	—	6^s
1		3.15		7
2	—	1.14	+	8
3		5.13		9
4		7.75		10
5		8.28		11
6		6.60		0

The foregoing calculations as combined with the projections, may be rendered ſomewhat more ſimple, by numbering the ſigns of the Sun's place in the ellipſe for aberration 3^s ſhort of the true ſigns; and the ſigns for the place of the Moon's aſcending node in the ellipſe for nutation 3^s forward, by which the calculations will coincide with the ſigns for which they were made, and ſo much of the rules for the application of the equations as depend upon a point 3^s behind the place of the Sun for aberration, and 3^s before the place of the Moon's aſcending node for nutation, will become unneceſſary.

There is yet one other equation which, in very nice operations, ſuch as determining the lengths of meridians, &c. may require ſome attention. It is the effect of the inequality of the action of the Sun between the ſolſtices and equinoxes, on the equatorial diameter of the earth, by which the poles are carried annually, twice round the mean poles in a ſmall circle, whoſe diameter is 1 . By which the equinoctial points, the obliquity of the ecliptic, the right aſcenſion, and declination of the ſtars, are affected in a ſmall degree. The maximum of the alteration

teration of the equinoctial points amounts 1″.15 or the $\frac{1}{13}$ of a second in time. The obliquity of the ecliptic is greater by 1″, when the Sun is in the equinoxes, than in the solstices. The right ascension of the stars will be insensibly affected, unless the declinations should be very great: the declination of 88° 6′ will produce but 1″ in time, and 81° 15′ but $\frac{1}{2}$ of a second. From the theory the apparent distance of every star from the pole of the equator will be subject to a variation of 1″ twice a year, and there being but three months between the greatest inclination, and reclination, it will sensibly affect the observations made with a good 8 feet zenith sector.—For a further explanation, and in aid to the calculations, take from any scale of equal parts 5, with that distance for a radius describe a circle, which divide into 12 equal parts for signs, (see Fig. 5. Plate I.) From what has been already observed it follows that, when the Sun is at 0, the pole will be at 3, when the Sun is at 3, the pole will be at 9, and when the Sun is at 6, the pole will be again at 0. For an example: Suppose it should be required to find the effect of the semi-annual equation in declination for β Medusæ, answering to 3^s of the Sun's place—lay off 1^s 13° 43′, the right ascension of β Medusæ from 0^s, to M; from M, through the centre C, draw the meridian MD; at right angles to which, draw the diameter EF. Then from the theory, whilst the Sun is advancing 3^s, the pole will advance 6^s, and therefore be at 9^s: and the distance 9 *m*, in the direction of the meridian, will be the quantity required, and when applied to the scale, will be .34.—this quantity may be readily calculated, being to the sine of arc 9 F = 43° 43′ as .5 is to radius, therefore

To .5	-	Log.	$\bar{1}.69897$
Add 43° 43′	-	Log. S.	9.83954
			9.53851

As radius cannot be deducted, the log. must be expressed $\bar{1}.53851 = .34 = 9\,m$: in this mannner the calculations may be made for any other points in the circle, and the quantities will be additive to the declination of a northern star; when the pole is on the same side of a diameter at right angles to the meridian with the point M, of the star's right ascension; but negative for a southern star;—the contrary is to be observed when the pole is on the other side of the diameter.

The

The following table by attending to the direction will anſwer for all ſtars.

	Argument. From twice the Sun's Longitude take the ſtar's right aſcenſion.					
If the difference be leſs than 6^s add for northern ſtars, but ſubtract for ſouthern.	$0^s\ 0^o$	$6^s\ 0^o$	$+0''.0-$	$6^s\ 0^o$	$0^s\ 0^o$	If the difference be more than 6^s ſubtract for northern ſtars, but add for ſouthern.
	15	15	0.13	15	15	
	1 0	5 0	0.25	7 0	11 0	
	15	15	0.35	15	15	
	2 0	4 0	0.43	8 0	10 0	
	15	15	0.48	15	15	
	3 0	3 0	0.50	9 0	9 0	

For an example of the application of the foregoing equations, let it be required to find the right aſcenſion, and declination of β Meduſæ for June 22d 1795; the Sun's longitude being 3^s and the longitude of the Moon's aſcending node 4^s.

Right aſcenſion of β Meduſæ the beginning of 1780.	$1^s\ 13^o\ 29'\ 7''.0$
Annual variation for 15 years	+ 14 22.9
Do for June 22d. - -	+ 27.0
Mean right aſcenſion - -	1 13 43 56.9
Aberration - -	— 9.35
Nutation - -	— 0.67
Equation of the equinoctial points	— 14.14
True right aſcenſion -	1 13 43 32.74

Declination of β Meduſæ the beginning of 1780.	$40^o\ 5'\ 37''.0$ N
Annual variation for 15 years	+ 3 39.45
Do for June 22d	+ 6.96
Mean declination - -	40 9 23.41
Aberration - -	— 9.35
Nutation - -	— 7.75
Semi-annual equation -	— .34
True declination - -	40 9 5.97

I am, Sir,

Your real Friend,

ANDREW ELLICOTT.

To Robert Patterſon, A. M.

A Letter

N°. VIII.

A Letter from Mr. ANDREW ELLICOTT, *to* Mr. ROBERT PATTERSON.

A Method of Calculating the Eccentric Anomaly of the Planets.

Philadelphia, April 4th, 1794.

SIR,

Read April 4, 1794.

HAVING occaſion ſome years ago to conſtruct a ſet of aſtronomical tables for the planet ♅, I made uſe of an operation to obtain the eccentric anomaly, the firſt part of which I believe to be new; the ſecond, is ſimilar to the method made uſe of by Sir Iſaac Newton in his Principia.—He firſt aſſumes an arc, and then proceeds to find its error: but by the method which I have purſued, we proceed directly to the ſolution of the problem without any aſſumption, and therefore adhere more cloſely to the principles of geometry.——The firſt part of the operation will give the eccentric anomaly almoſt ſufficiently exact for any of the planets belonging to our ſyſtem; and the ſecond which is very eaſy, will produce a greater degree of exactneſs than is requiſite for any of the bodies revolving round our Sun, the comets excepted. The method is as follows.

Let S, Fig. 9. Plate I. repreſent the Sun, and the arc AN the mean anomaly; join, SN—through the centre C; draw CP parallel to SN, and the angle ACP will be nearly the eccentric anomaly; and may be had by the following analogy.—From the log. tang^t. of half the mean anomaly, ſubtract the difference of the logs. of the aphelion and perihelion diſtances, the remainder will be the log. tang^t. of an angle, to which add half the mean anomaly, and the ſum will be the angle ACP.———For an example, take the planet ♅.

Let the mean anomaly AN=60° the half 30° log. tang^t.—9.7614394
Log. Aphelion diſt. 6.3007704 }
Log. Perihelion diſt. 6.2594052 } Deduct the difference —0.0413649, and
there remains log. tang^t. 9.7200754 which
anſwers

anſwers to 27° 41′ 41″, to which add half the mean anomaly, and the
30
ſum - - - 57 41 41 will be the angle ACP, which in this example will be about 3″ too ſmall, becauſe the right line ST, drawn at right angles to PC continued if neceſſary, and which is nearly the part to be deducted from the mean anomaly, will be ſhorter than the arc PN.—Then to find the value of ST:—ſuppoſe CA, or CP, to be equal to 1 or unity, then from the elements of ♅, SC will be equal to .04758735 and CP the radius being equal to an arc of 57°.29578,—SC will be equal to 2°, 7266,—then

As radius	- -	Log.	10.0000000
Is to SC,	2° .7266	Log.	0.4356115
So is the S. of TCS,	57° 41′ 41″	Log.	9.9269660
To ST, = 2° .3045	- -	Log.	0.3625775

60
18′ .2700
60
16″.2000. This arc of 2° 18′ 16″ being deducted from the mean anomaly will leave 57° 41′ 44″ for the eccentric anomaly corrected, and will be true within the $\frac{1}{5}$ part of a ſingle ſecond. If a greater degree of accuracy ſhould be requiſite, the corrected angle 57° 41′ 44″ which ſuppoſe to be ACO, muſt be uſed to obtain the value of SR, and that value applied as above. This correction will only be neceſſary in caſes where the orbits are very eccentric.

But as the planets do not revolve in circular, but elliptical orbits, the point O, in the arc AN, muſt be reduced to *r* the place of the planet in the curve of the ellipſe; which is the point cut by the right line OF drawn at right angles to AD.—The angle AS *r* will then be the co-equate, or true anomaly; and may be had by the following analogy.—From the log. tang^t. of half the eccentric anomaly, take the difference of the logs. of the aphelion and perihelion diſtances, and the remainder will be the log. tang^t. of an angle; to this angle add half the eccentric anomaly, and call the ſum U. Then to the log tang^t. of U, add half the ſum of the logs. of the aphelion and perihelion diſtances; from that ſum deduct the log. of the mean diſtance, and the remainder will be the log. tang^t. of the co-equate, or true anomaly.—For example

From half the eccentric anomaly 28° 50′ 52″ log. tang. 9.7410263
Deduct the diff. of the logs. of the aphelion and perihelion diſtances 0.0413649
The remainder 9.6996614 will be log. tang^t. of 26° 36′ 5″
Add ½ the eccentric anomaly - - 28 50 52
Call the ſum U - - - - 55 26 57

Then

PLATE. 1.

Fig. 2. P. 54.

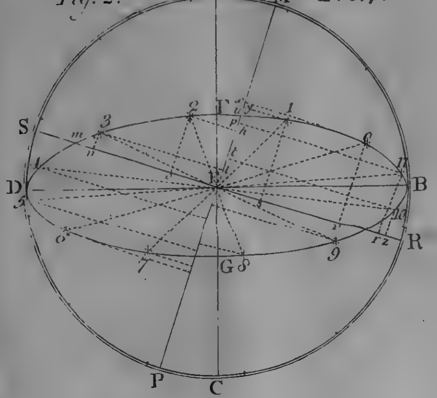


Fig. 1. P. 51.

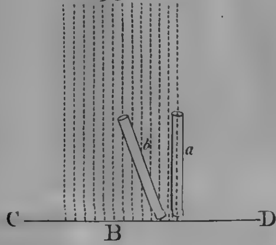


Fig. ϴ P. 27.

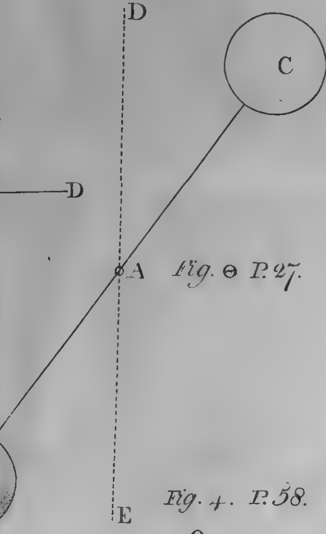


Fig. 3. P. 58.

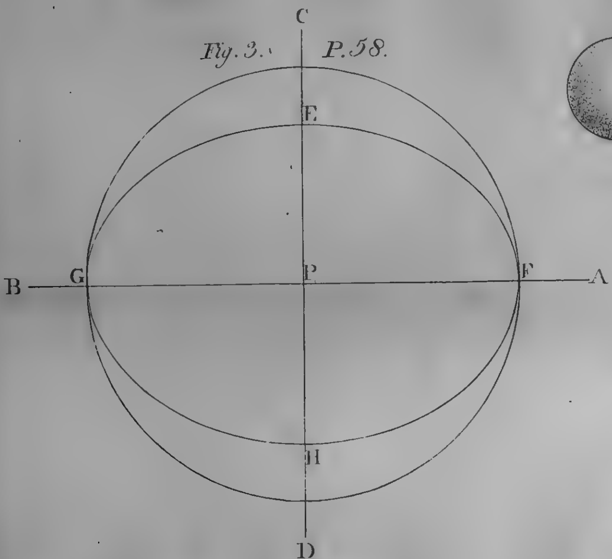


Fig. 4. P. 58.

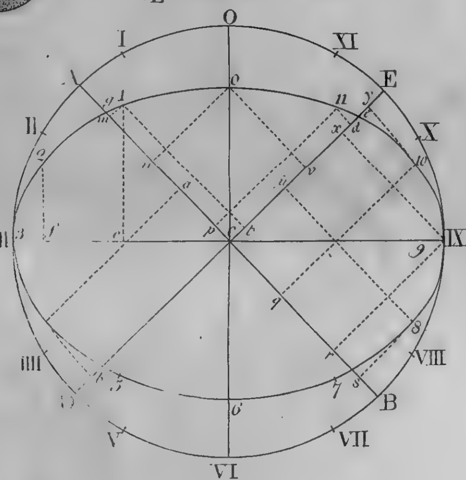


Fig. 5. P. 65.

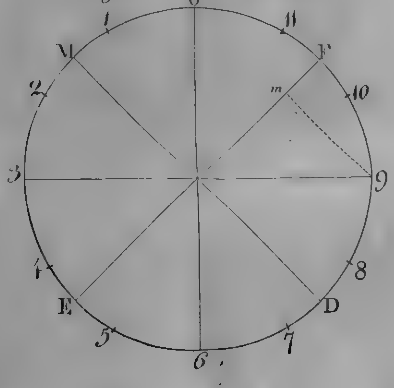
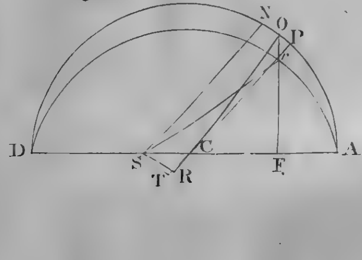


Fig. 9. P. 67.



Then to U* $= 55^\circ\ 26'\ 57''$ Log tangᵗ.	10.1620405
Add half the ſum of the logˢ. of the aphelion and perihelion diſt.	6.2800876
	16.4421281
Deduct the log. of the mean diſt.	6.2805800
The remainder is the log. tangᵗ. of $55^\circ\ 25'\ 7''$	10.1615481

The co-equate or true anomaly $55^\circ\ 15'\ 7''$ is the meaſure of the angle AC*r*, and when deducted from the mean anomaly will leave the equation of the centre: as for example, $55^\circ\ 25'\ 7''$ taken from 60° the mean anomaly uſed in the foregoing explanation the remainder $4^\circ\ 34'\ 53''$ will be the equation of the centre anſwering to it.—The equation of the centre muſt be negatively applied while the planet is moving from the aphelion, to the perihelion, and *vice verſa.*

I am, Sir, with much eſteem,
Your real Friend,
ANDREW ELLICOTT.

To Mr. Robert Patterſon.

N°. IX.

Method of raiſing the common Logarithm of any Number immediately, by DAVID RITTENHOUSE, *Preſident of the Society.*

Read Aug. 12, 1795. THE logarithm of any number is the index of that power of 10 which is equal to the given number. This index will always be fractional, unleſs the number be diviſible by 10 without any remainder.

If the number be greater than 10, divide it by the higheſt power of 10 that will leave the quotient not leſs than 1. The index of that power is the firſt figure, or index of the logarithm. Divide 10 by the quotient ſo found raiſed to the higheſt power that will leave the new quotient not leſs than unity. Divide

* *Note.* When U exceeds 90° take its ſupplement and in that caſe deduct the reſult of the calculation from two right angles, and the remainder will be the true anomaly.

the laſt diviſor by the laſt quotient raiſed to its proper power, and proceed in this manner until a ſufficient number of diviſions are made, which will be when the quotient approaches nearly to unity. Make a compound fraction, taking the ſucceſſive indexes of the powers you divide by for denominators and unity for numerators. Reduce this compound fraction to a ſimple one, and that by diviſion to a decimal fraction, which together with the index firſt found (if any) will be the logarithm required.

Example of the Calculation.

Required the Logarithm of 99.

Divided by $\frac{99}{10^1} = 9.9$. Here 1 is the index.

Divided by $\frac{10}{9.9^1} = 1.010101 = a$.

a, raiſed to its higheſt power, 228.

```
aa = 1.020304
       20406
         306
           4
--------------
a⁴ = 1.041020
       41641
        1041
          21
--------------
a⁸ = 1.083723
       86698
        3251
         759
          22
           3
--------------
a¹⁶ = 1.174456
       117446
        82212
         4698
          470
           59
            7
--------------
a³² = 1.379348
```

```
First quotient,        9.9      }
Divided by a²²⁸ = 9.889521      }  = b = 1.001059
                  --------
                   .010479
                      9889
                  --------
                       590
                       494
                  --------
                        96
                        89
                  --------
                         7

                 a = 1.010101   }
Divided by b⁹ = 1.009570        }  = c = 1.000526
                  --------
                   .000531
                       505
                  --------
                        26
```

```
b  = 1.001059
        1001
          59
-------------
b² = 1.002119
        2004
         119
-------------
b⁴ = 1.004242
        4017
         201
          42
-------------
b⁸ = 1.008502
× by b  1009
          59
-------------
b⁹ = 1.009570
= c = 1.000526
          526
-------------
c² = 1.001052
```

Calculation Continued.

$a^{32} = 1.379348$
413804
96554
12414
415
55
11

$a^{64} = 1.902600$
1.712340
3805
1141

$a^{128} = 3.619886$
$\times$ by a^{64} 3.257897
7240
2172

$a^{192} = 6.887195$
$\times$ by a^{32} 2.066158
482104
61985
2066
275
55

$a^{224} = 9.499838$
$\times$ by a^{4} 379993
9500
190

$a^{228} = 9.889521$

Divided by $\left.\begin{array}{l} b = 1.001059 \\ c^2 = 1.001052 \end{array}\right\} = d = 1.000007$

7

The quotient d, is now ſo ſmall that it is not neceſſary to proceed further in this way, for the decimals of c, divided by the decimals of d will give the power required, viz. 75.

Making a compound fraction, as before directed, with the ſeveral powers of the diviſors in the order they ſtand we have.

$$\frac{1}{1\frac{1}{228\frac{1}{9\frac{1}{2\frac{1}{75}}}}}$$

Which reduced 75

Gives the $151 = 75 \times 2 + 1$

$1434 = 151 \times 9 + 75$

Simple fraction $= \dfrac{327103 = 1434 \times 228 + 151}{328537 = 327103 \times 1 + 1434}$

Denominator	Numerator	
328537	327103,0	.995635194.8. The decimal part of the log. of 99. true to the 9th place, and 3 too much in the 10th.
	2956833	
	3141970	
	2956833	
	1851370	
	1642685	
	2086850	
	1971222	
	1156280	
	985611	
	170669	
	164268	
	6400	
	3285	
	3115	
	2957	
	158	
	131	
	27	

N°. X.

Experiments on Evaporation, by C. WISTAR, M. D.

Read Feb. 5, 1796.

IN an Eſſay publiſhed in the laſt Volume of the Tranſactions of the Society, I deſcribed a ſpecies of evaporation which was excited by ſuſpending ice, at the melting point, in air reduced to the temperature of 0 of Fahrenheit's ſcale; and conſidered it as the effect of a general law of nature, in conſequence of which an inelaſtic vapour, (which commonly is viſible,) ariſes from water, and from wet ſubſtances, whenever they are warmer than the atmoſphere which ſurrounds them.

From facts ſtated in the ſame paper it was inferred, that this inelaſtic vapour does not depend upon any poſitive quantity or degree of ſenſible heat in the evaporating body, but upon a relative degree, exceeding that of the atmoſphere to which it is expoſed; and that it is produced by the paſſage of heat from the moiſt body into the contiguous air.—If this theory be true, it follows that a ſlow diſtillation may be performed, with the common apparatus, by applying cold to the receiver or refrigeratory, without increaſing the heat of the retort or ſubſtance to be diſtilled, as there will be a continual paſſage of heat from the body to be evaporated or diſtilled, into the air of the receiver.

Although, for the reaſons there given, I had no doubt of the truth of the doctrine advanced in my former paper, I was deſirous of ſubmitting it to the teſt of this experiment, becauſe it has been ſuggeſted that the vapour which appeared to ariſe from ice, might have ariſen from the mixture of different portions of air of different temperatures;

peratures; whereas by diſtilling or evaporating in a luted retort and receiver, there can be no mixture of warm and cold air; and by uſing a ſubſtance which is not contained in the atmoſphere, we ſhall avoid all ſuſpicion that the vapour which ariſes from it, may have originated from the air in the veſſels.

With theſe views I poured an ounce and half of vitriolic æther into a retort and luted it to a receiver with a long neck, which was placed in a mixture of ſalt and ſnow, while the retort was ſurrounded by air of the temperature of 50° of Fahrenheit. The frigorific mixture, from the impurity of the ſalt, was ſeldom below 10°, ſo that the difference, between the æther in the retort and the air in the receiver, did not exceed 40°.

When the apparatus had been thirty hours in this ſituation the frigorific mixture was removed, and one third of the æther was found diſtilled into the receiver.

I believe no cauſe can be aſſigned for this diſtillation but the paſſage of heat through the æther into the cold air of the receiver; and to be certain that the application of cold to the receiver *really produced it*, I prepared a ſimilar diſtilling apparatus, in the ſame manner preciſely, and placed the retort in contact with that of the other apparatus, while the receiver, inſtead of being chilled by the cold mixture, ſtood in air of the ſame temperature with the retorts, viz. 50°; but no diſtillation took place during thirty hours.

To vary the experiment, I placed ſome camphor in another apparatus prepared as above, and fixed the receiver in the frigorific mixture, while the retort ſtood in air of the temperature of 50°, at the expiration of thirty hours ſome of the camphor was found ſublimed, and the ſublimate had thoſe arboreſcent appearances which uſually attend it when produced by heat.

Memoir

N°. XI.

A Memoir concerning the Faſcinating Faculty which has been aſcribed to the Rattle-Snake, and other American Serpents. By BENJAMIN SMITH BARTON, M. D.*

FIDEM NON ABSTULIT ERROR.

Read April 4, 1794. NATURALISTS have not always been philoſophers. The ſlight and ſuperficial manner in which they have examined many of the ſubjects of their ſcience; the credulity which has accompanied them in their reſearches after truth, and the precipitancy with which they have decided upon many queſtions of importance, are proofs of this aſſertion.

There is a queſtion in natural hiſtory that has, in an eſpecial manner, ſolicited from me theſe obſervations. I mean the queſtion concerning the FASCINATING FACULTY, which has been aſcribed to different kinds of American ſerpents. It is my intention to examine this queſtion, in the memoir which I now preſent to the Philoſophical Society.

Of this faſcinating faculty we have all heard and read. In many of our country ſituations, there is hardly a man or a woman, who will not, when the ſubject comes to be

* Since this memoir was read before the Society, it has been conſiderably altered, and ſomewhat enlarged. I hope, the alterations will render it more worthy of the notice of thoſe who, like myſelf, derive pleaſure and happineſs from the contemplation of the works and operations of nature, on this globe.

I fear, I ſhall be thought to have treated the queſtion in too diffuſive a manner. I have not, indeed, laboured to be conciſe. But if the memoir is more extenſive than was neceſſary, I flatter myſelf, it will be admitted that it, at leaſt, contains ſome new and intereſting facts. I ſubmit it to its fate.

be mentioned, ſeriouſly relate ſome wonderful ſtory, as a convincing proof of the doctrine. Children ſeem taught to believe it. I think, it is ſometimes one of the earlieſt prejudices imprinted on their tender minds. It is a prejudice which often increaſes with their years; and even in that happy period of life when the mind is moſt firm, and the leaſt propenſe to the belief of extraordinary things, the ways of which we are not capable of ſcanning, I have known this prejudice ſo deeply and ſo powerfully rooted, as to mock the light and ſureneſs of facts, and all the ſtrength of reaſoning.

It is not my intention, in this memoir, to give an analyſis, or complete view, of every thing that has been written on the ſubject. Nor is it my intention to examine the many ſtories, related by authors, in ſupport of the faſcinating faculty of ſerpents. For the firſt taſk, I have not leiſure; and, as to the ſecond, I ſhould think my time ill employed in pointing out the groſs abſurdities which ſeem to conſtitute a neceſſary part of many of thoſe ſtories. I think it proper, however, to obſerve, that I have anxiouſly ſought for, and have patiently peruſed, the volumes of tales publiſhed in favour of the doctrine which I mean to controvert.

I aim at giving a general, though correct, view of the queſtion, uninfluenced by the bold aſſertions of ignorance, or by the plauſible conjectures of ſcience. In the inveſtigation of the queſtion, I have ſought for facts: theſe have been my guides. I have ſtudiouſly endeavoured to follow where they ſeemed to lead. Perhaps, they have led me aſtray.

The manner in which the ſuppoſed faſcinating power of ſerpents is exerted has often been related, by different writers. I ſhall endeavour to convey ſome idea of the buſineſs, in as few words as I can.

The

The ſnake, whatever its ſpecies may be, lying at the bottom of the tree or buſh upon which the bird or ſquirrel ſits, fixes its eyes upon the animal which it deſigns to faſcinate, or enchant. No ſooner is this done than the unhappy animal (I uſe, for the preſent, the language of thoſe who differ from me in opinion, on this ſubject) is unable to make its eſcape. It now begins to utter a moſt piteous cry, which is well known by thoſe who hear it, and underſtand the whole machinery of the buſineſs, to be the cry of a creature enchanted. If it is a ſquirrel, it runs up the tree for a ſhort diſtance, comes down again, then runs up, and, laſtly, comes lower down. "On that occaſion," ſays an honeſt but rather credulous writer*, "it has been obſerved, that the ſquirrel always goes down more than it goes up. The ſnake ſtill continues at the root of the tree, with its eyes fixed on the ſquirrel, with which its attention is ſo entirely taken up, that a perſon accidentally approaching, may make a conſiderable noiſe, without the ſnake's ſo much as turning about. The ſquirrel as before mentioned comes always lower, and at laſt leaps down to the ſnake, whoſe mouth is already wide open for its reception. The poor little animal then with a piteous cry runs into the ſnake's jaws, and is ſwallowed at once, if it be not too big; but if its ſize will not allow it to be ſwallowed at once, the ſnake licks it ſeveral times with its tongue, and ſmoothens it, and by that means makes it fit for ſwallowing †."

It would be eaſy to cite, from different authors, other accounts of the manner in which the enchantment is per-

* Profeſſor Peter Kalm.

† Travels into North-America; containing its natural hiſtory, and a circumſtantial account of its plantations and agriculture in general, &c. &c. vol. i. p. 317 & 318. Alſo vol. ii. p. 207, 208, 209 & 210. Engliſh Tranſlation. London: 1770 & 1771.

formed; or, more properly ſpeaking, of the conduct, or behaviour, of the enchanting and enchanted animals. But between theſe accounts, there is hardly a ſpecifick difference. There is conſiderable unity in all the relations that I have heard, or read. However, thoſe who wiſh to examine this part of the ſubject more fully, will, at leaſt, receive ſome degree of entertainment from the peruſal of the many authors who have believed and aſſerted, that ſerpents poſſeſs a power of faſcinating other animals.

That the belief in the exiſtence of this power ſhould have been ſo general among the uninformed part of a people, ought not to be wondered at. The human mind, unenlightened by ſcience, or by conſiderable reflection, is a ſoil rich in the weeds of ſuperſtition, and credulity. It is ever prone to believe in the wonderful, even when this belief, as is often the caſe, brings with it fears, and cares, and miſery. The bondage of the mind in ſuperſtitious credulity is great and heavy. Neither religion nor virtue can give it its freedom. This it obtains from ſcience. How important, then, even in this point of view, is the enlargement of the mind by ſcience!

But it is, ſurely, a matter of ſome aſtoniſhment, that this belief ſhould have been admitted, in all the fulneſs of its extravagance, by men of learning, of obſervation, and of genius: by thoſe who have the book of nature in their hands; that book which will, in ſome future and ſome happier age, eradicate many of the prejudices which disfigure, and which mock the dignity of, human nature: by claſſical ſcholars, grown old in the diſbelief of ſimilar fables, heightened and embelliſhed by the charms of poetry; and alſo by the infidel, who denies the authenticity of ſcripture-miracles, few of which, even though they were not ſhown to be truths, are more improbable than the imaginary fact which I am examining.

I have ſought to diſcover the original, or ſource, of this belief. I do not find any traces of it among the ancient writers of either Greece or Rome. I think, it is moſt likely that no ſuch traces can be found. Lucan, had ſerpents been thought to poſſeſs a faſcinating faculty in his age, and in the country in which he lived, would, probably, have availed himſelf of its exiſtence, in his beautiful account of the march of Cato's army through the Libyan-Deſert*; and had ſuch a notion prevailed in the earlier days of Lucretius, would we not find ſome mention made of it in the poem *De Rerum Natura*, one of the fineſt and moſt varied productions of the human mind? Claſſical ſcholars may poſſibly, however, diſcover the dawn of this notion in Greek and Roman authors, unread by me. On this ſubject, I have not puſhed my inquiries as far as I wiſhed to have done. It is not unlikely that I may examine the queſtion, more curiouſly, at ſome future period.

It is probable that in the mythology of Aſia and of Africa, we ſhall diſcover ſome traces of this notion, ſo intimately connected with the ſuperſtitious credulity of a people, and even ſo naturally ariſing out of an imperfect view of the manners of ſerpents.

If we may believe the Reverend Dr. Cotton Mather †, Mr. Dudley ‡, and other perſons, who had reſided in North-America, we are to look for the beginning of this ridiculous notion among our Indians. How far, however, this is really the caſe may, I think, be doubted. It is certain that, at preſent, the opinion is by no means univerſal among the Indians. Several intelligent gentlemen, who are well acquainted with the manners, with

* Pharſalia, lib. IX.

† The Philoſophical Tranſactions, abridged, vol. v. part ii. no. 339. p. 162.

‡ Ibid. vol. vi. part iii. no. 376. p. 45.

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the religious opinions, and with the innumerable ſuperſtitious prejudices of the Indians, have informed me, that they do not think theſe people believe in the notion in queſtion. My friend Mr. John Heckewelder, of Bethlehem, writes to me, that he does not recollect to have heard the Indians ſay that ſnakes charm birds; though he has frequently heard them ſpeak of the ingenuity of theſe reptiles in catching birds, ſquirrels, &c. Mr William Bartram ſays, that he never underſtood that the nations of Indians among whom he travelled had any idea of the faſcinating power of ſnakes*. On the other hand, however, a Mohegan-Indian told me that the Indians are of opinion that the rattle-ſnake can charm, or bewitch, ſquirrels and birds, and that it does this with its rattle, which it ſhakes, thereby inviting the animals to deſcend from the trees, after which they are eaſily caught. According to this Indian, his countrymen do not think that the ſnake, in any manner, accompliſhes the buſineſs with its eyes. A Choktah-Indian aſſured me that the rattle-ſnake does charm birds, &c. but he was honeſt enough to confeſs that he did not know in what manner it does it. The interpreter, through whom I converſed with this Indian, ſaid that the ſnake charms by means of its rattle.

The veneration, or regard, which has been paid to the rattle-ſnake by certain North-American tribes ſeems, at firſt ſight, to favour the opinion, that theſe tribes attributed to this hideous reptile ſome hidden power †, perhaps that of faſcinating animals. Mr. William Bartram informs me, that the ſouthern Indians, with whom he is acquainted, ſeem to hold the rattle-ſnake in a degree of veneration ‡. Mr. Heckewelder ſays that, to his cer-

* MS. note, communicated to me by this ingenious gentleman.
† Vis abdita. Lucretius.
‡ MS. note communicated to me.

tain knowledge, this reptile was once held in particular eſteem by the Delawares. He was ſeveral times prevented, by theſe Indians, from killing the rattle-ſnake, being told that it was their grand-father, and, therefore, muſt not be hurt. At other times, he was told, he muſt not kill this ſnake, becauſe the whole race of rattle-ſnakes would grow angry, and give orders to bite every Indian that might come in their way *. But, of late, eſpecially among thoſe Indians who have had connection with the whites, theſe ridiculous notions have mouldered away, and our Indians, at preſent, kill their rattling " grand-father" with as little ceremony as the Eſkemaux are ſaid to kill their parents in old-age.

It is obvious, from contemplating the manners and the hiſtory of nations, that a part of their religions, and a large part of the fabrick of their ſuperſtitious notions, have ariſen out of fear. Perhaps, all mankind † admit the exiſtence of two great beings, the one good and all-benevolent, the other bad and ſtudious of evil. In our own continent, where, I believe, this notion was univerſal, certain tribes were aſſiduous in their adoration of the latter being, whilſt the former, whom the light of reaſon taught them to conſider as the ſource of life, and all their

* In my *Hiſtorical and Philoſophical Inquiry* (not yet publiſhed), I have collected many facts which ſeem inconteſtably to prove, that the mythology, or ſuperſtitious religion, of the Americans is a fragment of that mythology whoſe range in Aſia, and in Africa, has been ſo extenſive. Poſſibly, the veneration, or regard, which was paid to different kinds of ſerpents in America did not originate in this continent, but had its ſource in Aſia, from which portion of the globe (after a long and laborious attention to the ſubject) I cannot doubt, that almoſt all the nations of America are derived. It is unneceſſary, in this place, to cite inſtances of the religious veneration which was, and ſtill is, paid to ſome ſpecies of ſerpents, in various parts of the old-world. Theſe inſtances muſt be familiar to every perſon, who is acquainted with the hiſtorians or with the poets of antiquity, and with the hiſtory of the Gentoo-Indians.

† I ſpeak of mankind in the aggregate, and not of individuals among them.

bleſſings,

bleffings, was merely acknowledged and named, but unworfhipped and neglected *. The Delawares, and fome other nations who fpeak dialects of their language, believe that a turtle, of an enormous fize, inhabits the deep, and fupports upon his back this continent, or, as they call it, ifland. They fay it is in the power of this animal, by diving, to drown the world, as he has already done, in former ages. They, therefore, endeavour to conciliate his friendfhip and good-will. With this view, they make rattles of the turtle-fhell, into which they put fmall ftones, beans, or Indian-corn †, and play with this inftrument, at their dances. The turtle is greatly efteemed by them; and, in the fulnefs of a mixed zeal and fear, they even deign to call him *Mannitto*, or God; becaufe, they fay, he can live both upon the land and in the water ‡.

It feems very probable to me, that the veneration for the rattle-fnake had its birth in fear, and not in the belief that this reptile poffeffed the power of fafcinating animals. If, as fome writers have afferted, the Indians were in poffeffion of abfolute fpecificks for the bite of the rattle-fnake, I am of opinion that the veneration for this animal would not have exifted; or, at leaft, that it would not long have continued. But the Indians are often unable to prevent or to cure the effects of the active poifon of this ferpent, which not unfrequently deftroys them §.

* John De Laet, fpeaking of the Indians of New-York, has the following words: "Cæterum nullus ipfis religionis fenfus, nulla Dei veneratio: diabolum quidem colunt fed non tam folemniter neque certis ceremoniis, ut Africani faciunt," &c. Novus Orbis feu Defcriptionis Indiæ Occidentalis Libri xviii. lib. iii. cap. xi. p. 75. Lugd. Batav. 1633.

† Maize.

‡ MS. by Mr. John Heckewelder, *penes me*.

§ Adair fays, he does "not remember to have feen or heard of an Indian dying by the bite of a fnake, when out at war, or a hunting; although they are then often bitten by the moft dangerous fnakes." *The Hiftory of the American Indians*, &c. p. 235. London: 1775. It is certain, from the teftimony

I return to the more immediate path of my ſubject.

Among the Indians of South-America, I do not find any traces of the notion that ſerpents can faſcinate other animals. Piſo, the author of the *Natural and Medical Hiſtory of the two Indies*, ſeems to have been ſtudious to bring together the extraordinary things which have been related of the rattle-ſnake. But he ſays not a ſyllable concerning the faſcinating faculty of this reptile*.

But whatever may have been the native country of the notion which I am conſidering, it would have been well had it been confined to ſavages. It is a tale which ſeems nicely adapted to the wit and ſociety of rude and uncultivated nations. Unfortunately, the progreſs of error and of credulity is extremely rapid. Their dominion is extenſive. The belief in the faſcinating faculty of ſerpents has ſpread through almoſt all the civilized parts of North-America. Nor is it confined to America. It has made its way into Europe, and has there taken poſſeſſion of the minds of ſcholars, of naturaliſts, and of philoſophers.

teſtimony of many perſons, that the bite of the rattle-ſnake has often proved mortal to the Indians, and others, notwithſtanding the boaſted ſpecificks of theſe people. Father Cajetan Cattaneo ſays, many Indians die with the bite of ſerpents. "But," obſerves the father, "it is ſaid they commonly eſcape with life, when they can quickly apply the remedy which providence has prepared of certain herbs, eſpecially the ſpikenard, which ſome parts of *Paraguay* produce in plenty. But when they are bit by the rattle-ſnake it is confidently aſſured that the caſe admits no cure." *The third letter of F. Cajetan Cattaneo.* See *A Relation of the miſſions of Paraguay, wrote originally in Italian, by Mr. Muratori*. Engliſh Tranſlation. p. 26c. London: 1759. Father Cattaneo is here ſpeaking of the South-American rattle-ſnake, the poiſon of which, I have little doubt, is more deleterious than that of the ſame animal in our part of North-America. Still, however, I am confident, that this poiſon, even in the moſt fervid climates, is not always mortal.

* Gulielmi Piſonis medici Amſtelædamenſis de Indiæ utriuſque re naturali et medica libri quatuordecim. Amſtelædami: apud Elzevirios, 1658. Some of Piſo's aſſertions concerning the rattle-ſnake are very extravagant. Such are the following: "Caudæ extremitate in anum hominis immiſſa, mortem infert confeſtim; venenum autem quod ore vel dentibus infundit, multo lentius vitam tollit." p. 275.

I think,

I think, I have ſomewhere either heard or read that the tale was credited by the late Dr. Samuel Johnſon. If I am miſtaken, I hope the admirers of this great man, ſhould any of them read my memoir, will pardon me. It is certain, notwithſtanding the vaſt ſtrength and the rich fertility of Johnſon's mind, that he was credulous and timid. Did this union of credulity and timidity ariſe out of that unhappy melancholy ("thoſe caſual eclipſes which darken learning"), that often overclouded the brightneſs of his mind*? We are told that the Hercules of Engliſh literature believed in ghoſts, and in the ſecond-ſight. The man who would thus ſuffer his mind to be eſtranged from probability, and entangled in difficulties, would, perhaps, find it eaſy to bend to the belief, that ſerpents have the faculty of faſcinating other animals.

Although I profeſs myſelf to be a warm admirer of Linnæus, and although, at a very early period of my life, I enliſted myſelf under the banner of his ſchool, I ſhall not, neverthelеſs, attempt to conceal, that this great man gave credit to the tale of the faſcination of birds and other animals by ſerpents. In his *Syſtema Naturæ* (that immortal work), under the article Crotalus horridus, or the rattle-ſnake, he has the following words: "*Aves Sciuroſque ex arboribus in fauces revocat.*"† In another work, he ſpeaks as follows. "Whoever is wounded by the Hooded Serpent (*Coluber Naja*) expires in a few minutes; nor can he eſcape with life who is bitten by the

* Or, did his melancholy grow out of his credulity and fear?

† See volume firſt, p. 372. Vienna edition of 1767. Profeſſor Gmelin, in his edition of the *Syſtema Naturæ*, when ſpeaking of the rattle-ſnake, has the following words, viz. "*aves ſciurique ex arboribus non raro in fauces inhiantis apertas incidunt,*" tom. i. pars iii. p. 1080. The ſame laborious author ſpeaking of our grey-ſquirrel (Sciurus cinereus) ſays, "a crotalo comeditur," tom. i. p. 147. This is true: but he might have ſaid the ſame when ſpeaking of the ſtriped-dormouſe, or ground-ſquirrel (Sciurus ſtriatus), of our rabbit (Lepus americanus), and many other animals.

Rattle-

Rattle-ſnake *(Crotalus horridus)* in any part near a great vein. But the merciful God has diſtinguiſhed theſe peſts by peculiar ſigns, and has created them moſt inveterate enemies; for as he has appointed cats to deſtroy mice, ſo has he provided the Ichneumon *(Viverra Ichneumon)* againſt the former ſerpent, and the Hog to perſecute the latter. He has moreover given the *Crotalus* a very ſlow motion, and has annexed a kind of rattle to its tail, by the motion of which it gives notice of its approach: but, leſt this ſlowneſs ſhould be too great a diſadvantage to the animal itſelf, he has favoured it with a certain power of faſcinating ſquirrels from high trees, and birds from the air into its throat, in the ſame manner as flies are precipitated into the jaws of the lazy toad."*

Linnæus was, certainly, extremely credulous, though I do not find that any of his profeſſed biographers have taken notice of this feature of his mind. But the proofs of my obſervation are numerous: they are to be found in almoſt every eſſay that he has written. His credulity with reſpect to the powers of medicines is, perhaps, peculiarly ſtriking †. How far this credulity, in a mind otherwiſe truly great (a mind which with reſpect to the arrangement of natural bodies has never been equalled), is to be ſought for in the general character of the country which gave Linnæus birth, I ſhall not pauſe to inquire. Yet in an inveſtigation of this kind, where the opinion of the Swediſh Pliny is neceſſarily mentioned, it might be

* See Reflections on the Study of Nature, tranſlated from the Latin of Linnæus. p. 33 & 34. Dublin edition, 1786. Dr. I. E. Smith, the ingenious tranſlator of this diſſertation, in a note to the above paſſage, has the following words. "This opinion of the faſcinating power of the Toad has been refuted, and the appearance which gave riſe to it fully accounted for, by Mr. Pennant, in his Britiſh Zoology. Probably the ſtory of the Rattle-ſnake's having a ſimilar power might be found equally falſe, if enquired into with the ſame degree of accuracy." p. 34.

† See his Materia Medica, liber. i. de Plantis, &c. Amſtelædami: 1749.

curious to look to the ſources of his credulity. The ſtudy of nature, as it reſpects this globe, is, perhaps, of all the ſciences, the moſt unfavourable to ſuperſtition, or credulity. But the greateſt of naturaliſts was one of the moſt credulous of philoſophers.

It is proper, however, to obſerve, in this place, that Linnæus by no means aſſerts, that he himſelf had ever been a witneſs to the faſcinating power of any of the ſerpent-tribe. He ſeems to have received the tale from ſome of his many pupils, whom he animated with the love of natural hiſtory. It is probable that Kalm, whom Linnæus quotes upon various occaſions, and whom he could not but eſteem, principally contributed to fix his illuſtrious maſter's credulity in this reſpect: for, in different parts of his *Travels*, this induſtrious author has given his decided aſſent to the tale; and he informs us, that he has treated of the ſame ſubject, more fully, in a treatiſe which is printed in the *Memoirs of the Royal Swediſh Academy of Sciences*, for the year 1753*.

Kalm is candid enough to tell us, that he never ſaw an inſtance of the faſcinating power of the ſerpent-kind. "However," ſays he, "I have a liſt of more than twenty perſons, among which are ſome of the moſt creditable people, who have all unanimouſly, though living far diſtant from each other, aſſerted the ſame thing †." He then goes on to tell us a long ſtory, ſimilar to that which I have related, in the beginning of this memoir, and which, therefore, it is not neceſſary to repeat, in this place.

Our author is not content to make mere mention of the fact: he undertakes to ſpeculate upon it. And here, although a talent for ingenious reaſoning is, certainly,

* Travels into North-America, &c. vol. i. p. 318 & 319.
† Ibid. vol. ii. p. 207 & 208.

not the moſt ſtriking feature that is diſplayed in the *Travels* of Kalm, he acquits himſelf, for ſome time, very judiciouſly; but ſpoils all he has ſaid, by concluding, that the bird or ſquirrel "are only *enchanted*, whilſt the ſnake has its eyes fixed on them*." He allows that "this looks odd and unaccountable, though," ſays he, "many of the worthieſt and moſt reputable people have related it, and though it is ſo univerſally believed here," that is in New-Jerſey, &c. "that to doubt it would be to expoſe one's ſelf to general laughter.†"

Several American writers have adopted the notion, that ſnakes are endued with a faſcinating faculty. Fearful that their authority may extend the empire of this error, I have been the more anxious to offer my ſentiments on the ſubject to the ſociety ‡.

It has given me pleaſure to find, that the enchanting faculty of the rattle-ſnake is doubted by ſome very reſpectable European naturaliſts. "It is difficult," ſays my excellent friend Mr. Pennant, "to ſpeak of its faſcinating powers: authors § of credit deſcribe the effects. Birds have been ſeen to drop into its mouth, ſquirrels

* Travels into North-America, &c. vol. ii. p. 210.

† Ibid.

‡ Speaking of the rattle-ſnake, my worthy friend Mr. William Bartram ſays: "They are ſuppoſed to have the power of faſcination in an eminent degree, ſo as to inthrall their prey. It is generally believed that they charm birds, rabbits, ſquirrels, and other animals, and by ſteadfaſtly looking at them, poſſeſs them with infatuation; be the cauſe what it may, the miſerable creatures undoubtedly ſtrive by every poſſible means to eſcape, but alas! their endeavours are in vain, they at laſt loſe the power of reſiſtance, and flutter or move ſlowly, but reluctantly towards the yawning jaws of their devourers, and creep into their mouths, or lay down and ſuffer themſelves to be taken and ſwallowed." Travels through North and South Carolina, Georgia, Eaſt and Weſt Florida, &c. p. 267. Philadelphia: 1791.

§ "Lawſon—Cateſby—Ph. Tr. abridg. ix. 56, &c. vii. 410.—Brickel's Hiſt. Carolina, 144.—Beverley Virginia, 260.—Colden, i. 12." Dr. Brickel is an author of no credit. His *Hiſtory of North-Carolina*, here quoted, is one of the moſt daring and ſcandalous inſtances of plagiariſm I am acquainted with.

deſcend

defcend from their trees, and leverets run into its jaws. Terror and amazement feem to lay hold on thefe little animals: they make violent efforts to get away, ftill keeping their eyes fixed on thofe of the fnake; at length, wearied with their movements, and frightened out of all capacity of knowing the courfe they ought to take, become at length the prey of the expecting devourer, probably in their laft convulfive motion." *

My friend Mr. de la Cépède, one of the moft eloquent naturalifts of the age, has devoted a good deal of attention to the fubject, in his *Hiftoire Naturelle des Serpens*, a work of extenfive and fuperior merit. I regret, however, that this ingenious author was not in poffeffion of a few facts, well known in this country, which could not have failed to conduct a mind, like his, ftrengthened by the enlarged contemplation of the objects of nature, to the fulnefs and certainty of truth. As it is, however, Mr. de la Cépède deferves our thanks for reviving, and giving a new turn to, the fpeculations of naturalifts on this fubject.

I beg leave, in this place, to quote that part of Mr. de la Cépède's work which relates to the queftion of my memoir.

Speaking of the boiquira, or rattle-fnake, my ingenious friend has the following words: "His infectious breath, which fometimes agitates the little animals he is about to feize, may alfo prevent their efcape. The Indians relate, that a rattle-fnake is often feen, curled round a tree, darting terrible glances at a fquirrel, which after expreffing its fear by its cries and its tremour, falls at the foot of the tree, where it is devoured. Mr. Vofmaër (at the Hague), who has made feveral experiments on the bite of a rattle-fnake, which he had alive, fays that

* Arctic Zoology, vol. ii. p. 338. London: 1792.

the birds and mice, which were thrown into the cage, would immediately endeavour to ſquat in a corner, and that ſoon after, as if ſeized with deadly anguiſh, they would run towards their enemy, who continually ſhook his rattles: but this effect of a mephitick and fetid breath has been ſo much exaggerated, and miſrepreſented, that it becomes miraculous.

"It has been ſaid," continues our author, "that the rattle-ſnake had a faculty of enchanting, as it were, the animal he intended to devour; that by the power of his glance, he could oblige the victim to approach by ſmall degrees, and finally to fall into his mouth; that even man could not reſiſt the magick force of his ſparkling eyes; and that under violent agitations he would expoſe himſelf to the envenomed tooth of the ſerpent, inſtead of endeavouring to eſcape. If the rattle-ſnake had been more generally known, and if his natural hiſtory had engaged more attention, other circumſtances, ſtill more extraordinary, would have been added to theſe miraculous feats; and how many fables would not have been ſubſtituted to the ſimple effect of a peſtilential breath, which, however, has by no means been either ſo frequent or ſo fatal as ſome naturaliſts have believed!

"We may preſume, with Kalm, that, for the moſt part, when a bird, a ſquirrel, or any other animal, has been ſeen precipitating itſelf from the top of a tree into the jaws of a rattle-ſnake, it had been already bitten*;

* I do not find that Kalm has adopted this ſyſtem of explanation, in his *Travels*. On the contrary, in this work, he gives ſome judicious reaſons for rejecting this mode of explanation. *Travels*, &c. vol. ii. p. 209 & 210. His memoir, in the *Swediſh Tranſactions*, I have not ſeen. Sir Hans Sloane, a long time ſince, conjectured, that the whole myſtery of the faſcinating faculty of the rattle-ſnake is this, viz. "that when ſuch animals as are the proper prey of theſe ſnakes, as ſmall quadrupeds, birds, &c. are ſurpriſed by them, they bite them, and the poiſon allows them time to run a ſmall way; or perhaps a bird to fly up into the next tree, where the ſnakes watch them, with great earneſtneſs, till they fall down, or are perfectly dead, when having licked them over with their ſpawl or ſpittle, they ſwal- low

that after eſcaping, it manifeſted, by its cries and its agitation, the violent action of the poiſon left in its blood, and diffuſed through its circulation, by the envenomed inoculation of the reptile's tooth; that, its ſtrength gradually decaying, it would fly or leap from branch to branch, till finally exhauſted it would fall before the ſerpent, who with inflamed eyes, and eager looks, would watch attentively every motion, and then dart on his prey, when it retained but a ſmall portion of life. Several obſervations related by travellers, and particularly a fact mentioned by Kalm, appear to confirm this." *

From this long quotation, it appears that Mr. de la Cépède adopts two modes, or circumſtances, for explaining the miraculous power, which has been attributed to theſe ſerpents. The explanation is, undoubtedly, in both caſes, ingenious, and entitled to notice. I ſhall examine the queſtion with that attention which it deſerves.

In the firſt place, my learned friend ſuppoſes, that the rattle-ſnake's infectious breath †, by agitating the little animals which it means to devour, may prevent their eſcape.

I do not altogether underſtand this expreſſion of an infectious breath. I do not think that we are in poſſeſſion of any facts by which it can be proved, that the breath of the rattle-ſnake is, in general, more infectious, or peſtiferous, than that of many other animals, whether of the ſame or of a different family. I know, indeed, that in ſome of the larger ſpecies of ſerpents, inhabiting South-America, and other countries, there is

low them down." *Philoſophical Tranſactions*, vol. xxxviii. no. 433. Mr. de la Cépède does not appear to have ſeen Sloane's paper.

* Hiſtoire Naturelle des Serpens, p. 409, 410 & 411. a Paris: 1789.

† His words are, " ſon haleine empeſtée, qui trouble quelquefois les petits animaux dont il veut ſe ſaiſir, peut auſſi empêcher qu'ils ne lui échappent." p. 409.

evolved

evolved in the ſtomach, during the long and tedious proceſs of digeſtion in theſe animals, a vapour, or a gas, whoſe odour is intenſely fetid. I have not, however, found that this is the caſe with the rattle-ſnake, and other North-American ſerpents, that I have examined. But my own obſervations on this head have not been very minute. I have made inquiry of ſome perſons (whoſe prejudices againſt the ſerpent-tribe are not ſo powerful as my own), who are not afraid to put the heads and necks of the black-ſnake, and other ſerpents that are deſtitute of venomous fangs, into their mouths, and have been informed, that they never perceived any diſagreeable ſmell to proceed from the breath of theſe animals. I have been preſent at the opening of a box which contained a number of living ſerpents; and although the box had been ſo cloſe as to admit but a very ſmall quantity of freſh air, although the obſervation was made in a ſmall warm room, I did not perceive any peculiarly diſagreeable effluvium to ariſe from the bodies of theſe animals. I am, moreover, informed by a member of this ſociety*, who has, for a conſiderable time, had a rattle-ſnake under his immediate care, that he has not obſerved that any diſagreeable vapour proceeds from this reptile. On the other hand, however, it is aſſerted by ſome creditable perſons of my acquaintance, that a moſt offenſive odour, ſimilar to that of fleſh, in the laſt ſtage of putrefaction, is continually emanating from every part of the rattle-ſnake, and ſome other ſpecies of ſerpents. This odour extends, under certain circumſtances, to a conſiderable diſtance from the body of the animal. Mr. William Bartram aſſures me, that he has obſerved "horſes to be ſenſible of, and greatly agitated by, it at the diſtance of forty or fifty yards from the ſnake. They ſhowed," he ſays,

* Mr. Charles Wilſon Peale.

"their

"their abhorrence, by ſnorting, winnowing, and ſtarting from the road, endeavouring to throw their riders, in order to make their eſcape."* This fact related by a man of rigid veracity, is extremely curious; and, in an eſpecial manner, deſerves the attention of thoſe writers, who, like M. de la Cépède, imagine that this fetid emanation from ſerpents is capable of affecting birds, at ſmall diſtances, with a kind of aſphyxy †. It even gives *ſome* colour of probability to the ſtory related by Metrodorus, and preſerved in the *Natural Hiſtory* of Pliny ‡.

The facts which came under the notice of Mr. Voſmaër, at the Hague, are curious, and deſerved to be mentioned. But they do not appear to me to be proofs of the exiſtence of an infectious or mephitick vapour proceeding from the mouth of the rattle-ſnake. I am not at all ſurprized that the birds and mice that were put into the cage, along with this reptile, ſhould exhibit the motions which were obſerved by the Dutch naturaliſt. When the little animals ſquatted down in a corner of the cage, they were, moſt probably, impelled by the inſtinct of fear, which is ſo powerful, and ſo extenſive, in the vaſt family of animals. When they ran towards the ſerpent, it may have been fear that actuated them.

In conducting a ſeries of experiments, it is ever a matter of importance, that the mind of the experimentaliſt ſhould be free from the dominion of prejudice and ſyſtem. Perhaps, facts are never related in all their unadulterated purity except by thoſe, who, intent upon the diſcovery of truth, keep ſyſtem at a diſtance, regardleſs of its claims. The ſtrong democracy of facts ſhould exert its wholeſome ſway. I cannot help thinking, that if Mr. Voſmaër had diſbelieved the faſcinating faculty

* MS. note communicated to me.

† Hiſtoire Naturelle des Serpens, p. 355.

‡ Lib. xxviii. cap. 14.

of

of ſerpents, the concluſions which he would have drawn from his experiments, juſt mentioned, would have been ſomewhat different. But of this I cannot be certain, and, therefore, I ſhall not avail myſelf of the ſuppoſition.

Some experiments, which have been made in this city, do not accord with thoſe of Mr. Voſmaër. The birds, which were put into the cage that contained the rattle-ſnake, flew or ran from the reptile, as though they were ſenſible of the danger to which they were expoſed. The ſnake made many attempts to catch the birds, but could ſeldom ſucceed. When a dead bird was thrown into the cage, the ſnake devoured it immediately. He ſoon caught and devoured a living mole, an animal much more ſluggiſh than the bird. A few days ſince, I had an opportunity of obſerving the following circumſtance. A ſmall bird, our ſnow-bird *, had been put into a cage containing a large rattle-ſnake. The little animal had been thus impriſoned for ſeveral hours, when I firſt ſaw it. It exhibited no ſigns of fear, but hopped about from the floor of the cage to its rooſt, and frequently flew and ſat upon the ſnake's back. Its chirp was no ways tremulous; but perfectly natural: it ate the ſeeds which were put into the cage, and by its whole actions, I think, moſt evidently demonſtrated, that its ſituation was not uneaſy.

I do not relate this latter fact with any intention to diſprove the notion, that the rattle-ſnake poſſeſſes the faculty of charming. For the obſervation was made on the ſeventeenth of laſt month, which is ſomewhat earlier than the time when our ſnakes uſually come out of their dens. The ſnake, too, which was the ſubject of the experiment, appeared to be very languid, and had not

* The Emberiza hyemalis of Linnæus.

eaten any thing for a confiderable time. We ought not therefore, to fuppofe him poffeffed of the fafcinating faculty at this period; fince, I prefume, that this faculty, did it exift at all, is fubfervient to the purpofe of procuring the reptile its food. The fact is, perhaps, valuable in another point of view. It feems to fhow, it does fhow, that the mephitick vapour proceeding from the rattle-fnake, allowing that fuch a vapour really exifts, was, in no refpect, injurious to the bird.

If the mephitick vapour of the rattle-fnake were productive of the effects attributed to it by Mr. de la Cépède, and other writers; and, efpecially, if this vapour extended its influence to animals fituated at a confiderable diftance from the reptile, the atmofphere of the rattle-fnake would often be a kind of Avernus, which many animals would avoid, and which would generally occafion the ficknefs or death of thofe that were fo unfortunate as to come within its fphere. But how different is the cafe! The abodes of the rattle-fnake are the favourite haunts of frogs, and many fpecies of birds, which often pafs the feafons of their amours and generation in clouds of mephitifm: uninjured, and undeftroyed. How often has the rattle-fnake been known to continue, for days, at the bottom of a tree, or even a fmall bufh, upon the branches of which the thrufh or the cat-bird are rearing their young! This would be a fuitable fituation for the mephitick vapour to exert its noxious influence; but, in our woods, fuch influence has never been perceived.

Birds of the eagle and the hawk kind have been feen to foar, for a confiderable time, above the fpot occupied by a rattle-fnake, and at length to dart upon the reptile, and carry it to their young. Neither the parent-bird nor its young ones, have ever been known to receive any injury from the fnake's vapour. Poffibly, it may be faid, this vapour was diſſipated, or greatly diluted, in paſſing through the air.

A mephitick, or fetid, vapour emanates from the bodies of many animals, beſides the rattle-ſnake; from the opoſſum *, and the pole-cat †, for inſtance. The vapour of theſe quadrupeds would be as likely to affect birds, &c. with aſphyxy, as that of the rattle-ſnake. And poſſibly it does. There is, certainly, one thing in favour of the ſuppoſition. The opoſſum, in particular, is noted for his cunning in catching birds.

I ſhall conclude this part of my memoir by obſerving, that the odour of the rattle-ſnake is ſaid to be agreeable to ſome perſons.

Mr. de la Cépède's ſecond mode of explanation is much more plauſible. I have already obſerved ‡, that it was the ſyſtem of Sir Hans Sloane, who affected to ground it upon experiments. It is adopted by the author of the well-written account of de la Cépède's *Natural Hiſtory of Serpents*, in the *Monthly Review* §.

Mr. de la Cépède preſumes that, " for the moſt part, when a bird, a ſquirrel, &c. has been ſeen precipitating itſelf from the top of a tree, into the jaws of a rattle-ſnake, it had been already bitten;" and that its whole conduct, ſuch as its crying, its agitation, its leaping from branch to branch, &c. are all effects induced by the violent operation of the poiſon, thrown into its body, by the reptile.

An attention to facts conſtrains me to reject this attempt towards a ſolution of the queſtion, which I am

* Didelphis Opoſſum. † Viverra Putorius.

‡ See pages 30 & 31, *note*.

§ Appendix to the ſecond volume of the Monthly Review Enlarged. p. 511.

conſidering.

confidering. I fhall arrange my chiefeft objections under two heads.

Firft. We are pretty well acquainted with the moft prominent effects produced by the poifon of the rattle-fnake, in various fpecies of animals. It muft be admitted, that there is a confiderable variety in thefe effects, and a great difference in the ftrength of thefe effects. In one animal, the poifon produces an high degree of inflammatory action in the fyftem; in another, the moft ftriking primary effect is a fomnolency, or drowfinefs. In one animal, the poifon does not produce any obvious effect upon the fyftem for many minutes; in another the effects are almoft inftantaneous *. But in almoft every inftance in which the poifon of the rattle-fnake has been fuccefsfully thrown into the body of an animal, there enfue a fet of fymptoms, very different from the actions of birds and fquirrels when under the fuppofed fafcinating influence of the ferpent-kind. It is not neceffary to detail, in this place, thefe various fymptoms, becaufe I have already done it in a paper which is printed in the third volume of the *Tranfactions* of our Society †, and becaufe thefe fymptoms cannot be unknown to the members of the Society. It will be fufficient to obferve, that two of the moft univerfal effects of the poifon of the rattle-fnake, I mean the extreme debility and the giddinefs, which commonly almoft immediately fucceed the bite, will preclude the poffibility of a fquirrel's, or a bird's, dancing from branch to branch, flying about, and running to and from the ferpent, for a confiderable time, before it becomes a prey to its enemy. Befides, the farce of fafcination is often kept up for a much longer term of

* A fmall dog that was bitten in the fide by a large rattle-fnake, reeled about, and expired, feemingly fuffocated, in two minutes. This was in the month of Auguft.

† No. xi. p. 110 & 111.

time than any ſmall animals are known to live after a ſucceſsful bite by the rattle-ſnake. But, perhaps, it may be ſaid, that the rattle-ſnake, like ſome of our waſps, knows how to inject into the animal, which he means to devour, any given quantity of his ſubtile poiſon. Here, the analogy will not apply: but I have not time to point out the various inſtances in which its failure is conſpicuous.

Kalm mentions a well-known fact, which will be admitted to have conſiderable weight in deſtroying the force of this part of Mr. de la Cépède's ſyſtem. "The ſquirrel being upon the point of running into the ſnake's mouth, the ſpectators have not been able to let it come to that pitch, but killed the ſnake, and as ſoon as it had got a mortal blow, the ſquirrel or bird deſtined for deſtruction, flew away, and left off their moanful note, as if they had broke looſe from a net. Some ſay, that if they only touched the ſnake, ſo as to draw off its attention from the ſquirrel; it went off quickly, not ſtopping till it had got to a great diſtance. "Why" continues our author, "do the ſquirrels or birds go away ſo ſuddenly and why no ſooner? If they had been poiſoned or bitten by the ſnake before, ſo as not to be able to get from the tree, and to be forced to approach the ſnake always more and more, they could however not get new ſtrength by the ſnake being killed or diverted."*

Secondly. It is a fact well known in this country, that the rattle-ſnake is not the only kind of ſerpent that is ſaid to be endued with the faculty of faſcinating birds, ſquirrels, and other animals. As far as my inquiries have extended, it does not appear to me that, in general, the rattle-ſnake is thought to have ſo large a portion of

* Travels into North-America, &c. vol. ii. p. 209 & 210. It will be eaſy to diſcover what part of Kalm's reaſoning, in the above quotation, I admit.

this

this faculty as ſome other ſpecies of ſerpents. Of this, at leaſt, I am certain, that perſons reſiding in our country-ſituations tell as many wonderful tales of the bewitching eyes of the black-ſnake, the coluber conſtrictor of Linnæus, as they do of the boiquira, or rattle-ſnake. Now let it be ſuppoſed, for a minute, that the poiſon of this latter ſerpent, when thrown into the body of a bird, a ſquirrel, &c. is capable of producing, in theſe animals, thoſe piteous cries, thoſe ſingular movements, thoſe tremulous fears, which are mentioned by Kalm, by de la Cépède, and by other writers,—in what manner are we to account for the ſimilar cries, movements, and fears, in thoſe birds which are frequently ſeen under the faſcinating influence of the black-ſnake? For we Americans all know, that the bite of the black-ſnake is perfectly innoxious. This, indeed, is alſo the caſe with the greater number of the ſpecies of ſerpents that have, hitherto, been diſcovered in the extenſive country of the United States. And yet almoſt every ſpecies of ſerpents is ſuppoſed to be endued with the power of faſcinating ſuch animals as it occaſionally devours.

Theſe facts, and this mode of reaſoning, certainly involve, in ſome difficulty, Mr. de la Cépède, and thoſe writers who eſpouſe his opinion, which I have examined, under the firſt head of my objections. An attempt is made to account for the imaginary faſcinating faculty of the ſerpent from the powerful influence of a ſubtile poiſon. But, upon inquiry, it is found, that the power of bewitching different animals is not an excluſive gift of thoſe ſerpents which nature has provided with envenomed fangs: it is a gift which as extenſively belongs to that more numerous tribe of our ſepents, whoſe bite is innocent, and whoſe creeping motion is their only poiſon*.

* If there is any impropriety in this mode of expreſſion, the impropriety has its ſource in my feelings, with reſpect to the ſerpents. Perhaps, no

Theſe objections will, I am perſuaded, be ſufficient to convince every unprejudiced reader, that the ſyſtem of explanation offered by Mr. de la Cépède is unfounded in facts; and, conſequently, that the problem ſtill remains to be ſolved, in another way.

Among the number of ingenious men who have amuſed themſelves with ſpeculations on the ſubject of this memoir, and who, rejecting the commonly received notion of the exiſtence of a faſcinating power in the rattle-ſnake, have attempted to explain the phænomenon upon other principles, it is with pleaſure I recogniſe the reſpectable Profeſſor Blumenbach, of Gottingen. This gentleman, in a late publication, ſpeaking of the rattle-ſnake, makes a few remarks on the faſcinating faculty which has been aſcribed to this reptile. Theſe remarks I ſhall tranſlate at length.

" That ſquirrels, ſmall birds, &c." ſays he, " voluntarily fall from trees into the jaws of the rattle-ſnake,

no man experiences the force and the miſeries of this prejudice in a greater degree than I do. It is the only prejudice which, I think, I have not ſtrength to ſubdue. As the natural hiſtory of the ſerpents is a very curious and intereſting part of the ſcience of zoology; as the United-States afford an ample opportunity for the farther improvement of the hiſtory of theſe animals, and as I have, for a long time, been anxious to devote a portion of my leiſure time to an inveſtigation of their phyſiology, in particular, I cannot but exceedingly regret my weakneſs and timidity, in this reſpect. I had meditated a ſeries of experiments upon the reſpiration, the digeſtion, and the generation of the ſerpents of Pennſylvania. But, I want the fortitude which it is neceſſary to poſſeſs in entering on the taſk. Inſtead of ſlowly and cautiouſly diſſecting and examining their ſtructure and their functions, with that attention which the ſubject merits, I am more diſpoſed, at preſent, to obey the injunction of the Mantuan poet, in the following beautiful lines:

———Cape ſaxa manu: cape robora, paſtor,
Tollentemque minas et ſibila colla tumentem
Dijice: jamque fuga tumidum caput abdidit alte,
Cum medii nexus, extremæque agmina caudæ
Solvuntur, tardoſque trahit ſinus ultimus orbes.
GEORG. Lib. iii. 420—424.

lying

lying under them, is certainly founded in facts: nor is this much to be wondered at, as similar phænomena have been obferved in other fpecies of ferpents, and even in toads, hawks, and in cats, all of which, to appearance, can under particular circumftances, entice other fmall animals, by mere fteadfaft looks. Here the rattles of this fnake (the rattle-fnake) are of peculiar fervice; for their hiffing noife caufes the fquirrels, whether impelled by a kind of curiofity, mifunderftanding, or dreadful fear, to follow it, as it would feem, of their own accord. At leaft," continues Mr. Blumenbach, "I know from well-informed eye witneffes, that it is one of the common practices among the younger favages to hide themfelves in the woods, and by counterfeiting the hiffing of the rattle-fnake to allure and catch the fquirrels."*

I do not intend to take up much time in examining the foregoing explanation. I fhall offer my objections to it, in as concife a manner as I can.

Firft. The faculty of fafcinating is by no means peculiar to the rattle-fnake, but is attributed as extenfively to the black-fnake, and other ferpents, which are not furnifhed with the crepitaculum, or fet of bells †, by which this ferpent is fuppofed to be enabled to ring for its prey, when it wants it.

Secondly. Some perfons, who have feen the rattle-fnake in the fuppofed act of charming, affure me that the reptile did not fhake its rattles, but kept them ftill. It is true, that Mr. Vofmäer's rattle-fnake, already mentioned, continually fhook its rattles.

Thirdly. With regard to the practice of the young favages, fpoken of by Mr. Blumenbach, I know nothing. I have inquired of Indians, and of perfons who have re-

* Handbuch der Naturgefchichte, P. 253 Gœttingen: 1791.

† Serpent à fonnette is the French name for the rattle-fnake.

fided,

ſided, for a conſiderable time, among the Indians, and they appear to be as ignorant of the circumſtance as I am myſelf. I am inclined to think that Mr. Blumenbach has been impoſed upon: or, perhaps, the following circumſtance may have given riſe to the ſtory. The young Indians put arrows, acroſs, in their mouths, and by the quivering motion of their lips upon the arrows, imitate the noiſe of young birds, thus bringing the old ones ſo near to them, that they can be readily ſhot at. In like manner, the Lanius Excubitor, or great ſhrike, hiding itſelf in a thicket, and imitating the cry of a young bird, often ſucceeds in ſeizing the old ones, which have been ſolicited, by the counterfeited noiſe, to the aſſiſtance of their young.

Ever ſince I have been accuſtomed to contemplate the objects of nature with a degree of minute attention, I have conſidered the whole ſtory of the enchanting faculty of the rattle-ſnake, and of other ſerpents, as deſtitute of a ſolid foundation. I have attentively liſtened to many ſtories, which have been related to me as proofs of the doctrine, by men whoſe veracity I could not ſuſpect. But there is a ſtubborn incredulity often attached to certain minds. In me it was ſtrong. The mere force of argument never compelled me to believe. I always ſuſpected, that there was ſome deficiency in the extent of obſervation, and the reſult of not a little attention to the ſubject has taught me, that there is but one wonder in the buſineſs;———the wonder that the ſtory ſhould ever have been believed by a man of underſtanding, and of obſervation.

In conducting my inquiries into this curious ſubject, I thought it would be proper, and even neceſſary, previouſly to my forming a decided opinion, to aſcertain the two following points, viz. firſt: what ſpecies of birds are moſt frequently obſerved to be enchanted by the ſerpents?

pents? and, fecondly, at what feafon of the year has any particular fpecies been moft commonly feen under this wonderful influence? I was induced to believe that the folution of thefe two queftions would ferve as a clue to the inveftigation of what has long been confidered as one of the moft myfterious operations in nature. I am perfuaded that I have not been miftaken. Poffibly, the credulous may not think as I do.

It is a curious circumftance in the hiftory of birds, that almoft every fpecies, in the fame country at leaft, has an almoft uniform and determinate method of building its neft, whether we confider the form of the neft, the materials of which it is conftructed, or the place in which it is fixed *. Some obfervations on this fubject are neceffarily connected with the point under inveftigation, in this memoir:—indeed, they are involved in the queftion concerning the fpecies of birds which have moft generally been obferved to be enchanted by the rattle-fnake, &c.

Some birds build their nefts on the fummits of the loftieft trees; others fufpend them, in a pendulous manner, at the extremity of a branch, or even on a leaf †, whilft others build them on the lower branches, among bufhes, and in the hollows of decayed, and other trees.

* I do not mean, by this obfervation, to affert, that birds are neceffarily impelled to conftruct their nefts of the fame materials, or to place them in the fame fituations: yet fuch is the language of fome writers on natural hiftory, and on morals, who talk of the "determinate inftinct" of animals, and who think it impoffible that "animals of the fame fpecies fhould any where differ." "The groufe in America, we are told, perch upon trees; the hare burrows in the ground; and we have, in thefe inftances, fufficient reafon to deny that the fpecies of either is the fame with thofe of a like denomination, with which we are acquainted, in Europe." Thefe are the words of a late celebrated author. See Dr. A. Fergufon's Principles of Moral and Political Science, vol. i. p. 59 & 60. quarto edition.

† See a very interefting account of the Motacilla futoria, or Taylor-bird, by my learned friend Mr. Pennant, in his Indian Zoology, pages 44, 45 & 46.

Many ſpecies, again, are content with the ground, laying their eggs, and hatching them, in the cavity of a ſtone, an excavation from the earth, among the graſs of fields and meadows, or in fields of wheat, rye, and other grains. Thus, to confine myſelf to our own country, the eagle, the vulture, the hawk, and other birds of this extenſive family, make choice of the loftieſt oaks, and other trees of our foreſts; the baltimore-oriole*, commonly called, in Pennſylvania, the hanging-bird, ſuſpends a beautiful neſt to the extremity of a branch of the Liriodendron †, or ſome other tree; the migrating thruſh ‡, called robin, is content with the lower branches; the red thruſh ||, the cat-bird §, the red-winged oriole, called the ſwamp-black-bird ¶, and many others build in the low buſhes; the wood-peckers **, the blue motacilla (blue-bird) ††, the torchepot ‡‡, and others, build in the hollows of trees, the chattering plover ||||, and the whip-poor-will §§, take advantage of a hollow place in the ground, or in a ſtone, whilſt the great lark ¶¶, the marſh-wren ***, &c. place their neſts in the graſs; and, laſtly, the partridge ††† builds in the corn-fields.

Of all theſe birds, and of a great many others, thoſe which build their neſts upon the ground, on the lower branches of trees, and on low buſhes (eſpecially on the ſides of rivers, creeks, and other waters, that are frequented by different kinds of ſerpents), have moſt frequently been obſerved to be under the enchanting faculty of the rattle-ſnake, &c. Indeed, the bewitching ſpirit of theſe ſerpents ſeems to be almoſt entirely limited to

* Oriolus Baltimore.
† Liriodendron tulipifera.
‡ Turdus migratorius.
|| Turdus rufus.
§ Muſcicapa carolinenſis.
¶ Oriolus phœniceus.
** Pici.
†† Motacilla Sialis.
‡‡ Sitta.
|||| Charadrius vociferus.
§§ Caprimulgus.
¶¶ Alauda magna.
*** Motacilla Troglodytes?
††† Tetrao virginianus.

theſe

theſe kinds of birds. Hence, we ſo frequently hear tales of the faſcination of our cat-bird, which builds its neſt in the low buſhes, on the ſides of creeks, and other waters, the moſt uſual haunts of the black-ſnake, and other ſerpents. Hence, too, upon opening the ſtomachs of ſome of our ſerpents, if we often find that they contain birds, it is almoſt entirely thoſe birds which build in the manner I have juſt mentioned.

This fact I had long remarked. It had made ſome impreſſion upon my mind before I had turned my attention to the ſubject of this memoir. Lately, when I came to take a view of the ſubject, the fact appeared to me to be of ſome conſequence. I ſhall now avail myſelf of it.

The rattle-ſnake ſeldom, if ever, climbs up trees*. He is frequently, however, found about their roots, eſpeci-

* Some reſpectable writers aſſert, that the rattle-ſnake does climb trees, and that it does it with eaſe. Mr. de la Cépède is of this opinion. After telling us that this reptile lives upon worms, frogs, and hares, this naturaliſt proceeds: "il fait auſſi ſa proie d'oiſeaux & d'écureuils; car il monte avec facilité ſur les arbres, & s'y élance avec vivacité de branche en branche, ainſi que ſur les pointes des rochers qu'il habite, & ce n'eſt que dans la plaine qu'il court avec difficulté, & qu'il eſt plus aisè d'eviter ſa pourſuite." *Hiſtoire Naturelle des Serpens*. p. 409. At the concluſion of his account of the boiquira, or crotalus horridus, the eloquent author has run into the ſame error, in the following beautiful, though rather poetical, apoſtrophe. "Tranquilles habitans de nos contrées tempérèes, que nous ſommes plus heureux, loin de ces plages où la chaleur & l'humidité règnent avec tant de force! Nous ne voyons point un Serpent funeſte infecter l'eau au milieu de laquelle il nage avec facilité; les arbres dont il parcourt les rameaux avec viteſſe; la terre dont il peuple les cavernes; les bois ſolitaires, où il exerce le même empire que le tigre dans ſes déſerts brûlans, and dont l'obſcurité livre plus ſûrement ſa proie à ſa morſure. Ne regrettons pas les beautés naturelles de ces climats plus chauds que le nôtre, leurs arbres plus touffus, leurs feuillages plus agréables, leurs fleurs plus ſuaves, plus belles: ces fleurs, ces feuillages, ces arbres cachent la demeure du Serpent à ſonnette." *Hiſtoire Naturelle des Serpens*. p. 419 & 420. I have been at ſome pains to diſcover whether the rattle-ſnake does climb up trees. The reſult of my inquiries is that it does not. Although I have had opportunities of ſeeing great numbers of rattle ſnakes in the weſtern parts of Pennſylvania, &c. particularly in the vicinity of the river Ohio, I never ſaw one of them except

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ally in wet ſituations. It is ſaid that this reptile is often ſeen, curled round a tree, darting terrible glances at a ſquirrel, which after ſome time is ſo much influenced by theſe glances, or by ſome ſubtile emanation from the body of the ſerpent, that the poor animal falls into the jaws of its enemy. This ſtory is, I believe, deſtitute of foundation, though it is related by the good Cotton Mather*. The rattle-ſnake is, indeed, ſometimes ſeen at the root of a tree, upon the lower branches of which, at the height of a few feet from the ground, a bird or ſquirrel has been ſeen exhibiting ſymptoms of fear and diſtreſs. Is this a matter of any wonder? Nature has taught different animals what animals are their enemies; and although, as will be afterwards ſhewn, the principal food of the rattle-ſnake is the great frog, yet as he occaſionally devours birds and ſquirrels, to theſe animals he muſt neceſſarily be an object of fear. When the reptile, therefore, lies at the foot of a tree, the bird or the ſquirrel will feel itſelf uneaſy. That it will ſometimes run

on the ground. The black-ſnake I have often ſeen upon trees. I ought not, however, to conceal that in the ſummer of the laſt year, a Choktah-Indian told me, that the rattle-ſnake does climb trees and buſhes, *to a ſmall height*. He ſaid, that he had once ſeen one of theſe ſnakes upon a reed. I am not very willing to deny this Indian's ſtory: yet it is oppoſed to every information I have been able to procure from perſons well acquainted with the reptile of which I am ſpeaking. However, it is not impoſſible that where trees and buſhes grow very cloſe together, the ſnake may climb them *to a very ſmall height*. Moſt ſpecies of ſerpents move in a ſpiral manner: the rattle-ſnake moves ſtraight on; and this is the reaſon why he cannot climb trees. In the quotation which I have made from Mr. de la Cépède, another miſtake is involved. He ſpeaks of the agility with which the rattle-ſnake moves. This is not, however, merely the miſtake of Mr. de la Cépède. We find it in Piſo. Speaking of this reptile, our author ſays: "In triviis juxta ac deviis locis cernitur, tam celeriter proreptans ut volare videatur, idque velocius per loca ſaxoſa, quam terreſtria." *De Indiæ utriuſque re naturali et medica*. p. 274. Now the truth is that the rattle-ſnake is one of the moſt ſluggiſh of all our ſerpents. Linnæus was well informed, when he aſſerted that Providence had given "the Crotalus a very ſlow motion." See *Reflections*, &c. quoted p. 84 of this memoir.

* Philoſophical Tranſactions of the Royal Society, No. 339.

towards

towards the ſerpent, then retire, and return again, I will not deny. But that it is irreſiſtably drawn into the jaws of the ſerpent, I do deny: becauſe it is very frequently ſeen to drive the ſerpent from its hold; becauſe the bird or ſquirrel often returns, in a few minutes, to their habitations. Sometimes the bird or ſquirrel, in attempting to drive away the ſnake, approach too near to their enemy, and are bitten, or immediately devoured. But, from what will afterwards be ſaid, it will appear that theſe inſtances are not ſo common as is generally imagined.

My inquiries concerning the ſeaſon of the year, at which any particular ſpecies of birds has been ſeen under the faſcinating power of a ſerpent, afforded me ſtill more ſatisfaction. In almoſt every inſtance, I found that the ſuppoſed faſcinating faculty of the ſerpent was exerted upon the birds at the particular ſeaſon of their laying their eggs, of their hatching, or of their rearing their young, ſtill tender, and defenceleſs. I now began to ſuſpect, that the cries and fears of birds ſuppoſed to be faſcinated originated in an endeavour to protect their neſt or young. My inquiries have convinced me that this is the caſe.

I have already obſerved, that the rattle-ſnake does not climb up trees. But the black-ſnake and ſome other ſpecies of the genus coluber do. When impelled by hunger, and incapable of ſatisfying it by the capture of animals on the ground, they begin to glide up trees or buſhes, upon which a bird has its neſt. The bird is not ignorant of the ſerpent's object. She leaves her neſt, whether it contains eggs or young ones, and endeavours to oppoſe the reptile's progreſs. In doing this, ſhe is actuated by the ſtrength of her inſtinctive attachment to her eggs, or of affection to her young. Her cry is melancholy, her motions are tremulous. She expoſes herſelf to the moſt imminent danger. Sometimes, ſhe ap-

proaches

proaches ſo near the reptile that he ſeizes her as his prey. But this is far from being univerſally the caſe. Often, ſhe compels the ſerpent to leave the tree, and then returns to her neſt*.

It is a well known fact, that among ſome ſpecies of birds, the female, at a certain period, is accuſtomed to compel the young ones to leave the neſt; that is, when the young have acquired ſo much ſtrength that they are no longer entitled to *all* her care. But they ſtill claim ſome of her care. Their flights are awkward, and ſoon broken by fatigue. They fall to the ground, where they are frequently expoſed to the attacks of the ſerpent, which attempts to devour them. In this ſituation of affairs, the mother will place herſelf upon a branch of a tree, or buſh, in the vicinity of the ſerpent. She will dart upon the ſerpent, in order to prevent the deſtruction of her young: but fear, the inſtinct of ſelf-preſervation, will compel her to retire. She leaves the ſerpent, however, but for a ſhort time, and then returns again. Oftentimes, ſhe prevents the deſtruction of her young, attacking the ſnake, with her wing, her beak, or her claws. Should the reptile ſucceed in capturing the young, the mother is expoſed to leſs danger. For, whilſt engaged in ſwallowing them, he has neither inclination nor power to ſeize upon the old one. But the appetite of the ſerpent-tribe is great: the capacity of their ſtomachs is not leſs ſo. The danger of the mother is at hand, when

* Horace, though he has not, like his contemporary, Virgil, given any great proofs of his knowledge in natural hiſtory, appears to have known, full well, the anxiety of birds for the preſervation of their young:

> " Ut aſſidens implumibus pullis avis
> " Serpentium allapſus timet."
>
> Epod. I.

The author of theſe two fine lines, had he lived in America, the land of faſcination, would, I am inclined to think, have diſbelieved the whole ſtory. They would have been a clue to light and truth on this ſubject.

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the young are devoured. The ſnake ſeizes upon her: and this is the cataſtrophe, which crowns the tale of faſcination!

An attachment to our offspring is not peculiar to the human kind alone. It is an inſtinct which pervades the univerſe of animals. It is a ſpark of the divinity that actuates the greater number of living exiſtences. It is a paſſion which, in my mind, at leaſt, declares, in language moſt emphatick, the exiſtence, the ſuperintendance, the benevolence, of a firſt great cauſe, who regards with partial and parental, if not with equal, eyes the falling of a ſparrow and the falling of an empire.

Among the greater number of the ſpecies of birds, the attachment of the parent to the young is remarkably ſtrong. We have daily inſtances of this attachment among our domeſtick birds, and I believe, it is ſtronger among theſe birds in their wild ſtate: for there are ſome reaſons for ſuſpecting, that this amiable inſtinct is diminiſhed and weakened by culture*. The inſtances which I have already mentioned, as well as a fact, which remains to be mentioned, point out, in a ſtriking view, the attachment of the mother-bird to her offspring. She often guards her neſt, with the greateſt attention, fearful of the inſidious glide of the ſerpent. She endeavours to prevent the deſtruction of her eggs or young, by this enemy. When he has ſucceeded in obtaining them, ſhe attacks him either alone, or calls other birds to her aſſiſtance. We ought not to be ſurpriſed, that ſometimes ſhe falls a victim to her affection. For it is a well known fact, that ſome ſpecies of birds will ſuffer themſelves to be taken upon their neſts, rather than relinquiſh their young, or their eggs.

* This queſtion will be examined in my memoirs upon the ſtorge, or affections, of animals.

In

In the ſtudy of natural hiſtory, I am always happy to diſcover new inſtances of the wiſdom of providence, and new proofs of the ſtrong affections of animals. And for the diſcovery of ſuch inſtances of wiſdom, and ſuch proofs of affection, the contemplation of nature is an ample field. In the inſtances now before us, the ſtrength of the inſtinct of affection in birds is illuſtrated, in a ſtriking point of view; and I cannot help obſerving, that I feel an high degree of pleaſure in being able to do away, in ſome meaſure at leaſt, a prejudice, not leſs extenſive than it is unfounded, by bearing my ſlender teſtimony in favour of the exiſtence and the powerful dominion of a benevolent principle in animals.

The following fact was communicated to me, ſome time ſince, by our preſident, Mr. Rittenhouſe. I think, it ſtrikingly illuſtrates and confirms the ſyſtem which I have been endeavouring to eſtabliſh. I relate it, therefore, with pleaſure, and the more ſo, as I have no doubt, that the authority of a cautious and enlightened philoſopher will greatly contribute to the deſtruction of a ſuperſtitious notion which diſgraces the page of natural hiſtory.

Some years ſince, this ingenious gentleman was induced to ſuppoſe, from the peculiar melancholy cry of a red-winged-maize-thief*, that a ſnake was at no great diſtance from it, and that the bird was in diſtreſs. He threw a ſtone at the place from which the cry proceeded, which had the effect of driving the bird away. The poor animal, however, immediately returned to the ſame ſpot. Mr. Rittenhouſe now went to the place where the bird alighted, and, to his great aſtoniſhment, he found it perched upon the back of a large black-ſnake,

* Commonly called, in Pennſylvania, the Swamp-Black-bird. It is the Oriolus phœniceus of Linnæus.

which it was pecking with its beak. At this very time, the ſerpent was in the act of ſwallowing a young bird, and from the enlarged ſize of the reptile's belly it was evident, that it had already ſwallowed two or three other young birds. After the ſnake was killed, the old bird flew away.

Mr. Rittenhouſe ſays that the cry and actions of this bird had been preciſely ſimilar to thoſe of a bird which is ſaid to be under the faſcinating influence of a ſerpent; and I doubt not that this very inſtance would, by many credulous perſons, have been adduced as a proof of the exiſtence of ſuch a faculty. But what can be more evident than the general explanation of this caſe? The maize-thief builds its neſt in low buſhes, the bottoms of which are the uſual haunts of the black-ſnake. The reptile found no difficulty in gliding up to the neſt, from which, moſt probably in the abſence of the mother, it had taken the young ones. Or it had ſeized the young ones, after they had been forced from the neſt, by the mother. In either caſe, the mother had come to prevent them from being devoured.

We are well acquainted with the common food of the rattle-ſnake. It is the great-frog * of our rivers, creeks, and other waters. The ſnake lies inſidiouſly in wait for his prey, at the water-edge. He employs no machinery of enchantment. He truſts to his cunning and his ſtrength.

A very ingenious † friend of mine, who has devoted conſiderable attention to the natural hiſtory of the rattle-ſnake, and who has diſſected many of them, aſſures me, that he never ſaw but one inſtance in which a bird was found in the ſtomach of this reptile, and this bird was

* Rana ocellata of Linnæus.
† Timothy Matlack, Eſquire.

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the chewink, or ground-robin *. In another inſtance, he ſaw a ground-ſquirrel † taken out of one of theſe reptiles. In every other caſe, ſo long as the food retained enough of the form to be diſtinguiſhed, the ſtomach was found to contain the great-frog, which I have mentioned.

Another argument againſt the faſcinating power of the ſerpent-tribe ſtill remains to be conſidered.

It is natural to inquire, for what purpoſe nature has endued ſerpents with the ſuppoſed powers of faſcinating birds, and other animals? The anſwer to this queſtion is uniform. It is ſaid, the power is given that the ſerpents may obtain their food. Let us examine this opinion.

Admitting the exiſtence of this power, I ſhould have no heſitation in believing, that its uſe is what is here mentioned, though, indeed, it ought not to be concealed, that ſnakes are ſuppoſed, by ſome fooliſh people, to have the power of charming even children. And yet, I believe, there are no inſtances recorded of our American ſnakes devouring children. If, then, nature, in the immenſity of her kindneſs, had gifted the ſerpents with this wonderful power, we ſhould, at leaſt, expect to find that the common and principal food of theſe ſerpents was thoſe animals, viz. birds and ſquirrels, upon which this influence is generally obſerved to be exerted. This, however, is by no means the caſe.

As connected with this part of my memoir, it will not be improper to obſerve, that all our ſerpents are the food of different kinds of birds. Even the rattle-ſnake, whoſe poiſon produces ſuch alarming ſymptoms in man, and other animals, is frequently devoured by ſome of our ſtronger and more courageous birds. As far as I can

* This is the Fringilla erythrophthalma of Linnæus.
† The Sciurus ſtriatus of Linnæus.

learn,

learn, the birds which moſt commonly attack and deſtroy this reptile, are the ſwallow-tailed hawk*, and the larger kinds of owls. The owl often feeds her young with this ſnake, whoſe bones are frequently found, in her neſt, at conſiderable heights from the ground. Even a hen has been known to leave, for a minute, her affrighted chickens, and attack, with her beak, a rattle-ſnake, the greater part of whoſe body ſhe afterwards devoured †.

The black-ſnake is a ſerpent of much more activity than the rattle-ſnake. The latter, as I have already ſaid ‡, ſeldom, if ever, climbs up trees. But the former will ſometimes aſcend the loftieſt trees, in purſuit of the object of his appetite. The rattle-ſnake, it has been juſt obſerved, ſubſiſts principally upon the large frog, which frequents the waters of our country. He has, therefore, but little occaſion for activity. But the black-ſnake, feeding more upon birds, ſtands more in need of activity. He frequently glides up the trees of the foreſt, &c. and, commonly in the abſence of the mother, devours either her eggs or her young ones. The difficulty of obtaining his prey upon the tree is ſometimes very conſiderable, as will appear from a fact which will be related immediately. Now, if this ſerpent is gifted with the faculty of faſcinating, why is he not content to continue at the bottom of the tree, and bring down his object? And if he can employ this machinery of faſcination at his pleaſure, how comes it, that he ſo ſeldom ſucceeds in capturing old birds? For it is a fact that when birds are

* Falco furcatus.

† It is commonly believed, that the rattle-ſnake is a very hardy animal: but this is not the caſe. A very ſmall ſtroke on any part of its body diſables it from running at all; and the ſlighteſt ſtroke upon the top of the head is followed by inſtant death. The ſkull-bone is remarkably thin and brittle; ſo much ſo indeed, that it is thought that a ſtroke from the wing of a thruſh or robin would be ſufficient to break it.

‡ See page 103.

found in his ftomach, they are principally young birds.

I have faid, that the black-fnake fometimes finds great difficulty in obtaining his prey upon a tree. In fupport of this affertion, I could adduce many facts. But my memoir has already exceeded the limits which I originally prefcribed to it. I fhall content myfelf, therefore, with relating a folitary fact, which ftrikingly illuftrates my pofition.

A black-fnake was feen climbing up a tree, evidently with the view of procuring the young birds in the neft of a baltimore-bird. This bird, it has been already obferved, fufpends its neft at the extremity of the branch of a tree. The branch to which the bird, of which I am fpeaking, had affixed its neft, being very flender, the ferpent found it impoffible to come at the neft by crawling along it: he, therefore, took the advantage of another branch, which hung above the neft, and twifting a fmall portion of his tail around it, he was enabled, by ftretching the remainder of his body, to reach the neft, into which he infinuated his head, and thus glutted his appetite with the young birds.

The importance of this fact, in the inveftigation of the fubject of my memoir, appears to me to be great. An American foreft is not the filent refidence of a few birds. During the greater part of the fpring and fummer months, our woods are alive with the numerous fpecies of refident and vifitant birds. At thefe times, if the black-fnake poffeffes the faculty of fafcinating, it cannot be a difficult thing for him to procure his food. Yet, in the inftance which I have juft related, we have feen this reptile climbing up a tree, and there obliged to exert all his ingenuity to obtain his prey.

I cannot well conclude this memoir without obferving, that in the inveftigation of the fubject which it involves, I have

I have experienced much pleaſure. For to the cultivators of ſcience, the diſcovery of truth muſt, at all times, be a ſource of pleaſure. This pleaſure will even riſe to ſomething like happineſs, when, in addition to the diſcovery of truth, we are enabled to draw aſide the veil, which, for ages, has curtained ſuperſtition and credulity. Under the influence of various ſpecies of ſuperſtition, we fall from our dignity, and are often rendered unhappy. It ſhould be one of the principal objects of ſcience to rear and prop the dignity of the mind, and to ſmooth its way to comforts, and to happineſs. The ills and the infirmities of our earthly ſtate of being are numerous enough. It is folly, if not vice, to increaſe them. He who ſeriouſly believes, that an hideous reptile is gifted, from the ſacred ſource of univerſal life and good, with the power of faſcinating birds, ſquirrels, and other animals, will hardly ſtop here. He may, and probably will, believe much more. He will not, perhaps, think himſelf entirely exempted from this wonderful influence. He may ſuppoſe, that the property belongs to other beings, beſides the ſerpents; and he will, perhaps, imagine that it forms a part of a more extenſive plan, the effects of which, he will aſſert, are prominent, and unequivocal, though its ways, he will confeſs, are incomprehenſible to mortal minds.

Historia naturalis NON BENE DIGESTA ABIT IN FABULAM; PRÆJUDICIA VERO ET NIMIA CREDULITAS *Veritatem*, ETSI COMINUS SATIS COGNITAM, LONGISSIME ALIQUANDO PROPELLUNT.

JACOBUS THEODORUS KLEIN.

Deſcription

Nº. XII.

Some account of an American Species of DIPUS, *or* JERBOA.
By BENJAMIN SMITH BARTON, M. D.

Read Oct. 2, 1795.

FEW of the native quadrupeds of North-America have been defcribed with fufficient accuracy. Several fpecies, which are well known to the inhabitants of the country, have not been defcribed at all, and good figures of moft of them are yet wanted.

To remedy, in fome meafure, thefe defects in the natural hiftory of an extenfive portion of the globe, I began, a few years fince, to collect materials for an hiftory of the quadrupeds, and other *mammalia*, of the United-States of America. In this difficult undertaking, I have now made confiderable progrefs; and I hope to be able to prefent to the public, in the term of four or five years, if not the full refult, at leaft a pretty comprehenfive view, of my labours in this interefting department of zoological fcience. In the meanwhile, it is my intention to prefent to the Philofophical Society, from time to time, among other communications relative to the natural hiftory of the United-States, a feries of papers on fome of thofe native American quadrupeds, which are either undefcribed, or which have been but imperfectly defcribed, by preceding naturalifts.

In the prefent communication, I propofe to give the fociety fome account of a fmall quadruped, belonging to the order *Glires*, which I difcovered, in the month of May, laft, near the river Schuylkill, a few miles from Philadelphia.

I am of opinion, with the late Mr. de Buffon, that elaborate defcriptions of animals are by no means neceffary, when we are enabled to give accurate repre-
fentations

ſentations of theſe animals. The drawing which accompanies this paper has been made with great care, all the proportions being preſerved with the moſt ſcrupulous nicety. My deſcription, therefore, ſhall not be long.

In its general habit, or appearance, the animal of which I am ſpeaking is nearly allied to the murine, or mice, tribe. But it differs from the true mice ſo eſſentially, in ſeveral particulars, that I have no heſitation in ſeparating it from them, and in arranging it with the genus DIPUS of Schreber, Gmelin, and ſome other ſyſtematic writers on natural hiſtory. Of this genus I believe it to be a new ſpecies, which I have taken the liberty to call *Dipus Americanus*.

This animal is about the ſize of the common houſemouſe. I weighed two of them. The difference in their weight was very ſmall. That of which I have given a figure, and from which the following deſcription is principally taken, weighed nine penny-weight, and twenty-two grains, ſoon after the death of the animal, and before the bowels were taken out. Like all the other ſpecies of Dipus, this is furniſhed with two *dentes primores*, or cutting teeth, in each jaw. Theſe teeth are ſharp at the points, and of a cheſnut-brown colour. The upper-jaw projects conſiderably beyond the lower. The noſtrils are open. The whiſkers are long. The ears are ſmall, ſomewhat oval, and covered. The fore-feet, or rather arms, are ſhort, and are furniſhed with four toes or fingers, the nails of which are long, and very ſharp. Beſides theſe fingers, there is a kind of minute tuberculum, in place of a thumb. This tuberculum is entirely deſtitute of a nail. The hind legs are very long, and are furniſhed with five toes, the three middle ones being long, ſlender, and nearly of an equal length. The two ſide-toes are much ſhorter. The inner toe is the ſhorteſt of the five.

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The head, the back, and the whole upper part of the body, are of a reddiſh-brown colour, ſomewhat inclining to yellow. The back is marked by a darker brown than the other parts. The whole under ſide of the body, beginning with the upper jaw, and ending at the anus, is of a cream colour; as are, likewiſe, the inſides of the fore-legs, or arms, and the inſides of the hind-legs.

A yellow ſtreak, or band, beginning near the lower part of the noſtrils, on each ſide, runs along the whole length of the head and neck, the upper and under ſide of the fore legs, from thence all along the body, terminating with the thighs, at the joint.

The tail is conſiderably longer than the body, gradually tapers from its origin, and is finely ciliated, or lightly covered with hairs, its whole length. It ends in a fine pencil of hairs. The upper ſide is of a ſlate-brown colour, the under ſide is of a yellowiſh-cream colour. It is compoſed of a great number of joints.

From the deſcription which I have given of this animal, it will appear that it is very cloſely allied to the Dipus Hudſonius of Profeſſor Zimmermann*; or the Labrador-Rat of Mr. Pennant †. With this animal it agrees in ſize, in the relative proportions of the body and the tail, in the number of toes, or fingers, on the fore legs, and in the general colour, as well as in the circumſtance of the yellow band, or line. Neither of the deſcriptions given by my two learned friends, juſt mentioned, are ſo minute as to enable me to purſue the compariſon farther. It does not appear, from Mr. Pennant's deſcription, what is the number of toes on the hind legs of his La-

* See his Geographiſche Geſchichte des Menſchen, und der allgemein verbreiteten vierfueſigen thiere, &c. ſweiter band. p. 358. Leipſig: 1780. octavo.

† See his Hiſtory of Quadrupeds, vol. 2. p. 173 & 174. The third edition. Alſo his Arctic Zoology, vol. 1. p. 153 & 154. London: 1792.

brador-Rat. It is true, this excellent zoologiſt ſays that Linnæus has deſcribed this animal under the name of Mus Longipes. If this were certain, then the animal which I have deſcribed would appear to be ſtill more nearly related to the Labrador-Rat, for the Mus longipes is ſaid to have the *pedes pentadactyli*, or feet furniſhed with five toes. But, I do not think it at all certain that the Labrador-Rat of Pennant is the Mus longipes of Linnæus. Indeed, Mr. Pennant himſelf, notwithſtanding his aſſertion juſt mentioned, has deſcribed the two animals as diſtinct ſpecies, in the laſt edition of his valuable *Hiſtory of Quadrupeds*. Speaking of the Mus longipes, which he calls Torrid Jerboa, he ſays it inhabits, according to Linnæus, the torrid zone, and is mentioned by no other writer. * How, then, can it be the Labrador-Rat, ſince Mr Pennant has both ſeen and deſcribed this latter animal?

Mr. Pennant ſays that Dr. Pallas has deſcribed the Labrador-Rat under the name of Mus longipes. I am ſorry that I have not an opportunity of examining the learned Profeſſor's figure and deſcription. Pennant, however, allows that "the *Aſiatic* animal (Mus longipes of Pallas) differed in colour from the *American*, being above of a light grey mixed with tawny, white below: theſe colours divided lengthways by a ſtripe of duſky red. The tail covered with longer and looſer hair at the end than in the other parts: the ſoles of the feet clad with hair. This I could not well obſerve, continues Mr. Pennant, in the ſpecimen from *Hudſon's Bay*, as it was preſerved in ſpirits." †

Mr. Zimmermann conſiders the Mus longipes of Pallas as a ſpecies diſtinct from his Dipus Hudſonius. He makes the former the Dipus longipes of Schreber, which

* Hiſtory of Quadrupeds, vol. 2. p. 171.
† Ibidem, vol. 2. p. 173 & 174.

is the Dipus meridianus of Gmelin. I cannot, for my part, doubt that the two animals are really diſtinct ſpecies, although they are conſiderably allied to each other.

In this place, I take an opportunity of obſerving, that much confuſion has been introduced into zoological ſcience in conſequence of the ſyſtematic or ſpeculative genius of Mr. de Buffon, and other writers, who have too frequently thought it neceſſary to refer the animals which inhabit the northern parts of the old and the new world to one and the ſame ſpecies, becauſe between thoſe animals there is a general reſemblance, and becauſe it has commonly been imagined that America muſt have received her animals from Europe and from Aſia. I do not mean to deny that America poſſeſſes *ſome* ſpecies of mammalia in common with Europe, and with Aſia, eſpecially perhaps with the latter. But I am perſuaded, from a long and attentive inveſtigation of this ſubject, that the number of mammalia that are common to the old and the new world is much ſmaller than naturaliſts have ſuppoſed, and that America poſſeſſes many ſpecies of theſe and other animals, as well as of vegetables, which ever have been peculiar to herſelf. Every thing, in my opinion, favours the idea, that with reſpect to *many* of the living exiſtences, there has been a ſeparate creation in the old and in the new world.

I ſhall now mention in what particulars, or characters, the Dipus which I have deſcribed differs from any of the known ſpecies of the ſame genus to which it bears the greateſt reſemblance.

It is hardly neceſſary to obſerve, that of the five ſpecies of Dipus enumerated and briefly deſcribed by Gmelin, in his new edition of the *Syſtema Naturæ* of Linnæus, there is but one with which our Pennſylvania animal can be confounded. This is the "Dipus meridianus: palmis ſubtetradactylis, plantis pentadactylis, cauda concolore."

"Corpus

"Corpus 4 *pollices cum 9 lineis longum, ſupra pallide fulvum aliquando ſubgriſeum, ſubtus lacteum poſtice incraſſatum;* caput *magis oblongum, quam jaculo, roſtro productiore;* auriculæ *inſignes ovales;* os pedeſque *albi; horum volæ villoſiſſimæ, anteriorum pollex vix unguiculatus; poſteriorum femora carnoſiſſima;* cauda *vix ultra 3 pollices longa, craſſa, largiter piloſa* *."

From theſe deſcriptions, it appears that the Mus longipes of Pallas is larger than the Dipus Americanus. This circumſtance is farther confirmed by Zimmermann, who ſays that the ſize of the firſt of theſe animals is between that of the rat and the field-mouſe †. The colour of the Dipus Americanus is rather of a dark than of a pale tawny, colour. Below the colour in both animals ſeems to be the ſame. As far as I can judge from the figures of the Dipus Jaculus, I ſhould think that the head of this animal is more oblong than that of the Dipus Americanus. The ears of the Dipus meridianus are ſaid to be large. Thoſe of the Dipus Americanus are much ſmaller than the ears of the Dipus Jaculus, or any other ſpecies of the genus of which I have ſeen figures. The feet of the Dipus Americanus are not white, but are of a reddiſh or fleſh colour. The ſoles of the feet of the Dipus meridianus are ſaid to be very villous; but the ſoles of the feet of the Dipus Americanus are nearly naked. In the Dipus Americanus, the *tuberculum* of the fore-feet is entirely deſtitute of a nail. The thighs of the hind legs of the Dipus meridianus are ſaid to be very thick, or fleſhy. Thoſe of the Dipus Americanus do not appear to be remarkably ſo. The tail of the Dipus meridianus is ſaid to be ſhorter than the body, The tail of the Dipus Americanus is conſiderably longer than the body.

* Syſtema Naturæ, tom. i. p. 159.
† Geographiſche Geſchichte, &c. ſweiter band, p. 357.

I have ſaid that the Dipus Americanus is cloſely allied to the Dipus Hudſonius. But theſe two animals differ from each other, in ſeveral particulars. Mr. Pennant ſays the upper lip of the firſt of theſe animals is bifid. Profeſſor Zimmermann follows him in mentioning this character. The upper lip of the Dipus Americanus is not bifid. The ears of the Dipus Hudſonius are ſaid to be large, naked, rounded. The ears of the American Dipus are very ſmall, oval, and covered. The exterior toe of the hind-leg of the Dipus Hudſonius is ſaid to be the ſhorteſt. The inner toe of the Dipus Americanus is the ſhorteſt. The tail of this laſt ſpecies terminates in a pencil of hairs. It does not appear from Mr. Pennant's account of the Labrador-Rat, that the hairs at the end of the tail of this animal are longer than thoſe on other parts of the tail. Upon the whole, I have no heſitation in concluding, that the Dipus Hudſonius and the Dipus Americanus are two diſtinct ſpecies.

It has often been ſaid, and the obſervation is a juſt one, that the moſt curious and intereſting part of zoological ſcience is that which relates to the *mores*, or manners, the inſtincts, &c. of animals. To the philoſophical naturaliſt the mere deſcription of an animal can afford very little pleaſure, or inſtruction.

I am ſorry, that, at preſent, I am not able to give much information concerning the manners of our little animal. I hope to be able to complete its hiſtory at ſome future period. Meanwhile, I ſhall mention a few circumſtances, which I have already aſcertained.

The Dipus Americanus frequents our corn-fields, our meadows, and foreſts. It eats wheat, Indian-corn, clover, and acorns. I ſuſpect it likewiſe eats cheſnuts, hickery-nuts, and other ſimilar nuts. It often gets into the granaries of the Indians ſettled at Oneida, in the ſtate of New-York, and proves very deſtructive to the

Indian-

Indian-corn. Notwithſtanding the great diſproportion in the length of its legs, it runs up trees, in the hollows of which it is often found. In ſuch trees, it lays up ſtores of Indian-corn. It moves by leaping. Its leaps are conſiderable. It often jumps at leaſt one yard and a half at a time.

I have not learned, with certainty, at what time this animal brings forth its young. But it has been ſeen leaping about with the young ones ſtrongly attached to its teats *. Four young ones have been ſeen thus attached. The Indians aſſert that this Dipus breeds very faſt.

Cats kill, but will very ſeldom eat, this animal.

I cannot ſay, with certainty, whether the Dipus Americanus belongs to that claſs of mammalia, which Pallas has named *Species Lethargicæ*, or animals which are torpid, or aſleep, during the winter ſeaſon. Gmelin ſays that all the ſpecies of Dipus hybernate †. But, perhaps, this aſſertion ought not to have been made. The torpid ſtate of animals appears to be merely an accidental circumſtance, depending principally upon climate, and partly upon the ſpecific purity of the air, in which the animal is placed. Be this, however, as it may, it is certain that many of the ſame ſpecies of animals which become torpid in one country do not become ſo in another. This fact is very obſervable in the United-States. Many ſpecies of animals which hybernate in Pennſylvania, and other more northern parts of the country, do not hyber-

* My friend Mr. John Heckewelder, in a letter to me, has communicated the following information : " There is a kind of mice, in the Weſtern-Country, of a larger ſize than our common houſe-mice, and with a ſhort tail, about an inch long, which run about with their young naked and blind, ſticking to the teats. I have caught them, and placed them in a box, where I put hay, deer's hair, &c. for a neſt, and have fed them regularly, for days together, and never could obſerve one of them at liberty from the teat, until they became of a good ſize." From the length of the tail of this animal, it cannot be the Dipus Americanus.

† " Myoxi *omnes hybernant et dipodes.*" Syſtema Naturæ, tom. i. p. 157.

nate

nate in the Carolinas, and other ſouthern parts of the continent. In the northern parts of the world, the different ſpecies of Myoxus and of Dipus may, as Gmelin aſſerts, hybernate; but it does not neceſſarily follow that they do ſo in the milder climates.

In the vicinity of Philadelphia, the Dipus Americanus is called, by ſome perſons, the *Seven-Sleepers*. This name led me to believe that our little animal paſſes the winter ſeaſon in a torpid ſtate. The ſuſpicion was ſtrengthened, if not confirmed, by the information which I have received from two different quarters. In the month of February, one of theſe animals was found, ſeemingly in a torpid-ſtate, under a ſtone, in opening a quarry. A farmer, who reſides near the city, informs me that he has often diſcovered theſe ſeven-ſleepers, at the depth of eighteen inches or two feet under ground, when he has been digging for the roots of horſe-radiſh and parſley, in the winter-time. He ſays, they were always lower than the influence of the froſt. When firſt diſcovered, they were not motionleſs, but ſtupid; and, as he expreſſed himſelf, they ſeemed very "awkward." Upon being taken out of their habitation, they always endeavoured to regain it again.

I have ſaid that the torpid ſtate of animals is merely an accidental circumſtance. This aſſertion will receive ſome confirmation from what I am now to mention.

The Dipus Americanus is pretty common at Oneida: it is frequently diſcovered in the cabbins of the Indians, ſettled at that place, but they obſerve it to be abroad the whole winter. In the woods, it is likely, it is torpid at the ſame ſeaſon.

I have taken ſome pains to aſcertain whether the little animal which I have deſcribed has been found in other parts of North-America beſides Pennſylvania. Upon ſhowing my drawing of the animal to an intelligent Indian

dian who is ſettled at Oneida, he aſſured me that the ſame animal is very common at that place. This Indian, who is a Mohegan, moreover ſaid, that in his language this Dipus is called *Wauh peh ſous*, which ſignifies *the creature that jumps or ſkips like a deer*. This Dipus, as I am informed by a Wyandot-Indian, is common at Sanduſky. The Wyandots call it *Su ge ta ah*.

I cannot learn that this animal has been ſeen to the ſouthward of Pennſylvania. But I do not doubt that it inhabits the ſouthern ſtates. The range, from north to ſouth, of moſt ſpecies of mammalia appears to me to be much more extenſive than is generally imagined. It has been obſerved by naturaliſts *, that the Glires have a very extenſive range in the old-world. I think, in the new-world it is ſtill more extenſive. Indeed, I am of opinion, that, with reſpect to the quadrupeds which are not domeſticated, the range of any given ſpecies is greater in America than in the countries of the old-world. Perhaps, it would not be a difficult matter to aſſign reaſons why this is the caſe: but that, I preſume, is not neceſſory now.

The northern parts of Aſia and an extenſive tract of North-America appear, more than any other parts of the world, to abound with ſmall animals of the order Glires. Even in Aſia, notwithſtanding the zeal and labours of Pallas, and other able naturaliſts, it is probable that many ſpecies of theſe animals remain to be diſcovered. In America, I preſume, the field for new diſcovery is much greater. In the period of one year, we have diſcovered three diſtinct ſpecies of the genus Dipus, in the vicinity of Philadelphia. A fourth ſpecies has been diſcovered in New-Hampſhire, by my friend Mr. W. D. Peck, a very intelligent American naturaliſt. This ſpecies, however, I will not take upon me to ſay is new.

* Sonini De Mancourt, &c.

In its colour, in the ſize and form of its ears, &c. it makes a nearer approach to the Mus longipes of Pallas than the Dipus Americanus does. It is thus deſcribed by Mr. Peck, in a letter to me. "Mus cauda elongata piloſa, palmis ſubpentadactylis, diſco fuſceſcente, lateribus rufeſcentibus, abdomine albo; pedibus poſticis longitudine corporis." A farther account of this and of the other ſpecies of Dipus, which I have diſcovered, I ſhall, probably, communicate to the Philoſophical Society, at ſome future period.

N°. XIII.

A Letter from Mr. JOHN HECKEWELDER, *to* Dr. BARTON, *giving ſome account of the remarkable inſtinct of a bird called the Nine-Killer.*

Bethlehem, December 18th 1795.

Read April 1, 1796. HAVING an opportunity by a friend of mine to Philadelphia, I muſt mention to you a curious fact, that came to my knowledge but yeſterday.

I went to a farm, about eleven miles and a half from this place, to view a young orchard, which had been planted, about five weeks ago, under my direction, where on viewing the trees, I found, to my great aſtoniſhment, almoſt on every one of them, one and on ſome two and three graſshoppers, ſtuck down on the ſharp thorny branches, which were not pruned when the trees were planted. I immediately called the tenant, and aſked the reaſon and his opinion of this. He was much ſurpriſed at my ignorance about the matter, and informed me, "that theſe graſshoppers were ſtuck up by a ſmall bird of prey, which the Germans called *Neun-toedter* (in Engliſh

Dipus Americanus.
American Jerboa.

Englifh, Nine-killer); that this bird had a practice of catching and fticking up nine grafshoppers a day, and that as he well knew they did not devour the grafshoppers, nor any other infects, he thought they muft do it for pleafure. I afked him for a defcription of this bird, and was perfectly fatisfied that it lived entirely on fmall animals, fuch as fmall birds, mice, &c. for I had paid attention to this bird as early as the year 1761, when, in the winter, one of the fame fpecies took a favourite little bird out of my cage at the window, from which time I have watched them more clofely, and have found them more numerous in the weftern-country than here. Not being fatisfied with what the tenant had told me refpecting the intention of the bird's doing all this (viz. for diverfion fake), and particularly obferving each and every one of thefe grafshoppers ftuck up fo regularly, and in their natural pofition as when on the ground, not one of them having its back downwards, I began to conjecture what might be the real intention which the bird had in this, and my determined opinion was, that this little bird-hawk, by inftinct, made ufe of this art, in order to decoy the fmaller birds, which feed on infects, and by thefe means have a fair opportunity of catching them. All this I communicated to my friends, on my return home, and they were not lefs aftonifhed at what I had related to them, than I had been on difcovering the fact. It being agreed that one or more gentlemen of learning and obfervation fhould more minutely examine into this matter, the proprietor of this farm, with another gentleman and myfelf, went this day out for the purpofe, and viewing the grafshoppers on a number of thefe fmall trees (fome of which we cut off, and took home), we returned to the tenant, who not only himfelf but alfo his father and fifter gave us the beft affurances, that they had, long fince, and from time to time, obferved this bird catching graſshoppers and ſticking them up in the manner already related, and that ſometimes they had obſerved, in places where this ſpecies of bird keeps, numbers of graſshoppers ſtuck up on a thorn-buſh in like manner. The Reverend Mr. V. Vleck is perfectly ſatisfied that this bird-hawk is the Lanius Canadenſis (in Bartram*), and has obligingly communicated the following account of this little bird-hawk to me: it is extracted from a German publication printed at Gœttingen, in 1778, under the title of "Natural Hiſtory for Children, by M. George Chriſtian Paff," who after giving a deſcription of the different ſpecies of this bird, concludes thus: "Why is this bird of prey called the nine-killer? Becauſe it is ſaid to have the habit of ſticking beetles or other inſects, and perhaps ſometimes nine of them in ſucceſſion, upon thorns, that they may not eſcape until he has leiſure to devour them all at once. And for the ſame reaſon, it is ſometimes called the thorn-ſticker." Now by the above account, we ſee that it is known in Europe that this ſame ſpecies of birds actually does ſtick up inſects of different kinds on thorns, &c. but it is ſuppoſed they eat them immediately after being ſtuck up. Here the caſe is quite otherwiſe. They remain ſtuck up, for we muſt ſuppoſe theſe to have been ſtuck up at leaſt ſome weeks ago, and before the hard froſts ſet in. The very birds (as we ſuppoſe) that ſtuck them up are now on the ſame ground, watching the ſmaller birds that come out to feed, and have been ſeen catching the latter but a few days ago. If it were true, that this little hawk had ſtuck them up for himſelf, how

* I do not find that Mr. Bartram has mentioned, in any part of his *Travels*, a Lanius Canadenſis. Since the date of this letter, Mr. Heckewelder has favoured me with a well-preſerved ſpecimen of the bird-hawk. It proves to be the Lanius Excubitor of Linnæus, the great-ſhrike of Mr. Pennant. B. S. B.

long

long would he be feeding on one or two hundred grafshoppers? But if it be intended to feduce the fmaller birds to feed on thefe infects, in order to have an opportunity of catching them, that number, or even one half, or lefs, may be a good bait all winter: and all of us, who have confidered thefe circumftances, are firmly of opinion, that thefe infects thus ftuck up, are to ferve as a bait, &c. through the courfe of the winter.

You will readily excufe my being fo lengthy on this fubject. The matter appeared to me of too much confequence to pafs over haftily. I fhall be glad to hear your opinion on this fubject.

I fend you a few of thefe grafshoppers, as I cut them from the trees. They being hard and dry, moft of their legs broke off in taking them home.

I am, with great refpect,
Dear Sir,
Your moft obedient and
Humble fervant,
JOHN HECKEWELDER.

N°. XIV.

An Enquiry into the Caufes of the Infalubrity of flat and Marfhy Situations; and directions for preventing or correcting the Effects thereof, by WILLIAM CURRIE.

Read Oct. 2, 1795.

THAT flat and marfhy fituations are unfavourable to health, and that intermittent and remittent fevers with bilious evacuations are particularly prevalent in fuch fituations during the feafon of Autumn in temperate climates as well as within the tropics, has been

been remarked by Phyſicians and Hiſtorians in every age.

But although they have agreed with reſpect to the fact, they have differed materially with reſpect to the cauſe of this circumſtance.

A deſire of aſcertaining the true cauſe of this inſalubrity induced me to engage in the enquiry which I am now about to ſubmit to this reſpectable ſociety, and I hope the time and attention which I have beſtowed upon a ſubject ſo intereſting to mankind, will not be deemed labour miſemployed.

The atmoſphere in ſalutary ſituations, has been demonſtrated by Mr. Lavoiſier and his colleagues, to be a compound body conſiſting of two diſtinct gaſes or aeriform fluids, the one called azote or nitrogen gas, and the other oxygen-gas or pure reſpirable air; and that in one hundred parts of the atmoſphere, the proportions of theſe gaſes are 72 of the azote and 28 of oxygene, or as three to one.

From Mr. Vanbreda's experiments, on the atmoſphere of marſhes in the autumnal ſeaſon, which he ſubjected to the common teſt of nitrous air in the eudiometer, it appears that theſe proportions were very different; there being but 14 or 15 parts of oxygene, to 84 or 85 of azote, but that the bulk was ſupplied, and the ſame weight preſerved by a certain quantity of carbonic gas or fixed air, and a ſmall portion of hydrogene and ammoniacal gaſes or aeriform fluids.

All theſe gaſes are the effects of vegetable and animal putrefaction, and muſt be derived from the ſoil, or the vegetable and animal ſubſtances connected with the ſoil.

The ſoil of marſhes is compoſed entirely of vegetable and animal ſubſtances, which have undergone the proceſs of putrefaction, and conſiſts principally of vegetable earth, carbon or charcoal and nitre, mixed with more or

leſs

leſs calcareous and argilaceous earth, and by diſtillation affords oil, hydrogen, and azote.

From this ſoil, and from the various vegetable and animal ſubſtances mixed with it, and conſtantly putrefying in hot weather, it has been ſuppoſed miaſmata iſſue, which give origin to the diſeaſes peculiar to marſhy ſituations; and as there are no ſubſtances but thoſe gaſes, already enumerated, which can be diſcovered to iſſue from a marſhy ſoil, or from putrefying vegetable or animal ſubſtances, if thoſe diſeaſes depend upon miaſmata or effluvia, theſe miaſmata muſt conſiſt of one or more of the gaſes enumerated.*

In order to determine this matter, it will be neceſſary to enquire into the effects which theſe ſubſtances, ſingly, or combined, uſually produce on the living human body.

If the carbonic gas or fixed air, when applied in a certain quantity or in a concentrated ſtate, deſtroys life inſtantly by its action on the irritability of the muſcular fibres of the heart, as from the obſervations of Meſſrs

* "In the vinous fermentation, part of the principles of the vegetable ſubſtance, viz. the hydrogen, remains united with a portion of water and of carbon to form the alkohol.

"In the acetous fermentation, a union takes place between the oxygen and the alkohol, and earthy matter is depoſited. In other words the baſe of the pure air abſorbed, uniting with the alkohol of the liquor, and the eſſential ſalts diſſolved in it, forms vinegar, while a depoſition takes place of earthy or oily matters no longer ſoluble in the liquor. Hence vinegar is in an intermediate ſtate between wine and fixed air, accordingly vinegar may be made by impregnating alkohol and water with fixed air.

"The gas of fermenting liquors which is fixed air, holding ſome ſpirit of wine in ſolution received into water, has the ſame effect.

"In the putrid fermentation (which is the only ſpecies that takes place in marſhes,) the whole of the hydrogen is diſſipated, under the form of inflammable gas, while the oxygen and the carbon uniting, with the caloric or principle of heat, eſcapes under the form of fixed air," after this proceſs, if there has been ſufficient water and heat to complete the putrefactive proceſs, nothing remains but the earth of the vegetable, mixed with a little carbon and iron. *Chaptal.*

Prieſtley,

Prieſtley, Bergman, Fontana, Cavallo, and other Philoſophers of credit, appears to be the caſe, nothing is more probable than that a leſs quantity though much weakened by diffuſion in, and mixture with, the atmoſpheric air would operate in a ſimilar manner, though in a leſs degree, and occaſion a diſeaſe of a paralytic or inſenſible kind, and not an intermittent or remittent, ſince in theſe laſt the ſenſibility and irritability are manifeſtly increaſed.

* That the hydrogen gas or inflammable air, has little or no ſhare in the generation of the diſeaſes under conſideration, is rendered evident by the experiments of Chaptal, De Roſier, and Beddos.

The former aſſures us, that he inſpired it ſeveral times, without perceiving any effect from it, and that it returned from his lungs without any alteration either in weight, bulk or quality, whereas common atmoſpheric air ſuffers a very material change by reſpiration, its pure or oxygenous portion being abracted, and the remainder rendered incapable of ſupporting flame, and unfit for reſpiration.

De Roſier not only inſpired inflammable air, but applied flame to it as he diſcharged it through his noſtrils, without receiving any injury from it. He alſo diſcharged the burning gas from his mouth through a tube, ſo that he appeared to breath flame.—No detonation took place in his mouth, becauſe he had diſcharged all the atmoſpheric air from his lungs, before he inſpired the inflammable air.

Dr. Beddos prevailed on a ſtout florid young woman, to inſpire hydrogen for two minutes, without any per-

* It appears from the experiments of Mr Lavoiſier, that hydrogen is always the reſult of decompoſed water; and that water is a compoſition of hydrogen and oxygen kept in a fluid ſtate by its union with coloric and conſiſts of 85 parts in 100 of oxygen and 15 of hydrogen.

ceptible

ceptible effect, except a flight giddiness after she had descended a flight of stairs.

No alteration is made in their properties by the mixture of carbonic with hydrogenous gas. No decomposition takes place, no caloric is set at liberty or heat rendered sensible by such union. We may therefore from what has now been stated, conclude that neither carbonic nor hydrogen gas, singly or combined is the miasma or effluvium by which the diseases in question are produced.

In consequence of the putrefaction of farinaceous plants, and all such as abound more in gluten than in the saccharine, or mucilaginous principles, as well as from the putrefaction of animal substances, an ammoniacal gas is produced, owing to the union of the hydrogen, evolved in the putrefactive fermentation, with the superabundant azote of the atmosphere.* But this gas instead of diminishing the powers of the human body, is well known to have a contrary effect, except when received into the lungs in a large quantity, and then it proves destructive from its stimulating quality, inducing a spasm on the glottis or bronchiæ. That neither the water of marshes, nor the exhalations which arise from thence, are septic or promoters of putrefaction, has been fully demonstrated by the experiments of Dr. Alexander. †

* Does the union of dead animal and vegetable substances prevent the noxious effects of each other?

† Having filled a tea cup with putrid water taken from a ditch in the meadows on the south side of Edinburgh, (which in summer contain a considerable quantity of extremely putrid stagnating water), and another cup with pure water, "I put a bit of mutton into each cup and set them together in the open air. The mutton in the pure water began to putrefy in about 36 hours. At the end of three days, that in the marsh water was quite sweet. On the 5th day it was taken out washed carefully with pure water, and found perfectly sweet. That in the pure water was now become intolerably fetid, and on that account was thrown away. The 7th day the mutton in the marsh water was washed again, and found as fresh as before. When it had lain in about six weeks, it still continued perfectly sweet, and the liquor around it of the same smell and colour as at first

But that any exhalation or other ſubſtance, ſhould act on the moving powers or ſolids of the human body ſeveral days after it has been received into the body, without making ſome material change in the condition or quality of the circulating fluids is inadmiſſible becauſe it is ſcarcely conceivable. That ſuch alteration is made in the quality of the fluids in putrid fevers is manifeſt from the contagious effects of the ſeveral excretions. But in caſes of intermittents and remittents which originate in marſhy ſituations, no ſuch evidence is afforded, for there is no authentic inſtance of theſe being contagious or communicable from one to another.

As no other exhalations or noxious matters than thoſe which have now been enumerated, can be diſcovered in the moſt unſalutary atmoſphere of marſhes; as there is no ſource from whence any other noxious ſubſtance can be introduced into the atmoſphere of ſuch ſituations, and as it is evident from the known effects of the gaſes which have been diſcovered in it, that they can not have the effect of producing the diſeaſes under conſideration either when applied ſingly or united, we certainly ought to heſitate before we adopt the doctrine heretofore taught, reſpecting marſh miaſma.

But as it is well known that a very material alteration is made in the proportions, which one of the component parts of the atmoſphere bears to the other, by certain proceſſes of nature and art, let us enquire how far the alteration which is made in the atmoſphere of marſhes, by the proceſs of putrefaction may affect the preſent queſtion.

firſt. After two months, things were exactly the ſame. The mutton was then thrown out." Alexander's Experimental Enquiry, p. 71.

From the experiments of the ſame gentleman it appears, that pieces of dead fleſh ſuſpended over the exhalations of the putrid water of marſhes, are five or ſix days longer putrefying, than thoſe ſuſpended over the exhalations of pure water. (See his 15th & 17th experiments).

Mr. Vanbreda's experiments, prove that there is lefs oxygen in the atmofphere of marfhes during autumn when the weather is dry and hot, than in more falutary fituations, and it is well known from innumerable experiments made by different philofophers, that this can only be diminifhed by combuftion, fermentation putrefaction or refpiration, or a procefs of a fimilar kind.

It is alfo a fact fully eftablifhed, that the functions of life as well as the procefs of combuftion and fermentation can only be continued by the application of oxygenous gas, and that thefe are affected in proportion to the quantity and purity of the gas applied.

It was formerly difcovered by Vefalius, and has fince been confirmed by the obfervations of Drs. Lower, Prieftley, Crawford and others, that the blood in the pulmonary veins is of as red and florid a colour as in the arteries, which is the reverfe in every other part of the fyftem. This circumftance has been demonftratively proved to be owing to the action of the oxygen, or the bafe of pure air upon the blood in the pulmonary veins.

From the experiments of the difcerning and ingenious Dr. Goodwin upon living animals, it appears, that the action of the heart cannot be continued by the reception of blood, which has not undergone this change of colour in the pulmonary veins from the application or introduction of oxygen. This fact has been fince confirmed by the experiments of Dr. Girtanner, as may be feen in his Effay on the principle and laws of irritability.

That blood impregnated with oxygen, or the bafe of pure air is the neceffary, and appropriate ftimulus for giving motion to the heart, and enabling it to carry on the circulation of the blood was rendered evident from the gradual diminution and debility of its contractions, as the colour of the blood became darker when the pure air was excluded, and from its contractions becoming ftronger as the blood recovered its florid colour from the application of pure air.

In theſe experiments, all the other functions of the body were obſerved to be proportionally affected with the heart. As its contractions diminiſhed, the power of theſe alſo declined: As the power of the heart recovered, theſe alſo recovered.

By theſe experiments, we learn that the abſtraction or excluſion of the oxygenous part of the atmoſphere, in a given ſpace is ſufficient of itſelf to deprive animals of life by withholding the cauſe of action. Hence we are authoriſed by the chaſteſt rules of induction to conclude that health and life muſt be affected more or leſs in proportion to the quantity of this vivifying principle at any time abſtracted from the atmoſphere, which more immediately ſurrounds us.

The preſence of the other component part of the atmoſphere, the baſe of the azotic gas though totally oppoſite to the oxygen with which it forms a perfect compound, and neutral ſubſtance when mixed in the proportions already mentioned, appears to have no ſhare in deſtroying life, though its name is derived from a miſtaken ſuppoſition that it had that effect; for the heart immerſed in this gas, will retain its irritability ſeveral hours, in a warm ſituation, after all ſigns of life have diſappeared in the reſt of the body. Mr. Valli's experiments on animal electricity have eſtabliſhed this fact.

Carbonic gas or fixed air, on the contrary, produces its deſtructive effects by a direct operation, for it deſtroys the nervous power and the irritability of the muſcular fibres the inſtant that it is received into the lungs, and comes in contact with the heart.

If the carbonic gas operated, as ſuggeſted by Mr. Kite, by inducing a ſpaſm of the glottis and thereby excluding the atmoſpheric air, the heart as in other caſes of ſuſpended

pended refpiration would retain its irritability for fome time; but this is not the cafe.

From the facts and obfervations which have now been ftated, I think it may be fairly concluded, that the caufes of the unwholefomenefs of low and moift fituations in the fummer and autumnal months, is not owing to any invifible miafmata or noxious effluvia, which iffue from the foil and lurk in the air, but to a very different caufe, viz. to a deficiency of the oxygenous portion of the atmofphere in fuch fituations, in confequence of vegetable and animal putrefaction, in conjunction with the exhaufting, and debilitating heat of the days, and the fedative power of the cold and damp air of the nights.

For want of the refrefhing and falutary ftimulus of pure air, all the functions of the body are performed imperfectly and languidly. The nervous fyftem in particular, becomes preternaturally fufceptible of impreffions from every change that occurs in the temperature of the furrounding atmofphere. The application of or expofure to a damper and colder ftate of the air than ufual, renders the veffels on the furface of the body powerlefs, and atonic, the brain and heart fympathife with the extreme nerves and veffels, the power of every function of the body declines, till the heart roufed by accumulating blood reacts with increafing velocity, and is relieved of the unufual burthen.

That the caufes which I have now affigned, are the true ones is rendered next to certain, from the frequent occurrence of thofe difeafes, (which have heretofore been fuppofed to depend upon the operation of fpecific miafmata), in fituations remote from marfhy ground, particularly in large and populous cities, where fedentary occupations and want of exercife, render the inhabitants delicate and infirm. I have feen numerous inftances of this kind even in the winter feafon, when no effluvia from marfhes could poſſibly exiſt, eſpecially among thoſe who had been previouſly debilitated by other diſorders. Nor is it uncommon for perſons who have recovered from intermittents in the autumn, to have frequent recurrences of the ſame diſeaſe in the winter, merely from ſitting in a damp room, or other expoſure to cold.

In perſons much reduced by the diſeaſes of autumn, it is alſo very common when attacked with the inflammatory diſeaſes of winter, for the ſyſtem to reſume its cuſtomary habits of action, and for the fever to reſemble an intermittent in the time and manner of its exacerbations, and remiſſions, and immediately after the removal of the local affection to become a regular intermittent. This is ſo generally the caſe on the eaſtern ſhore of Maryland, that the phyſicians in that country ſeldom make much uſe of the lancet in any of the diſeaſes which occur there, except in the ſpring ſeaſon. Are we not authoriſed from theſe facts to infer, that any circumſtances which occaſion a certain ſtate of debility, and irritability in the veſſels and nerves on the ſurface of the body, and in the ſenſorium at the ſame time, are prediſpoſing cauſes of the diſeaſes, we are now conſidering, and that when the ſyſtem is in this condition by whatever cauſe induced, the ſudden application of cold, terror, or any other ſuddenly debilitating power, may become the exciting or occaſional cauſe of febrile diſeaſe, in an indirect manner by repelling the blood to the heart, lungs and brain, and forcing them to react by the ſtimulous of diſtenſion?

If the diſeaſes of marſhy ſituations were produced by a ſpecific matter, they could never be produced by any other cauſe, but as they are frequently induced in ſeaſons and ſituations, where that ſuppoſed ſpecific matter or miaſma cannot poſſibly exiſt, there is nothing more clear than that they are not produced by any ſuch ſpecific matter.

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The opinion that thoſe diſeaſes, are the product of ſpecific matter generated by vegetable putrefaction, appears to be rendered groundleſs from the diſeaſe varying in its type and ſymptoms, in proportion to the extent and putridity of the ſoil, ſtate of climate, ſeaſon and weather with reſpect to heat, moiſture, &c. and alſo, in its not being contagious, the reverſe of which is the caſe with all known diſeaſes that are derived from ſpecific matter.

We are aſſured by the accurate Monro, in his account of the diſeaſes which prevailed in the military hoſpitals in Germany, in 1761 and 1762, that the intermitting fever ſeldom attacked any but thoſe whoſe ſolids had been previouſly relaxed by the preceding heat of the ſummer, except when they had been fatigued and overheated by the ſun and afterwards expoſed to the evening dews.

Dr. Lind of Windſor, ſays, ſudden expoſure to cold occaſioned either an inflammatory fever or a ſimple intermittent at Bengal, according to the prediſpoſition of the body.

The ſcurvy as well as the diſeaſes already enumerated alſo appears to derive its exiſtence from a deficiency of pure air in conjunction with a cold and moiſt atmoſphere, and a diet of ſalted fleſh meats. For it generally prevails in long voyages after a continuance of wet weather. The hatches being kept ſhut at ſuch times, prevents ventilation, in conſequence of which the oxygen becomes exhauſted.

Captain Cook in his two laſt voyages preſerved his crew from the ſcurvy by frequent ventilation, conſtant cleanlineſs, ſuitable cloathing, and ſtrict diſcipline.

Dr. Trotter aſſures us that in a ſlave ſhip of which he was ſurgeon, the ſeamen that were conſtantly on deck, and fed with the ordinary ſea diet remained free from the

the ſcurvy, while the ſlaves that lived principally on vegetables, but breathed a confined impure air fell miſerable victims to it.

The remarkable caſe of the blue boy, deſcribed by Dr. Sandifort of Leyden, furniſhes another ſtriking example of the importance of oxygen in the preſervation of health and life, as well as a confirmation of its being the cauſe of the red colour of the blood.

In this boy, whoſe ſkin was as blue as indigo, the aorta communicated with both ventricles of the heart, in conſequence of which the greateſt part of the blood was immediately propelled from the right ventricle into the aorta, ſo that very little paſſed into the pulmonary artery to be oxygenated.

An opinion equally erroneous with that which has lately prevailed reſpecting the cauſes of intermittent fevers, &c. has alſo been delivered down from age to age, reſpecting the cauſes of continued fevers of the nervous or putrid kind.

The doctrine formerly taught reſpecting theſe, was that they derived their exiſtence from the effluvia of dead and putrid animal ſubſtances: but from more recent and accurate obſervations, it appears that the contagion by which this kind of fever is produced as well as thoſe of a peſtilential nature, is always derived from the living human body in confined and unventilated ſituations, and it is probable that the effluvia thus excreted, partake of the quality of nitrogen gas, from their being rendered harmleſs by a union with oxygen or the baſe of pure air.

It appears more than probable alſo from the hiſtory of the circumſtances always preſent at the time febrile contagion is generated, that it is rendered virulent and powerful in proportion to the abſence or defect of oxygen and the degree of heat to which the living body has

has been expofed in fuch fituations. It was a concurrence of thefe circumftances which gave origin to the yellow fever which appeared in Grenada in the beginning of the year 1793, and which was afterwards imported into Philadelphia, as appears from the account publifhed by Dr. Chifholm.*

Noxious effluvia indeed frequently arife from putrid animal fubftances in confined fituations. Dr. Monro mentions a remarkable inftance of this, and fome later examples are recorded by Mr. St. John; but it does not appear from thefe cafes that thofe noxious effluvia produced any fymptoms, refembling thofe of putrid or peftilential fevers; on the contrary they acted as direct ftimulants, and occafioned inflammatory affections without being preceded by that fenfe of debility which always precedes thofe fevers that are occafioned by febrile contagion.

Having now fhewn, that the difeafes which prevail moft generally during the autumnal feafon in low and marfhy fituations, owe their origin, not to invifible exhalations or miafmata, but to the caufes which I have affigned, the prophylaxis, or the means of preventing the occurrence of thofe difeafes muft be fimple and obvious.

Thefe are to introduce and increafe the proportion of oxygenous gas in the fuperincumbent atmofphere, and to prevent its future abftraction, by cutting off or diminifhing the fources of putrefaction.

It would be a happy circumftance if the application of the means fuited to produce an amendment in a body fo large and fluctuating as the atmofphere, was as practicable as the means fuited to effect that purpofe are obvious: but unfortunately, this requires too much labour and expence to admit of extenfive application, efpecially

* Vide Chifholm's Effay on the fever of Grenada in 1793 &c.

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in a country where population and wealth do not bear a due proportion to the extent of territory.

We ought however to attempt every thing in our power to effect ſo deſirable and uſeful an event.

Chemiſtry furniſhes various articles by means of which we can generate and introduce a ſupply of oxygen into the atmoſphere, as well as alter the quality of thoſe noxious gaſes with which it is occaſionally contaminated.

Theſe however can only be employed in a very limitted and partial manner, and of courſe can only produce a limited and partial amendment.

I ſhall therefore mention only a few of the ſubſtances that may be occaſionally employed for this purpoſe.

A large portion of oxygen may be furniſhed by the decompoſition of nitre, as is demonſtrated from its maintaining the combuſtion of inflammable bodies.

If lighted charcoal, be placed in a proper expoſure to the open air it will continue to burn till the whole be reduced to aſhes.

If nitre be mixed with charcoal, and when kindled placed in a cloſe veſſel, the combuſtion will continue as well as if expoſed to the open air; whereas, without the aſſiſtance of the nitre, the charcoal would be immediately extinguiſhed in that ſituation for want of a ſupply of oxygen.

Mr. Schele by heating nitre to red heat in a retort, received into a moiſtened bladder more than fifty ounces in meaſure of oxygen gas from one ounce of nitre. A pound will therefore furniſh 800 ounces.

Nitre ground with two thirds of its weight of mintum and moiſtened with water ſo as to form a paſte, burns very rapidly and emits a conſiderable quantity of pure air.

But the grand engine, by which, the ſources that deprive the atmoſpere of its ſalutary and vivifying princi-

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ple, are to be cut off; and the great magazine, from whence a ſufficient ſupply is to be obtained, muſt be ſought for in the art of agriculture.

The ſtagnant waters may be carried off and the ſoil of marſhes rendered dry, by means of drains, deep trenches, and wells; and farther ſtagnation and putrefaction prevented, by conſuming the dead weeds, graſs, and woods, and by filling up the flats, ſinks and hollows with clay, ſand, or lime.

And the atmoſphere may be ſupplied with a profuſion of oxygen by cultivating on ſuch ſoils, graſſes and plants of vigorous growth, and eſpecially thoſe which live and flouriſh lateſt in the ſeaſon. For vegetables while living and growing, when expoſed to the rays of light conſtantly decompoſe the water they imbibe from the earth and air, and while they retain the hydrogen or baſe of inflammable air for the formation of oil, wax, honey, or reſin, they repleniſh the atmoſphere with oxygen."*

When it is impracticable to render marſhy ſituations dry, on account of their extent, they ſhould be kept conſtantly flooded by means of dams and ſluices, to prevent the effects of putrefaction, for when dead vegetable or animal ſubſtances are immerſed in water ſo as to be entirely excluded from contact with the air, putrefaction can only take place in a ſlow and imperfect manner.

But clearing the woods, plants, and herbs, from marſhy or fenny tracts without draining off the ſtagnant water at the ſame time, and deſtroying the dead herbage by fire, inſtead of rendering ſuch ſituations more healthful has been found to have a different effect, becauſe a greater extent of putreſcent ſurface is thereby expoſed to the rays of the ſun, and of courſe a greater portion of oxygen abſtracted from the atmoſphere. It is owing in

* Chaptal's Chemiſtry. Ingenhauſz's Obſervations, &c.

great meaſure, to this circumſtance, that all new countries are ſo generally fatal to the firſt ſettlers.

The ſame land after it has been cultivated a few years, eſpecially if there be ſufficient declivity to prevent the water from ſtagnating, loſes its unwholeſomeneſs, the putreſcent ſubſtances mixed with the ſoil or ſuperficial ſtratum of the ground haivng finiſhed the putrefactive proceſs by that time. In order therefore to render and preſerve marſhy countries healthful, they ſhould be preſerved dry and clean by means of the ſpade, the plow, and the rake.

When the level ſituation of a place prevents the ſtagnant water from being carried off by drains, deep wells ſhould be dug, in different places for the water to collect in, by which means a greater portion of the ſoil will be rendered dry, and leſs noxious.

To prevent ſtill farther the injurious effects of reſiding near marſhes or mill ponds; rows of ſuch trees as grow rapidly, and retain their verdure late in the ſeaſon, ſhould be planted between thoſe ſituations and the manſion, for the purpoſe of intercepting the moiſture in its progreſs, while they furniſh a conſtant ſupply of oxygen to the atmoſphere.

Lodging in the upper ſtory of a houſe has been found to preſerve health during a ſickly ſeaſon, inſtances of which are recorded by Sir John Pringle. This appears to be owing to thoſe ſituations being out of the reach of the moiſture from the ground.

N°. XV.

Deſcription of a Machine for ſaving Perſons from the upper Stories of a Houſe on fire, by NICHOLAS COLLIN, D. D. *the Inventor; with a Drawing from the Model.*

Read Nov. 4, 1791. ABCC is a trunk with a ſocket from top to bottom, and two pulleys *a*, *a* in the ſides C, C. In this the cylindrical ſhaft DE moves, ſupported on the pulleys by two ropes. Theſe are by one end tacked to the foot of the ſhaft, and by the other to the axes of the windlaſſes I, I, which ſtand below. The cranks, winding the ropes round the axes, lift the foot of the ſhaft to the pulleys, and by unwinding let it down to the bottom.

FEG is a lever, turning on its fulcrum, which is fixed in the top of the ſhaft, by the iron tire *d*. The long arm is preſſed by the frame at its end: the ſhort one is held in balance by the rope GH, which is faſtened on the baſe of the machine. As the lever deſcends with the ſhaft, the rope ſlackens on the ſhort arm; then the other, no longer balanced, deſcends round the fulcrum, and by this motion turns the ſhort one upwards tight againſt the rope. Their angular contrary movements continue thus, until the ſhaft reaches the bottom of the ſocket; and the baſket at the ſame time comes to the ground.

The baſe is a rectangular plank floor with a compact frame underneath. The trunk is fixed in it, by ſeveral bracers *c*, and other uſual faſtening, both above and below, ſtanding ſomewhat from the centre towards the rope GH, in order to counteract its raiſing that end.

The windlaſſes are firmly ſet in the floor, near the trunk, directly under the pulleys.

The wheels are low and ſtrong, placed near the corners of the baſe. They have locks, to be uſed when the machine is loading.

The baſket is breaſt-high, and wide enough for four perſons. The three iron rods keep it more ſteady than cords would; and their openings admit one perſon at a time. It hangs free from the end of the lever.

The principles of this conſtruction ariſe from the requiſite properties of this machine: It muſt be ſpeedily brought; readily fitted; practicable in confined places; want no ſupport from the wall; reach a conſiderable height, and alſo project over obſtacles on or above the ground; take down ſeveral perſons at once, and without any exertion of theirs. The two vertical pieces folded, with the lever oblique, can both be houſed and eaſily carried: when put in action, they expand gradually; and the lever is high when at full length. The baſe may be convenient, becauſe its own weight, with the trunk and the four men at the windlaſſes keep the centre of gravity pretty low; and all the preſſures bear perpendicular on the longer face. Beſides, people may ſit down on the ſides, or hold it by handſpikes, which may be kept on it, and when wanted, put into holes near the margins, ſo as to project outwards.* A level poſition being very neceſſary, a plumb rule ſhould alſo be hung on the trunk.

The cylindrical form of the ſhaft and ſocket procures an eaſy gliding motion, that leſſens the occaſional ſideway bearings. It is alſo leſs alterable by wear, and change of weather; it makes the ſhaft more ſolid, and thus requires leſs bulk in both pieces. The two quarters of the trunk which are ſlit for a communication of the ropes with the ſhaft, will yet be ſtrong enough by the ſolid piers that bear the pulleys. The other two have

* It is wider on the model than it appears under an oblique perſpective.

ſufficient

ſufficient firmneſs againſt oblique preſſures, ariſing from the action of the machine, or from the caſual inclination of the baſe. The part from the brim to the pulleys incluſively is fortified by the iron-band BB, to ſecure the ſhaft when drawn up. The height of that piece, its own weight, the preſſure on its head, and the obliquity of the ſame at the time, together with probable external accidents, will determine the width; it need perhaps not exceed a fifth of the part below.

The trunk and ſhaft require ſtiff and hard materials. The lever being made of tough wood, may be further ſtrengthened by iron plates. The ſolidity and ſhape of the fulcrum and arms muſt be carefully proportioned to their reſpective weights.

The ſhaft, when down, reaches beyond the trunk ſo much as to clear the lever of the piers. This added to the diſtance of the brim from the pulleys, is the difference between the ſhaft and their height *a a*. The altitude *a* E is therefore *a a* doubled and that ſurplus. If the ſhaft bore a greater proportion in effecting this altitude, it muſt be longer; conſequently it would, when ſtanding, hold the arm further from the ground, and render that piece, with itſelf, leſs portable; and, when up, increaſe the weight on the fulcrum, and the preſſure againſt the ſocket.

The arm EF riſes from *a* E on an angle about fifty degrees with the horizon, thus making a good projection, and a conſiderable increaſe of altitude. Its proportion to the ſhaft, and angle with it when down, are ſuch as to ſet the baſket on the ground.

Theſe angles, the ſhaft, and the ſpace *a a* (its elevation), accord in adjuſting the proportion of EG, and the poſition of the rope. When the lever deſcends, the arm EG opens, and throws out the rope, keeping it ſtretched all the while. This makes it deſcribe a circular arch.

arch. The lever having come to a horizontal poſition, the arm by the continued rotation returns towards the line *a* E, and makes the rope retrace the arch until the ſhaft is down. If the reverſed angle of the arm is then equal to *a* EG, the rope comes to the point G from whence it went, if leſs it goes beyond. In the firſt caſe EG is the ſide of a triangle which has *aa* for its baſe and two angles equal to *a* EG; in the ſecond it is leſs than this ſide. As the elevation of EF is not above 50, its complement *a*EG (40) is greater than the angle of EF with the ſhaft, which with that ſpan would hold the arm too high from the ground. It conſequently exceeds the reverſed angle which (being oppoſite) is equal with this. Therefore the ſhort arm is leſs than the ſaid ſide, but the difference is not great.—The ſituation of the ſhaft being moſt critical in the higheſt elevation, the rope ſhould then allow the lever very moderate rotation, and thus leſſen the kicking of the weight againſt the fulcrum. Its poſition will therefore be nearly perpendicular, when the deſcent begins, that the arch, immediately falling, or riſing very little, may keep down the end of EG. This poſition requires a moderate elevation of EF; for if the angle DEG is too contracted, EG will project far beyond the foot of the rope, in and about its horizontal paſſage, and thus produce a detrimental obliquity. It alſo limits the fall of EF, by regulating the reverſed angle; for the nearer the point G returns to the ſhaft, the further it goes from the rope, and the more oblique is the outward draught.

The obliquity of draught admitted in this machine, appears eaſy on the model: trials may prove a greater elevation of the lever practicable. The proportion of the ſhort arm is ſufficient while the whole weight to be wound up does not exceed that of twelve perſons; for ſo much, at leaſt, four hands at the windlaſſes are competent.

petent. If the rope is drawn by hands, this arm muſt ſtill, for the ſake of convenient carriage, be much ſhorter than the other; and accidents of miſmanagement might happen, which the fixed rope prevents: it is prudent to entruſt the machine with all the powers it can exerciſe; eſpecially as it muſt be uſed on the diſmal occaſion of nocturnal incends.

In conſiderable towns the houſes differ ſo much in height, that machines of two ſizes are neceſſary. The larger ſhould be competent for the third or fourth ſtories in the higheſt buildings, according to their dimenſions, and the practicable ſize of the three pieces; and the ſmaller for thoſe in the loweſt. As both have ſeveral degrees of elevation, they will alſo ſuit inferior ſtories reſpectively, and thus take in all the different heights. The baſket muſt aſcend ſo near beneath the window, that the perſons may, without fear, get in, and deſcend ſo low that they may get out. For the laſt the arm cannot be long enough, when great altitudes demand very high ſhafts; then a rope ladder may be fixed on the baſket, and let down when the machine ſtops; ten feet from the ground, if neceſſary. As the dimenſions of the lever and ſhaft of large machines demand great ſolidity, their weight will bear a conſiderable proportion of the whole that is wound up. One or two perſons the more in the baſket would proportionally leſs increaſe the whole. Their baſes will alſo be wide enough for two men at each of the four cranks. The ſmaller machines muſt ſupport the weight of four full-grown perſons, as the fire may prevent a ſecond going up. Their baſes are to be fitted for narrow alleys, and crouded places.

Much property might be ſaved from the fire by machines of a ſimilar conſtruction with this, having a capacious baſket, and capſtans inſtead of windlaſſes. The combination

combination of perpendicular and lateral movements may alſo be applied to many other valuable purpoſes.

Perpendicular lines being dropt from the point G to the baſe, the ſquares of theſe, and the ſquares of the diſtances of their ends from the foot of the rope, are jointly equal to the ſquare of the rope. The ſquare of EG is likewiſe equal to thoſe of its ſines. When the ſhaft is drawn up, the perpendicular height of the rope differs from the joint heights of the ſhaft and pulleys by the coſine of DEG; and when the ſhaft is down, it is equal to the height of this piece, joined to the coſine of the reverſed angle. The ſines of theſe angles are equal to the diſtances between the ends of the ſaid perpendiculars and the center of the trunks bottom. Taking the value of the rope in the two caſes, gives this equation: $aa^2 + 2\text{ſhaft.} \times aa =$

$$\left.\begin{array}{l} 2aE \times \text{cos. DE} \\ + 2fh \times \text{cos. rev. ang.} \\ + 2aH \times \text{Sin. DEG—Sin. rev. a.} \end{array}\right\} \text{EG.}$$

This will guide the choice of angles, proportion of EG, and poſition of the rope.

A Diſ-

N°. XVI.

A Disquisition on wool-bearing Animals, by Dr. JAMES ANDERSON, *of North Britain, in a letter dated 6th December* 1794.

LAST summer a Danish East-Indiaman put into Leith Roads on her return home. I went on board to see what curiosities she had. I there found a very fine sheep, which was covered with a close coat of thick short hair, very smooth and sleeked, like the coat of a well dressed horse; but the hairs rather stiffer, and thicker set on the skin; the colour a fine nut brown. This sheep I was told was bought at the island of Madagascar, and that all the sheep found at that place were of the same sort. Along with it, was another sheep brought from India at the same time, carrying a very close fleece of good wool; which clearly proves the influence of *breed* in over-ruling that of climate. I mention this last circumstance to obviate an idea that will readily occur of the influence of climate; an idea that prepossesses most men's minds, as it did my own for many years, so as to close their eyes against observing facts that fall often under their view.

This set my mind upon a more minute investigation of facts. I had, before that time, received from Russia some wool obtained from the common goat, of a softness that exceeds any thing of the wool kind I have ever seen, a small sample of a shawl made of which I send inclosed. I have since then seen some Angora goats' wool produced in Britain, which answers in every respect to the characteristics of *wool*, and not of hair. I have heard of the Angora rabbit also in Britain, but have not seen it as yet; but from the defcription, I have every reafon to be fatisfied that alfo is *wool* of a deep pile, and foft ftaple. I have likewife examined the fleeces of fome European fheep which are abundant, but which anfwer the characteriftics of *hair* rather than wool; and the refult of the whole is, as far as I at prefent fee, as follows:——

Firft—That the fheep is not neceffarily a wool-bearing animal, but that there are only certain *breeds* of it which can be thus diftinguifhed; and that, fetting afide leffer diftinctions, the different *breeds* of fheep may be arranged under the following claffes:

1ft. Thofe that carry fhort ftiff hair only, and nothing that refembles wool, or that can be employed in manufactures for the fame ufes as wool; the Madagafcar fheep, and alfo the Boucharian fheep of Pallas, which I am now fatisfied is of this fort.

2d. Thofe that carry wool properly fo called.—The fheep of this fort are diftinguifhed into a vaft variety of breeds, including moft of thofe reared in Europe and Afia. Some of thefe breeds have among their wool a quantity of a particular kind of opaque white hair, called *kemps* in England, and fome have none of it at all; and fo in various proportions.

3d. Sheep that carry long hair, that may be fhorn like wool, and may alfo be employed in coarfer fabricks in the place of wool. Though this be in fact *hair*, yet it has been in general confounded with wool, and fo denominated. Many breeds of European fheep may be referred to this clafs: As alfo the *Argali* of Afia. There feem to be two varieties of this clafs, viz. one that carries a fine kind of wool among the hair, as the Argali: the other that never has any of that fine wool among the hair; as the European fheep of this clafs.

Second—There are other animals, fome breeds of which, like the fheep, carry only clofe ftiff hair, while other

other breeds of them carry wool, or at leaſt fleeces which admit of being ſhorn like the wool of ſheep, and applied to the ſame purpoſes in arts. How many kinds of animals may be arranged under this head I know not, but the following ſeem to be undeniable.

1ſt. The dog.—1. Cloſe ſtiff ſhort haired: a variety of breeds common. 2. Long ſoft haired breeds: the Engliſh ſpaniel; Newfoundland dog, &c. 3. Woolly breeds: a dog that is by no means rare in this place; but the particular name I know not. It muſt be ſhorn every year, and yields a fleece as cloſe as that of any ſheep, and finer than many of them.

2d. The goat.—1. With ſhort ſtiff hair common. 2. With long coarſe ſhagged hair, common alſo. The goats of this ſort have in general ſome very fine wool growing among the hair. The Thibet goat, from which the Indian ſhawl wool is obtained, belongs to this claſs: I had ſome of them in my hands very lately. 3. Goat carrying a fleece of wool: the Angora goat, ſome of which I lately ſaw in Lancaſhire.

3d. The ox (*Bos* tribe).—1. Cloſe ſtiff haired kinds, common. N. B. I ſaw two days ago a bull of the *Zebu* kind, which had a very cloſe pile of exceedingſhort hair, not above half an inch, but ſo thick ſet, that it appeared to be liker a deer-ſkin than any thing elſe. This animal has been in Britain ſeveral years. A cow of the ſame ſort was brought over with him from India, which has had a bull and a cow-calf. There are alſo about a dozen calves by the ſame bull with common cows. The creature is extremely gentle, ſtrong, ſtands well on his legs, which are clean and ſinewy; is in every reſpect handſome but for the hump on his ſhoulder. The greateſt peculiarity is a deep dewlap, conſiſting of two looſe ſkins only, that can be eaſily ſeparated by the hand, like

 a bag.

a bag. The keeper ſays he can occaſionally fill this with water, for his own uſe, but I ſaw it not ſo employed. 2. Long lank ſofter hairs alſo common in this country, eſpecially among the highland cattle; ſome of theſe have manes like horſes, which I have ſeen. 3. Softer and cloſer hair more reſembling wool, but ſhorter: the Louiſiana ox, according to the beſt deſcription I can get of it. 4. Still longer, and more ſoft and ſilky, the fleece applied to various purpoſes in arts; the Sarluc, and Chittigong cow of India. 5. Longer and deeper fleece than almoſt any ſheep; the muſk ox of Hudſon's Bay. A gentleman here who lived long in that country, aſſures me the fleece is as cloſe as that of any ſheep, that in ſome pa ts it is finer and ſofter than any wool he has ſeen, and about half a yard in length: But we are not yet ſufficiently acquainted with the animal to ſpeak with greater certainty.

The camel ſeems alſo to be referable to this head. Nor is it altogether certain if the hog, and many other animals might not be included under it; but I will not puſh the matter further at preſent.

The practical inference I would draw from theſe facts is this.—If different breeds of animals preſerve in general their diſtinguiſhing peculiarities, when the breed is not contaminated by an intermixture with other breeds; of which the proofs are undeniable—and if a breed of the ſame kind of animal which carries wool can be found, which is equally good in other reſpects with another breed that carries no wool, (the ſheep of Madagaſcar ſeemed to be in no reſpect ſuperior to other wool-bearing ſheep: nor is the goat of Angora, for aught that I can learn, in the leaſt inferior in any reſpect to the common goat);—will it not be a matter of great economical concern to propagate as many of theſe wool-bearing breeds of animals

animals as poſſible, in preference to thoſe of the ſhort haired kinds? and is it not an object of great national concern to obtain as many varieties of theſe wool-bearing animals as can be got, in order to make accurate trials of their reſpective qualities, to aſcertain in what reſpects they would prove beneficial or hurtful? We ſee by experience that the natives of Madagaſcar have reared no other ſheep but the naked breed till this hour; and probably they believe either that there are no wool-bearing animals of that kind exiſting, or that they would not live in their country. We in Britain have never *till now* believed it was poſſible to have a wool-bearing goat; and even while I write ſupported by ſuch facts, where is the man who would not laugh at me as a viſionary, were I to talk of rearing wool-bearing cattle in place of the naked breeds we have hitherto propagated?

N°. XVII.

Later Communication by the Author on this Subject, with a Sample taken from the Fleece of a Sheep brought from Jamaica to England.

THIS ſheep forms a diſtinct variety altogether different from any other I have ever ſeen. The hair is a ſubſtance ſui generis, and is as different from the *kemps*, or ſtitchet hair of Europe, as from the long tough hair of the Ruſſian and other hairy breeds. The wool too is as different from other ſheep's wool as the hair; it is *finer* than any other, not excepting the Shetland breed; though I would ſuſpect it is ſcarcely ſo ſoft. This affords the moſt convincing proof that ever was given of the prevalence of *breed* above climate, and the

the abſurdity of thoſe opinions we have all heard repeated thouſands of times, of wool being converted into hair in the Weſt Indies.

From a compariſon of this with other facts I am perfectly ſatisfied that the varieties of this claſs of animals, poſſeſſing very diſtinct qualities inſeparably connected with *breed*, are much greater, and infinitely more diverſified than has hitherto been ſuſpected. The *ſoftneſs* of the Shetland wool is a peculiarity inſeparable from it, inſomuch that in the *coarſeſt* kinds of ſtockings made of this wool, which I have bought as low as four pence a pair, of a ſufficient ſize for the largeſt man, I could undertake to diſtinguiſh them at the firſt *by the feel*, for their ſuperior ſoftneſs even above the fineſt Spaniſh wool.

N°. XVIII.

An eaſy and accurate Method of adjuſting the Glaſſes of Hadley's Quadrant, on Land for the Back-Obſervation, by ROBERT PATTERSON, *in a Letter to* Dr. DAVID RITTENHOUSE, *Preſident of the Society.*

April 18th, 1794.

SIR,

Read April 18, 1794.

THERE are few if any inſtruments of modern invention, of more extenſive uſe in the meaſuring of angles, than that invented by our countryman, Mr. Godfry, but which has unjuſtly got the name of Hadley's quadrant.

I have however often regretted, that for want of ſome eaſy and accurate method of adjuſting the glaſſes for the back-obſervation, practicable on land, and applicable to the *comman octant*, this inſtrument was ſtill ſo much limited

limited in its uſe. For when an angle exceeding 90 degrees is to be meaſured, or when an altitude of the ſun, &c. exceeding 45 degrees is to be taken, by means of a reflecting horizontal ſurface, and ſuch caſes frequently occur, then we muſt either have recourſe to the back-obſervation, or to ſome other inſtrument.

In order to remedy, in ſome meaſure, this inconvenience, the ſextant, without any glaſſes for the back-obſervation, has been introduced; but even this inſtrument is incapable of meaſuring an altitude, by means of a reflecting horizontal ſurface, as above, when exceeding 60 degrees; and this will be the caſe with the meridian altitude of the ſun, in moſt parts of the United States, during four months in the year.

Various methods of adjuſting the glaſſes for the back-obſervation, have indeed been propoſed; but theſe are either very inaccurate and troubleſome, or inapplicable to the common octant, and require ſome appendage to the inſtrument, with which but very few have yet been made.

The following method of making this adjuſtment will not, I flatter myſelf, be found liable to any of the above objections.—It may be made on land, in a few minutes, at any time of the day when the ſun ſhines; requires no additional apparatus, but what any perſon may readily make for himſelf; and the adjuſtment may be relied upon as equally accurate with that for the fore-obſervation.

Deſcription of the Neceſſary Apparatus.

Take a piece of plane glaſs (a piece of looking-glaſs will do very well)—take the poliſh off one ſide of it, and cement it, with the rough ſide down, on the flat ſide of the ſegment of a wooden ball. The ball may be about three or four inches in diameter, and the piece of glaſs of about the ſame dimenſions. Or the glaſs may be cement-ed ed to a piece of board, and this board to a three or four pound ſhot, or ſmall hand-granade, when either of theſe may be conveniently had.

Next take a piece of triangular board of about four inches on the ſide, and through this cut a triangular mortice of about two inches on the ſide. Near the corners of this board let there be inſerted three ſmall nails or pieces of wire, to ſerve as feet for it to ſtand on.

Method of making the Adjuſtment, or finding the Quantity of the Index-Error.

At any time when the ſun ſhines, ſet your triangular board on a table, the cill of a window, or any other convenient ſtand expoſed to the ſun, and place the ball with the piece of glaſs, on the triangular mortice; which, touching the ball only in three points, will conſequently keep it ſteady in any poſition. Turn the ball into ſuch a poſition that the plane of the glaſs may be, as nearly as you can judge, parallel to the equator; and then incline this plane, in the direction of the meridian paſſing through the ſun, till the ſun be about 45° above it.

Now take your octant, and by the fore-obſervation, bring one of the limbs of the ſun's image, ſeen by a double reflection from the ſpecula of the inſtrument, exactly into contact with the image of the ſame limb, ſeen by a ſingle reflection from the ſurface of the glaſs plane, and read off the angle pointed out by the index. Immediately turn round your inſtrument, and bring the ſame limbs into contact by the back obſervation. If the angle now pointed out by the index be exactly the ſupplement (to 180°) of the former angle, the horizon-glaſs for the back-obſervation will be truly adjuſted, or exactly at right-angles with the horizon-glaſs for the fore-obſervation: But if theſe two be not equal, then take half their difference, which will be the correction or index-error for the back-obſervation; ſuppoſing the fore-horizon-glaſs to have been previouſly well adjuſted. This correction will be additive to all angles meaſured by the back-obſervation, when the angle pointed out by the index in the firſt of the above obſervations is greater than the ſupplement of the other, and vice verſa.

For the ſake of greater accuracy, you may repeat theſe obſervations till you have taken two, four, or ſix ſets; obſerving that if in your firſt ſet you begin with the fore-obſervation, as above directed, then in your ſecond ſet you muſt begin with the back-obſervation, and ſo on. A mean of the corrections thus obtained may be taken as the *true* correction of adjuſtment.

I ſhall conclude this paper with the following miſcellaneous remarks, relative to the ſubject.

1. If the arch of exceſs beyond 90° be but ſmall, as in ſome octants is the caſe, then it may be neceſſary to place the index one or two degrees before the 0, on the extra arch, and adjuſt the fore-horizon-glaſs to this poſition of the index. You will thus obtain a greater range for the index in adjuſting the back-horizon-glaſs.

2. When the reflecting glaſs-plane is placed in the poſition above mentioned, viz. at right angles to the plane of the meridian paſſing through the ſun, then the ſun's path in the heavens will, for ſeveral minutes, be very nearly parallel to the ſaid plane; and therefore no ſenſible error is to be apprehended from the increaſe or decreaſe of the ſun's altitude above this plane during the interval between the firſt and ſecond obſervations in the ſame ſet. But even if this ſhould be the caſe, from the glaſs plane being placed conſiderably *out* of the above poſition, yet, by conducting the obſervations as above directed, the ſmall errors ariſing from this ſource will naturally correct one another.

3. When the poliſh is taken off one ſurface of a piece of glaſs, then the image of the ſun, being reflected only

from one ſurface, will appear ſingle and diſtinct; whereas the image reflected from both ſurfaces, will, moſt frequently, appear double or indiſtinct; ariſing from the want of paralleliſm between the ſurfaces.

4. The image of the ſun ſeen by a ſingle reflection from one ſurface of the glaſs plane, and that ſeen by a double reflection from the ſpecula of the inſtrument, will both appear nearly of the ſame ſhade or degree of brightneſs; and this will ſeldom be ſo intenſe as to require any ſcreen or coloured glaſs to be interpoſed between the eye and the image.

5. In making obſervations with this inſtrument, whether for the purpoſe of adjuſting the glaſſes, or for any other purpoſe, where great accuracy is required, it is neceſſary that the point on the index-ſpeculum from which the firſt reflection is made, that on the horizon-glaſs from which the ſecond reflection is made, and the eye-hole through which the image is viewed, be all in a plane, parallel to the plane of the inſtrument. This will be effected by wrapping a piece of tape, or the like, round the index ſpeculum, leaving only a bare ſtrip of about a quarter of an inch broad, parallel to the plane of the inſtrument, and at the ſame height above it as the eye-hole, and tranſparent part of the horizon-glaſs.

6. The eye-hole is generally made too ſmall. In meaſuring angles on land, as in the practice of ſurveying, (for which purpoſe this inſtrument is much preferable to any other in common uſe,) in taking altitudes at ſea, in meaſuring the angular diſtance of the ſun and moon, but eſpecially of the moon and a ſtar; the eye has generally need of all the light that can be admitted from the object ſeen by direct viſion.—The eye-hole therefore, ſhould, I think, be nearly as large as the ordinary ſize of the pupil of the eye; nor is any inaccuracy, in this caſe, to be apprehended from the line of viſion not being parallel to the plane of the inſtrument; the eye being capable of placing

placing itſelf with great exactneſs oppoſite the *centre* of the eye-hole.

The ſame objection and remedy are applicable to the ſlits in the ſight-vanes, of the common ſurveying inſtruments.

7. Mr. Maſkyline, and others, recommend the ſun itſelf as the beſt object by which to adjuſt the fore-horizon-glaſs.—There is however conſiderable difficulty attending this mode of adjuſtment.—The ſun is too bright to be viewed directly, without a piece of coloured glaſs interpoſed between it and the eye; and even if the eye-piece be furniſhed with ſuch an appendage (which in the common octant is ſeldom the caſe) ſtill the two images will be of very different ſhades; and either the one ſeen by direct viſion will be too bright, or that ſeen by reflection will be too faint for an accurate obſervation of their coincidence or contact.—This difficulty may however be obviated in the following manner.

Every octant is furniſhed with at leaſt two coloured glaſſes, of different ſhades—take the darkeſt of theſe out of its frame, and with a thread faſten it on *behind* the horizon-glaſs, and turn down the other, between this and the index-ſpeculum: The two images of the ſun will thus be generally of the ſame ſhade, or nearly ſo, and the adjuſtment may then be made with the utmoſt eaſe and ſafety to the eye. Or, which is perhaps better.—Place your eye behind the fore-horizon-glaſs, and, looking through this towards the centre of the index ſpeculum, hold the inſtrument in ſuch a poſition as that the line of viſion may be directed to any point in the heavens, &c. at the angular diſtance of 90 degrees from the ſun; and then, the index being placed at 0, two images of the ſun will appear, both by a ſingle reflection, one from the index-ſpeculum, and the other from the back part of the fore-horizon-glaſs; and by bringing theſe images into coincidence or contact, as when you look directly at the ſun, the adjuſtment may be made, or the index-error found, with great eaſe and exactneſs. If the images of the ſun ſhould be too bright for the eye, one of the coloured glaſſes belonging to the inſtrument may be held before the eye. The two images of the ſun, thus viewed, will appear nearly of the ſame ſhade, ſince the one from the index-ſpeculum, which would otherwiſe appear the brighteſt, will loſe part of its light by paſſing through the horizon-glaſs.

After all, I am of opinion that this adjuſtment may be made with equal accuracy, and much more eaſe, by any well defined object on land, as the edge of a chimney, the roof of a houſe, or the like, at a ſufficient diſtance.

Any one may ſatisfy himſelf that this is the caſe, by repeatedly meaſuring the error of adjuſtment in the common way, viz. by moving the index till he produces an apparent coincidence between the object ſeen directly and by reflection. If a chimney, or the like, be the object viewed, he will ſcarce ever find any of theſe errors to differ from the mean error more than *one minute;* and the difference of ſuch errors, when the ſun is uſed, will not be leſs, but generally greater.

If the diſtance between the index-ſpeculum and the line of direct viſion (viz. a line joining the eye-hole and horizon-glaſs) ſhould not exceed three inches, which it ſeldom does, then the parallax of the inſtrument will not amount to a quarter of a minute, and may therefore be ſafely neglected; provided the object viewed be at the diſtance of two thirds of a mile. If a ſuitable object at ſuch a diſtance cannot be readily found, then you may take one at any given diſtance, and compute the parallax to be allowed for that diſtance, thus—Multiply the conſtant number 95 (the nat. tang. of 1′ to rad. $\frac{1}{10}$) by the diſtance, in inches, of the centre of the index-ſpeculum above the line of direct viſion, and dividing the product

product by the diſtance of the object in yards, the quotient will be the parallax of the inſtrument, or correction of adjuſtment, for that diſtance in minutes. For example, if the height of the index-ſpeculum above the line of viſion be three inches, and the diſtance of the object 150 yards: then $\frac{95 \times 3}{150} = 1'.9$ will be the error of adjuſtment. If therefore you place the index ſo much behind the o, on the limb of the inſtrument and then adjuſt the horizon-glaſs by an object at the above diſtance, the adjuſtment will be true for an object conſidered as at an infinite diſtance.

From the above rule it is obvious that much exactneſs in meaſuring or eſtimating the diſtance of the object you adjuſt by is not neceſſary, provided that diſtance be not very ſmall; for, in the above example, an error in the diſtance even of 10 yards would have produced an error of no more than about $\frac{1}{10}$ of a minute in computing the parallax of the inſtrument.

8. In meaſuring angles by this inſtrument, when the object ſeen by direct viſion is at no great diſtance, the parallax of the inſtrument muſt be taken into conſideration. In ſuch caſes it is commonly recommended, previouſly to adjuſt the horizon-glaſs by that object; but this, in the back-obſervation, would be attended with very great difficulty—it will therefore be beſt always to keep the back-horizon-glaſs at the ſame adjuſtment, and make the neceſſary correction, as above directed, for the diſtance of the object.

I am, Sir, with much eſteem

Yours, &c.

ROBERT PATTERSON.

An

N°. XIX.

An Eſſay tending to improve intelligible Signals, and to diſcover an Univerſal Language. From an anonymous Correſpondent in France, (probably the Inventor of the Telegraph) tranſlated from the French.

POSSUNT QUIA POSSE VIDENTUR, *Virgil.*

Read June 20, 1788.

ALL the delights, and conveniences of life originate in the mutual aids which men render to each other; but theſe aids require, or in a great meaſure depend on the mutual communication of their ideas. Every thing therefore which may contribute to extend and facilitate ſuch communication, will doubtleſs be conſidered as uſeful, and meet with a favourable reception from the ſociety; and this is the object of the preſent eſſay.

A Natural Square.

11	21	31	41	51	61	71
12	22	32	42	52	62	72
13	23	33	43	53	63	73
14	24	34	44	54	64	74
15	25	35	45	55	65	75
16	26	36	46	56	66	76
17	27	37	47	57	67	77

The

The foregoing ſquare forms the baſis of the following table, at the foot of which will be found the method of uſing it. This little eſſay is only intended to illuſtrate it by ſome examples.

Suppoſe I would tranſmit, by ſignals, the following phraſe, to my correſpondent.

Il ne devroit pas y avoir pour les lettres d'autre poſte que l'aerienne.

1. I look at the table, which is divided into two parts, and ſearch for the ſyllables which in both parts are placed in alphabetical order.

2. I find *il* in the 5th column. I begin then by indicating the fifth column, and place the figure 5 by itſelf above the place for the units which are to follow. I then place under it the figures, correſponding to the ſyllable *il* inthe table 75, making $\frac{5}{75}$ *il.*

3. In the ſame manner I find the ſyllable *ne* in the 3d column againſt 16, $\frac{3}{16}$ *n*, and ſo on till I get through the whole phraſe. See A in the examples at the end of this eſſay.

4. I keep a note of my letter, either to correct any error that might eſcape attention, or to make the table the more familiar, and to enable me to write currently in figures without looking at it.

5. I only ſend my numbers to the aerial poſt as they are placed in column B in the examples.

6. The director of the poſt will only have to tranſlate theſe numbers into longs (—), and briefs (◡), as in the column C, and to deliver this tranſlation to the operator.

7. The operator need only know how to expreſs, and diſtinguiſh the longs and briefs by his inſtrument and to wait the anſwer to one ſignal, before he makes a ſecond.—So much for the outſet.

At

At the next ſtation the operator muſt exactly anſwer every ſignal by repeating it, in caſe there are more ſtations than one; If the next ſtation be the laſt, one ſignal will do, after having written the longs (—), and briefs (ᴗ), as in column C.

2. The director of the correſponding poſt will tranſlate theſe ſigns back again into numbers, and ſend them to their addreſs in the form of column B.

3. The correſpondent will conſult his table and join the ſyllables to the numbers received, ſupplying thoſe that by agreement may be ſuppreſſed or abridged, ſee column A.

I cannot avoid obſerving that in ſpite of my endeavours the number of ſignals (61) exceeds the number of letters in the phraſe (57), by 4, but it will be ſeen that I have left a number of figures without correſponding letters, which might have aſſiſted me; and in this phraſe I have not been favoured by the chance of finding many ſyllables in the ſame column, which would have ſaved the repetition of column numbers. Beſides this, I preſume the conſtraint I have laid upon myſelf, by uſing only four ſtrokes of one inſtrument for facilitating and inſuring the ſucceſs of the enterprize, will merit ſome indulgence.

Be this as it may, I imagine that in the courſe of practice the number of ſignals may be diminiſhed, either by reducing the number of columns in the table, or by placing ſeveral ſyllables to the ſame figure, the ſenſe of which may be determined by the choice of them. Without having recourſe to all theſe expedients, let us try another example to obtain the ſame end.

Quelle plus etroite obligation que celle de nous ſecourir mutuellement?

I begin by ſuppreſſing all letters, and even words that may be readily ſupplied, and reduce the phraſe to this—

2 *Que*

Qué pu etoi obigaiſon que nou ſcouri. See column D in the examples.

On counting the number of the letters and of the ſignals, it will be ſeen that the proportion is as 61 to 33 in favour of the ſignals. The frequent changing of the column number might be avoided by reverſing ſome ſyllables, as un *noto* de vin for un *tono* de vin.

To give an idea of the degree of perfection which may, by practice, be given to our table; let any one number (ſuppoſe 76 of the 7th column) be fixed upon to deſignate "*the ſum of*", "*the number of*", "*the burden of*", &c. and by agreeing that after theſe, all ſucceeding figures of that column, (which would otherwiſe expreſs ſyllables) ſhall only expreſs numbers until a new column ſignal be made, it would be eaſy to deſignate divers numbers from 1 to 400,000 by making the 9 firſt numbers units, and the 9 following decimals.

Example. *Les ennemis m'ont pris un navire de* 400 *tonneaux et de* 35 *hommes d'equipage, dans lequel j'avois cent mille ecus en eſpeces.* See column E of the examples.

Further, if a book were made, in which, the longeſt words in our language, every member of the phraſes moſt in uſe in different profeſſions, and even whole phraſes for caſes that might be foreſeen, were to be numerically arranged from 1 to 400,000, by referring to any number, as in the laſt example, the whole phraſe attached to ſuch number might be communicated firſt uſing one number, (75 for inſtance), to ſignify "*look at your book of numbers.*"

Ex. Suppoſe I have occaſion for this phraſe from the book No. 2. *Je vous enverrai ce que vous me demandez par la meſſagerie le*——

Note. The firſt number following ſhould indicate the day of the month, the ſecond, the month itſelf, the third, the year. Then finding the phraſe in the book at No. 2. I fill up the blanks as in column F of the examples. It will be there ſeen that the firſt 31 numbers of the table may ſignify the days of the month, the firſt 12 the 12 months of the year, and ſuppoſing the firſt number to indicate the preſent year, the others may follow in courſe, either paſt or future, as may be, indicated by another figure. It appears therefore that our table may be made to indicate many different things without the leaſt confuſion.

If it be deſired to uſe any other article of the book, 75 may be added to the numbers uſed in the preceding example, which would refer to another column, if immediately afterwards you would expreſs yourſelf in detached ſyllables.

Another Example. Suppoſe I have occaſion to uſe this phraſe from the book No. 4. *Je partirai pous vous aller voir.* N. B. The firſt following number will indicate the day of the week, the next the hour, &c. which phraſe I ſhall find with the note annexed at No. 4. in that book. I may then write as in example G.

It may be alſo obſerved that the 7 firſt numbers of the table will indicate the 7 days of the week, the firſt 24 the hours from midnight to midnight. For inſtance to indicate 2. P. M. I would uſe No. 27. which would deſignate the 14th hour.

This mode of writing may appear tedious, but beſides the conſideration that practice facilitates every thing, your correſpondent may have read half of your letter before you have had time to finiſh it, in caſe you write on detached leaves, and tranſmit as faſt as they are filled. Your correſpondent may even read your letter at an earlier moment than it is begun; for if the ſignals be ſent from eaſt to weſt their communication may outſtrip the velocity of the diurnal rotation of the globe.

It is needleſs to add that by changing the order of the ſyllables, the communication may be kept ſecret from all, except the perſon who has notice of ſuch change.

A Magic

A Magic Square.

74	12	13	17	73	72	47
21	64	52	26	32	46	67
31	23	54	33	45	65	57
77	66	35	44	53	22	11
37	25	43	55	34	63	51
27	42	36	62	56	24	61
41	76	75	71	15	16	14

It now only remains to diſcover what can make an impreſſion on our ſenſes, at the greateſt diſtance, and at the leaſt expence. I would here remark that great advantage might be made of the obſervations of Dr. Franklin, relative to the rapid tranſmiſſion of ſound under water.

Archimedes ſaid, Give me a fulcrum without the globe and I will raiſe a power that ſhall remove it from its orbit; and I ſay—Place a correſpondent in Saturn, with the power of producing and ſuppreſſing at will, any one appearance, and I will obtain an exact account of what paſſes in that planet.

Before I quit this ſubject of ſignals, it will be ſeen that they contain the very eſſence of an univerſal language, ſo long deſired by all commercial nations. It may however be obſerved that it is not ſo much a new language that is ſought after, as the means of correſponding with every one in his own; and for this purpoſe numbers appear to me, the moſt proper *medium.* It need not then be contended which language is the moſt univerſal, nor need there be any college for the ſtudy of languages. One plant named differently by fifty thouſand different nations, may be found under the ſame number in the Dictionary of each nation. This language would be the cleareſt and leaſt equivocal of all others, and ſhould mankind be able univerſally to underſtand each other, they would doubtleſs be diſpoſed to promote each other's happineſs.

I believe all Europe makes uſe of the ſame figures, and at any rate they would be ſooner learnt than a language.

I will extend theſe ideas if they are found to be new and uſeful, which, according to Mr. Voltaire, is the only excuſe an author can have for making a book.

N. B. This little Eſſay made this year 1786, is only an extract from a more ample work begun in 1781.

Examples referred to in the foregoing Eſſay.

A	B	C	D	E
5.	5.	— ∪ ∪ ∪	1.	6.
75. il	75.	∪ ∪ ∪	32. qué	22. les
		— ∪ ∪		
3.	3.	— ∪ ∪ —	3.	1.
16. n	16.	—	35. pu	66. enne
		∪ ∪ —		
1.	1.	— — — —	5.	2.
56. d	56.	— ∪ ∪	47. etoi	73. mi
		∪ ∪ —		
4.	4.	∪ ∪ — —	3.	6.
46. y	46.	∪ ∪	21. o	33. mon.
		∪ ∪ —		
1.	1.	— — — —	1.	3.
15. ai	15.	—	23. bi	33. pi
		— ∪ ∪		
3.	3.	— ∪ ∪ —	2.	4.
31. pa	31.	— — —	21. ga	34. un
		—		
2.	2.	— — ∪ ∪	6.	3.
41. i	41.	∪ ∪	75. ſion	11. na
		—		
4.	4.	∪ ∪ — —	1.	4.
73. avoi	73.	∪ ∪ ∪	36. ke	43. vi
		— — —		
6.	6.	∪ ∪ ∪ —	6.	7.
54. pou	54.	— ∪ ∪	43. nou	76.
		∪ ∪		41.400
22. les	22.	— —	7.	
		— —	14. ſcou	4.
22. lés	22.	— —		24. to
		— —	3.	

A	B	C	D	E
4.	4.	∪ ∪ — —	43. ri	3.
26. t	26.	— —		14. no
		∪ ∪ —		
1.	1.	— — — —		7.
54. do	54.	— ∪ ∪		76.
		∪ ∪		25. 30
4.	4.	∪ ∪ — —		15. 5
26. t	26.	— —		
		∪ ∪ —		6.
3.	3.	— ∪ ∪ —		45. ome
34. po	34.	— — —		
		∪ ∪		3.
4.	4.	∪ ∪ — —		26. ou
16. ſt	16.	—		
		∪ ∪ —		2.
1.	1.	— — — —		56. j
36. ke	36.	— — —		
		∪ ∪ —		4.
2.	2.	— — ∪ ∪		73. avoi
61. la	61.	∪ ∪ —		
		—		7.
1.	1.	— — — —		76.
61. é	61.	∪ ∪ —		74,100,000
		—		
3.	3.	— ∪ ∪ —		1.
43. ri	43.	∪ ∪		61. é
		— — —		35. cu
1.	1.	— — — —		17. an
66. enne	66.	∪ ∪ —		67. es
		∪ ∪ —		
				6.
				52. pés

Example. F

7. See column 7 in the table.
75. Look at your book of numbers.
12. No. 2. of the book, " I ſhall ſend you," &c.
12. The 2d.
12. February.
12. Next year.

Ex. G

7. Column 7.
75. Look at your book of numbers.
14. No. 4, " *I ſhall ſet off*," &c.
14. Next Thurſday.
14. At 4 o'clock in the morning.

This univerſal language would ſave more than one half of the preſent writing, it would therefore have the advantage of ſhort hand.

As there are three ſorts of language. 1ſt. That of analogy. 2d. That of tranſpoſition, and a third of a mixed nature, it appears to me proper to begin by making a dictionary of 2 or 3 languages of each kind, and as theſe three ſorts differ from each other, if by adapting one to the other they ſhould become intelligible, it might not be difficult to bring all languages into the ſame plan.

I will not now enlarge on this ſubject, becauſe it would be uſeleſs, if my firſt and fundamental idea ſhould not be adopted.

Table

Table of Correſpondence by the Sight, the Hearing or the Feeling, by any Inſtrument capable of making an Impreſſion on either of the Senſes.

1		2		3		4	
11. a	51. da	11. fa	51. ja	11. na	51. ſa	11. ſta	51. xa
2. ab	2. dé	2. fé	2. jé	2. né	2. ſé	2. ſté	2. xé
3. ace	3. di	3. fi	3. ji	3. ni	3. ſi	3. ſti	3. xi
4. ad	4. do	4. fo	4. jo	4. no	4. ſo	4. ſto	4. xo
5. ai	5. du	5. fu	5. ju	5. nu	5. ſu	5. ſtu	5. xu
6. aiſe	6. d	6. f	6. j	6. n	6. ſ	6. ſt	6. x
7. an	7.	7.	7.	7.	7.	7.	7.
21. ba	61. e	21. ga	61. la	21. o	61. ſca	21 ta	61. za
2. bé	2. ec	2. gué	2. lé	2. ob	2. ſqué	2. té	2. zé
3. bi	3. eil	3. gui	3. li	3. oi	3. ſqui	3. ti	3. zi
4. bo	4. el	4. go	4. lo	4. oin	4. ſco	4. to	4. zo
5. bu	5. emme	5. gu	5. lu	5. on	5. ſcu	5. tu	5. zu
6. b	6. enne	6. gue	6. l	6. ou	6. ſq.	6. t	6. z
7.	7. es	7.	7.	7. oui	7.	7.	7.
31. ca	71. ette	31. gna	71. ma	31. pa	71. ſpa	31. u	71. ayant
2. qué	2. eu	2. gné	2. mé	2. pé	2. ſpé	2. ub	2. ayons
3. qui	3. euil	3. gni	3. mi	3. pi	3. ſpi	3. ui	3. avoi
4. co	4. eur	4. gno	4. mo	4. po	4. ſpo	4. un	4.
5. cu	5. ex	5. gnu	5. mu	5. pu	5. ſpu	5. une	5.
6. ckq	6. exem	6. gn	6. m	6. p	6. ſp	6. up	6.
7.	77.	7.	77.	7.	77.	7. us	77.
41. cha		41. i		41. ra		41. va	
2. ché		2. ian		2. ré		2. vé	
3. chi		3. ié		3. ri		3. vi	
4. cho		4. ien		4. ro		4. vo	
5. chu		5. ieu		5. ru		5. vu	
6. ch		6. in		6. r		6. v	
47.		47. ion		47.		47.	

5		6			7		
11. ban	51. fan	11. jan	51. pan		11. ſcan		51. van
2. bés	2. fés	2. jés	2. pés		2 ſqués.		2. vés
3. bon	3. fon	3. jon	3. pon		3. ſcon		3. von
4. bou	4. fou	4. jou	4. pou		4. ſcou		4. vou
5.	5.	5.	5.		5.		5.
6.	6.	6.	6.		6.		6.
7.	7.	7.	7.		7.		7.
21. can	61. gan	21. lan	61. ran		21. ſpan	9	61.
2. qués	2. gués	2. lés	2. rés	9	2. ſpés		2.
3. con	3. gon	3. lon	3. ron		3. ſpon		3.
4. cou	4. gou	4. lou	4. rou		4. ſpou		4.
5. queu	5.	5.	5.		5.		5.
6. ction	6.	6.	6.		6.		6.
7. ceuil	7.	7.	7.		7.		7.
31. chan	71. gnan	31. man	71. ſan		31. ſtan		71.
2. chés	2. gnés	2. més	2. ſés		2. ſtés		2.
3. chon	3. gnon	3. mon	3. ſon		3. ſton	9	3.
4. chou	4. gnou	4. mou	4. ſou	9	4. ſtou		4.
5.	5. ill	5. Mr.	5. ſion		5.		5.
6.	6. ique	6. M. de	6. ſin		6.		6.
7.	77.	7.	77.		7.		77.
41. dan		41 nan			41. tan		
2. dés		2. nés			2. tés		
3. don		3. non			3. ton		
4. dou		4. nou			4. tou		
5. etant		5. ome			5.		
6. été		6. ote		9	6.		
47. etoi		47.			47.		

Notes.

Notes.

1. Orthography is here out of the queſtion, it is ſufficient to be underſtood. The reader will therefore pleaſe to ſupply the letter *y* inſtead of *i*, (the *h* is underſtood) the *e* mute alſo as in *ſe*, *me*, *ne*, which are marked *ſ*, *m*, *n*, all double letters and thoſe that do not ſound, alſo the *l* and the *r*, at the end of ſyllables as *eigale armée* and alſo when one of theſe two letters follow a conſonant as *vraiſemblablement;* the ſenſe of the phraſe will ſufficiently indicate the letters ſuppreſſed, which however, might be marked by ſignal if neceſſary.

2. To correſpond by hearing, ſome ſonorous inſtrument ſhould be uſed, the figures may be expreſſed by ſounds or ſtrokes in ſlow or in quick ſucceſſion continued or interrupted.

1, by one long —
2, by two longs — —
3, by three longs — — —
4, by two briefs ◡ ◡
5, by one long and two briefs — ◡ ◡
6, by two briefs and one long ◡ ◡ —
7, by three briefs. ◡ ◡ ◡

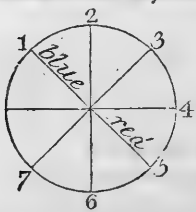
The figures ſhould be taken two and two to form the numbers which indicate the ſyllables, except the laſt 77, which indicates the word *column* in the table, and the number of the column will be indicated by the very next figure expreſſed. Example: I hear three precipitate ſounds, I then write 7 theſe are followed by three others, I then recognize 77 or column, then inſtead of writing another 7 I place a point over the firſt 7̇ to ſignify column, and I liſten for the next ſignal, if I hear three more precipitate ſounds, I add 7 to the pointed 7̇, which I underſtand to be column 7, thus 7̇7. Then I muſt look into that column for the ſyllables of all ſucceeding ſignals. This general

general way of ſignaliſing the columns, will do for the ſight, but there is another more expeditious way for the hearing, which will not deprive the columns of the No. 77.

Example. Four ſtrokes or ſounds may be combined in ſeven different ways.

First column — — — —
Second ditto. — — ᴗ ᴗ
Third do. — ᴗ ᴗ —
Fourth do. ᴗ ᴗ — —
Fifth do. — ᴗ ᴗ ᴗ
Sixth do. ᴗ ᴗ ᴗ —
Seventh do. ᴗ ᴗ ᴗ ᴗ

3. To correſpond by ſight, a flag of two colours will anſwer in the day time, which may be fixed to a turning



circle, the figures may be expreſſed by ſeven different poſitions and the eighth may ſerve to ſignaliſe a repetition of the laſt figure. At night a light may be made to appear and diſappear at unequal intervals to deſignate the longs — or briefs. ᴗ

4. It is evident that, with the ſame table, a like communication of intelligence might be conveyed by the touch, merely by taking hold of the hands; the table being previouſly adapted to this purpoſe, conſequently a deaf, a blind or a dumb perſon might converſe with any one who can read, provided he learns the table by heart.

5. The two particular columns which are included in the 7th column are to indicate numbers. Any number of this column may ſerve to indicate that all that follow are to be conſidered as quantities inſtead of ſyllables, until by a new ſignal you are referred to ſome other column; for inſtance let 76 be the number.

4 Example.

Example.	76	
	76	300,000
	52	4000
	26	40
		304040

N°. XX.

Memoir on the Subject of a new Plant, growing in Pennsylvania, particularly in the Vicinity of Philadelphia, by Mr. BEAUVOIS.

Read August 21, 1795.

AMONG the many obfervations I have hitherto made on the natural productions of this country, there is one which appears worthy of being prefented to you. This I do after having often repeated my obfervations on the plant both in the fields and in my own chamber, where I have preferved it thefe two months.

The firft time I faw this plant, was near Wilmington in a ditch continually wet, fince which I have found it between Reading and Pottfgrove, and latterly near this city, where it is very common, growing as it were under our eyes; we trample it under our feet; the cattle feed greedily on it, and yet it is unknown among botanifts, none of whom having publifhed any defcription of it. Whether it poffeffes any medicinal virtues or not I cannot fay. I have many times tafted it, and always found it infipid.

Although the filence of botanifts on fo common a plant, authorifes me to prefent it to you as a difcovery of my own, yet I fhall not attach fo high a value to the

 pretended

pretended merit of being the firſt diſcoverer of any production to which chance might as well have directed the ſteps of the moſt ignorant as thoſe of the moſt learned naturaliſts, as to withhold from you any thing I know relative to the ſubject. Dr. Barton, with whom I ſpoke on my return from Wilmington, informed me that Mr. Muhlenberg had mentioned it in his catalogue of plants, under the name of *Ixia* followed by ? that it has ſince been ſent to England to Mr. Smith, by the name of Bartonia clandeſtina, that this Engliſh botaniſt thinks it a new genus. Probably it has been but imperfectly examined by thoſe who ſent it to Mr. Smith, ſince they themſelves ſurname it clandeſtina. Nor has Mr. Smith himſelf publiſhed any deſcription thereof. We may then regard it as a new plant not deſcribed in any work.

In my opinion, this plant ſhould be placed in the ſame claſs with the Pontederia, to which it ſeems very nearly related in the natural order. I will not heſitate then to place it with the Narciſſi, claſs III. order VII. according to Juſſieu's method. But this naturaliſt has himſelf ſeen the genus *Pontederia*, as well as many others, which he has deſignated as not having a perfect affinity with the narciſſi, *genera narciſſis non omnino affinia*, ought to be ſeparated from them; and that he only places them thus until their proper order ſhall be determined, *donec horum verus ordo conſtiterit.* I ſhall only then, at preſent, ſhow the place of the new plant I have the honour to preſent you. It cannot be claſſed with the narciſſi, for it is wanting in one of their principal characters, viz. that of having ſix ſtamina. The difference in the number of ſtamina has not however, Gentlemen, determined me to make a new genus of it inſtead of uniting it with the *pontederia*; for if it only differed from it in this reſpect, it might as well belong to the pontederia, as the *valeriana rubra, calcitrapa, and cornucopiæ*, to the *valeriana*, the two

firſt

firſt having but one ſtamen and the third but two, three being the proper number in the valeriana. But the plant in queſtion is materially different in other reſpects. 1ſt, in the conſtantly unequal proportion of its ſtamina. 2d, in its corolla, and 3d, in the form of its ſeed.

However the moſt remarkable character of all, is to be found in the proportion of its ſtamina, always to the number of three, the two ſmaller of which are drawn together by a round yellow anthera inſerted at the orifice of the tube; and are always ſheltered as it were by a different diviſion of the larger ones, coloured more internally than the others at its baſe, which may be regarded as a ſort of nectarium; the third ſtamen is longer, having the ſame point of inſertion, but oppoſite to the two others. Its anthera is oblong of a dirty white. All the three ſtamina are fertile. This very particular character has determined me to make a new genus of the plant, at the ſame time it has furniſhed me with a name that appears perfectly applicable, *Heterandra*. I would willingly have continued the name of Bartonia, by which I am told it was ſent to England, in honour of Dr. Barton, whoſe knowledge and zeal in the ſtudy of nature are already known to you; but, beſides that I have always thought the names of men no ways applicable to plants, it appears to me more proper to give it ſuch a name as may deſignate the particular character which eſſentially diſtinguiſhes this plant from all others. Formerly, and even now, we are ſtill in the habit of giving proper names that are void of ſignification to plants. If this cuſtom had gone no farther than uſing the names of thoſe men who have been uſeful to ſcience it might have been tolerated; but now that pretenders to ſcience claim the ſame honour, it is unworthy the attention of men renowned for their genius and works. In fact, was it neceſſary that the names of Hippocrates, Tournefort, Linnæus, Juſſieu,

 and

and many others, ſhould be given to plants in order to be tranſmitted to poſterity? Or was it proper that ſuch men ſhould be claſſed with thoſe who hardly know botany by name, and yet have their names given to plants? The word Heterandra ſignifies two different kinds of ſtamina.

I finiſh this memoir by a comparative deſcription of this plant and the pontederia, which will enable you to decide on the juſtice of my obſervations, and may aſſiſt naturaliſts in fixing its proper place in the natural order.

Deſcription of the Pontederia, according to Linnæus.	*Deſcription of the Heterandra.*
Spatha communis, oblonga latere dehiſcens.	Spatha, ut in Pontederia, ex utriculo foliorum egrediens.
Corolla (calix Juſſieu) monopetala, bipartita, tubuloſa, labium ſuperius rectum, tripartitum, exterius æquale. Labium inferius reflexum, tripartitum laciniis æqualibus.	Corolla (calix Juſſieu) monopetala, ſexfida, ſubbipartita, tubuloſa, infera, labium ſuperius tripartitum, lacinia intermedia major, interior baſi colorata, nectarii æmula; labium inferius tripartitum, lacinia intermedia auguſtior, reflexa.
Stamina, filamenta ſex corollæ inſerta, quorum tria ſubulata, longiora ori tubi corollæ, tria reliqua baſi ejuſdem tubi inſerta. Antheræ erectæ, oblongæ.	Stamina, filamenta tria corollæ inſerta, quorum duo minora approximata ad baſin laciniæ nectarii æmuli, antheræ fulvæ rotundæ, altera longitudine ſtyli diſtans, anthera oblonga pallida.
Piſtillum germen oblongum inferum *aut ſuperum*. Stylus ſemplex, declinatus, ſtigma craſſiuſculum.	Piſtillum, germen ejuſmodi ſed ſemper ſuperum.
Capſula carnoſa, conica, apice lato inflexo, trilocularis, triangularis, triſulca.	Capſula, oblonga, trigona, trilocularis à latere dehiſcens.
Semina ſubrotunda, plurima.	Semina oblonga utriuſque obtuſa, plurima.

The

The annexed figure represents the other particularities and ſpecific characters of the plant, which I ſhall call *Heterandra reniformis.*

(Signed) BEAUVOIS.

N°. XXI.

A Letter from Colonel WINTHROP SARGENT, *to* Dr. BENJAMIN SMITH BARTON, *accompanying Drawings and ſome Account of certain Articles, which were taken out of an ancient Tumulus, or Grave, in the Weſtern-Country.*

Cincinnati, N. W. Territory, Sep. 8th, 1794.

Read May 20, 1796. I HAVE the pleaſure, my dear Sir, to tranſmit you a drawing of ſome matters more extraordinary than have heretofore come under my obſervation, in all the reſearches into the antiquities of this country. The multiplicity of my avocations leaves not leiſure for more than rough delineations; and you muſt be contented to receive them in this ſtyle. I poſſeſs all the originals, and intend by ſome ſafe conveyance preſenting them to the Philoſophical Society, ſhould they believe them of importance enough for a depoſit of my diſpoſition to promote the purpoſes of their inſtitution. The drawing, perhaps, is too imperfect to ſtand the teſt of criticiſm, and it might not be prudent to hazard it to their view. Your judgment, however, ſhould govern.*

It may be proper to add, that the body with which this collection was interred, was found lying in nearly a horizontal poſition, about five feet from the ſurface of the earth,

* Since the date of this letter, the Philoſophical Society have received the articles. From comparing them with the drawings, it appears that the latter are, in general, ſufficiently correct. B. S. B.

earth, with the head towards the ſetting ſun, and at the S. W. ſide of, or about fifteen feet from, an extenſive artificial mound of earth, raiſed probably for the purpoſe of a burial ground, upon the margin of the ſecond bank of the Ohio-river (ſuddenly riſing fifty feet above the firſt) and now elevated, in the extreme, eight feet from the general level of the ſame, with a gradual ſlope in the various directions, and a baſe of about 120 feet by ſixty. One of the main ſtreets of the town paſſes through the Weſtern part of this grave, and in the frequent repairs of the acclivity, human bones have often been found. You have, I think, been heretofore told by me, and perhaps received a ſketch, of very extenſive ancient fortifications at Cincinnati, not regular as thoſe at Muſkingum, but very worthy of notice.* I ſhould not omit to mention to you, that upon this mound are the ſtumps of oak-trees, ſeven feet diameter; and within ſeven feet, one of ſmall ſize ———— years of age. Many, in its vicinity, that might have been of more duration, are removed by the opening of this road, or ſtreet. In addition to the matters of which you have the drawing, were ſeveral utenſils, or ornaments, loſt or miſlaid. If hereafter they come to my view, you ſhall receive information.

In the meantime, and always, believe me deſirous of contributing to your amuſement, and of promoting ſcience all in my power.

Adieu,

W. SARGENT.

* For plans and deſcriptions of the ruins at Muſkingum, ſee the *Columbian Magazine*, for May 1787; and my *Obſervations on ſome Parts of Natural Hiſtory*, &c. printed in London, in the year 1787. B. S. B.

A Drawing

N°. XXII.

A Drawing of ſome Utenſils, or Ornaments, taken from an old Indian Grave, at Cincinnati, County of Hamilton, and Territory of the United-States, North-weſt of the River Ohio, Auguſt 30th 1794. *By* Colonel WINTHROP SARJENT. *Communicated by* BENJAMIN SMITH BARTON, M. D.

Explanations.

Figures.

1. A ſtone or compoſition, hard and ponderous; ſuperficies ſmooth and regular, almoſt as if finiſhed in a turner's lathe—mixed colours of black and white, or grey.
2. Do. do. of verditer colour throughout.
3. A cryſtalline ſubſtance, regularly wrought as the preceding figure, and of conſiderable degree of tranſparency.
4. As figure 1.—Mixed black and yellow colours.
5. Probably a compoſition, ponderous and of dark colour, like black glazed potter's ware—ſeems to have been hardened by the ſun or fire, and unequally compreſſed in the operation. Two views are preſented, better to ſhew this effect.
6. A repreſentation of the bill of ſome bird, not now known in this country.
7. A regular circular figure of ruſty black colour, tolerably well poliſhed, and not unlike ebony in appearance, but much leſs ponderous; probably either of coal or a compoſition:—No. 2. ſhews a ſegment of the ſame—its exterior and interior dimenſions—the groove or place for a band: at the

Explanations.

Figures.

the dotted lines are perforations, about a line diameter, which it would ſeem were intended to ſecure it upon a large axis.

8. Alſo a circular figure, yellowiſh colour—appears to have been hardened by the ſun or fire, and glazed—probably for ſimilar uſes with the laſt deſcribed—a double number of ſmall perforations, and its thickneſs three lines leſs.

9. A piece of thin ſheet-copper—Two perforations as deſcribed in the drawing—a roughneſs on the edge produced by mouldering in the grave.

10. A piece of ſheet or plate copper, which ſeems to have been wrought into an ornament for the hair: this, however, only conjecture: No. 1. ſhews the back and folding parts with four perforations.—No. 2. is intended to give an idea of the other ſide, which is ſwelled longitudinally into three pipes, or diviſions. The remains of ſome ſmaller pipes encloſed and now almoſt mouldered away, ſeem to deſtroy the idea of its being originally meant as a mere hair-ornament.

11. The two ſides of a bone, with the hieroglyphicks on each.

N. B. Some iſinglaſs, or talk, in regular figures—the greateſt about thirty inches circumference, and a few pounds of very rich lead-ore, were found in the grave.

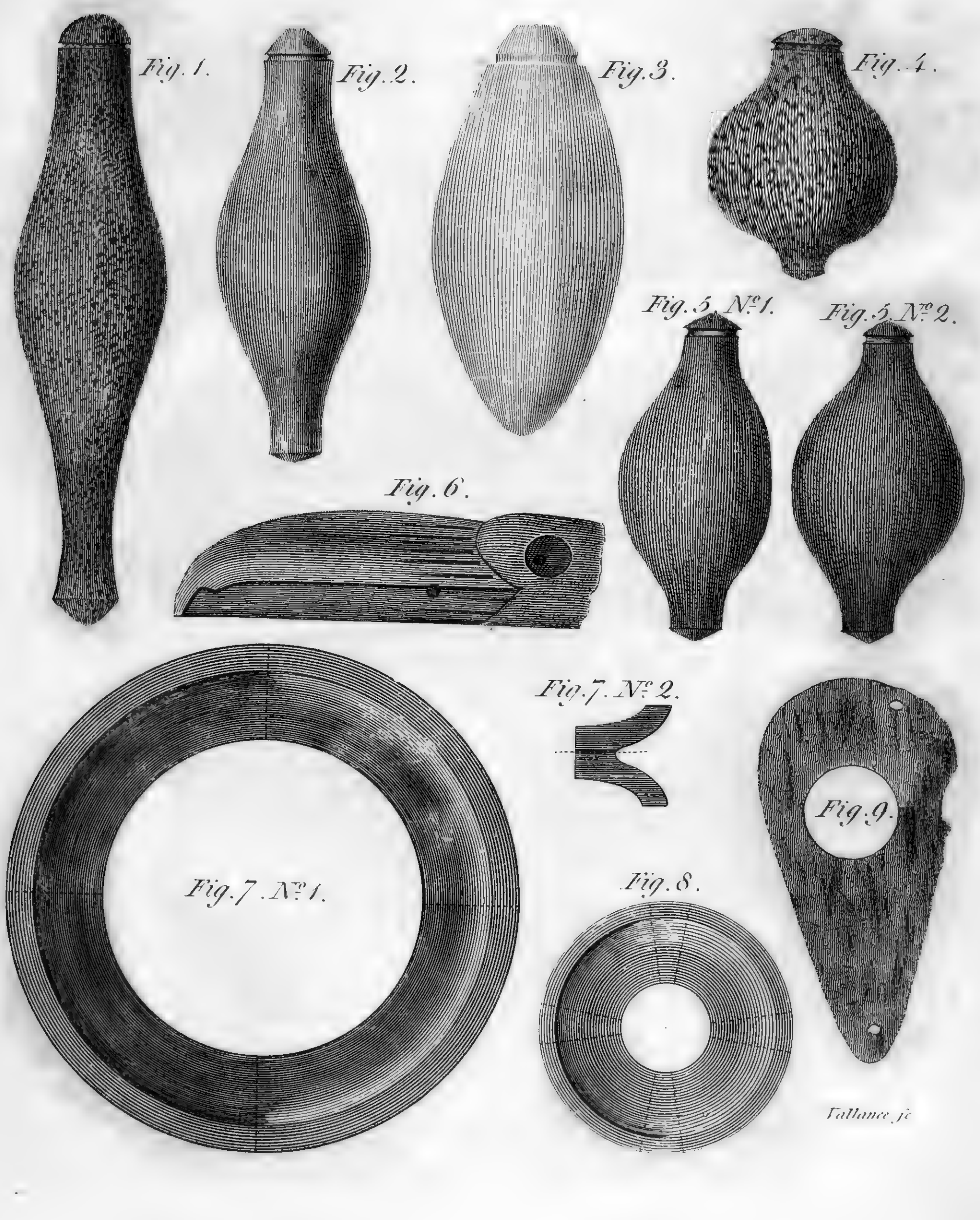


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5. No. 1.
Fig. 5. No. 2.
Fig. 6.
Fig. 7. No. 2.
Fig. 9.
Fig. 7. No. 1.
Fig. 8.
Vallance sc

Fig. 10. No. 1.

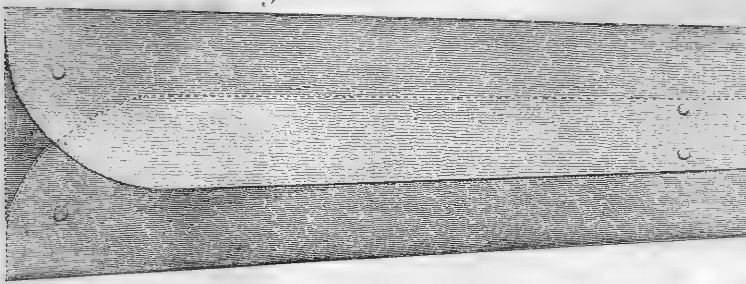


Fig. 10. No. 2.

Fig. 11. No. 1.

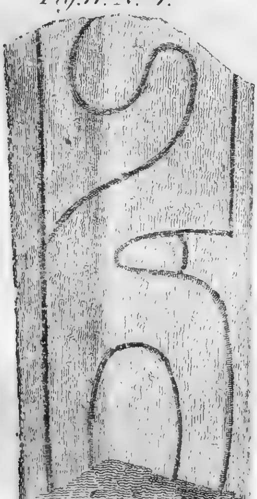
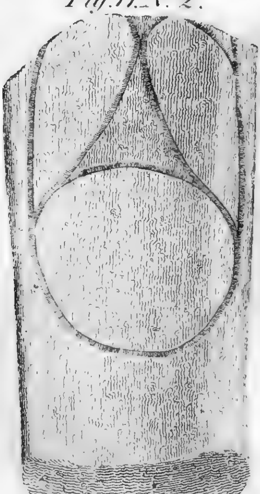


Fig. 11. No. 2.



Vallance fc.

N°. XXIII.

Obſervations and Conjectures concerning certain Articles which were taken out of an ancient Tumulus, or Grave, at Cincinnati, in the County of Hamilton, and Territory of the United-States, North-Weſt of the River Ohio: in a letter from BENJAMIN SMITH BARTON, M. D. *to the* Reverend JOSEPH PRIESTLEY, L L. D. F. R. S. &c.

Philadelphia, May 16th, 1796.

REVEREND AND DEAR SIR,

Read May 20, 1796. AS you expreſſed a deſire to ſee my obſervations and conjectures concerning the articles which were lately preſented to the Philoſophical Society, through my hands, by Colonel Winthrop Sargent, I take the liberty of troubling you with them, and ſhall think myſelf fortunate if they contribute any thing to your information, or amuſement. I need not tell you, that you will ſometimes find me leaving the ſure road of hiſtorical inquiry, for the narrow, and too often uncertain, path of the antiquary. In moſt of the inveſtigations and reſearches of the antiquary, ſome uncertainty is neceſſarily involved. The light which ſerves to conduct him is frequently extremely faint: the imagination and conjecture are, therefore, naturally called in to his aid. If this be ever allowable, it is eſpecially ſo in an inquiry, ſuch as the preſent, where the ſubjects of inveſtigation have been taken from the darkneſs of the GRAVE.

For the account of the diſcovery of the articles, concerning which I am about to offer my opinion, I refer you to Colonel Sargent's letter to me, which has been read before the Philoſophical Society, and which you will receive, along with my obſervations. I proceed, therefore, to the immediate buſineſs of my communication.

I propoſe, in the firſt place, to inquire by what people theſe articles were made; and, ſecondly, for what purpoſes they were intended.

FIRST.

FROM the obvious antiquity of the tumulus in which they were found; from their general fabric, or appearance, and from the materials out of which ſome of them are formed, it muſt, at firſt ſight, ſeem very improbable, that theſe articles are the work of any people in the ſtate of ſociety and improvement of the Indian or ſavage nations of North-America, that are now known to us. Theſe nations, although they are not, as has been aſſerted, "the verieſt ruins of mankind,"* and although in the range of human improvement, and of human glory, they actually rank higher than many of the ancient and modern nations of the old-world, it muſt ſtill be confeſſed, are in a very humble ſtage of ſociety: humble, at leaſt, when contraſted with the point of improvement in manners, in arts, and in ſciences, to which many nations have attained. But are there no proofs that the rude nations of America have fallen from a more reſpectable form of ſociety than that in which we now contemplate them? It appears to me that there are. Theſe proofs are even numerous. Some of them are monuments whoſe magnitude or materials ſhall ſecure to them an

* "Mr. Hooker ſays, they are the verieſt ruins of mankind upon the face of the earth." See Governor Hutchinſon's Hiſtory of Maſſachuſetts, Vol. I. p. 414. Salem: 1795.

an existence, when the nations by whose ancestors they were constructed shall have passed away.

In the following inquiry, I shall offer some of my reasons for believing that there formerly existed in many parts of North-America, a race of people, who, whilst they were more numerous, had made much greater advances in the arts, and in improvement, than the present races of Indians, *or than their ancestors since our actual acquaintance with them.* This mode of investigation cannot be said to be foreign to my subject, since it is my opinion that the artificial tumular articles are the work of some of the ancient and more improved people to whom I allude. Besides, by pursuing the inquiry in this manner, I shall render my letter more worthy of your notice, and of the notice of our Philosophical Society, to whom I wish you to present it.

The Aztecas, or Mexicans, in the progress of their migration from the northern country of Aztlan, to the vale in which they afterwards founded the capital of their empire, discovered many and extensive ruins. These ruins were supposed, by the Mexicans, to be vestiges of the Toltecas, a numerous and powerful people, who had made greater advances in the arts of life, and in one of the sublimest of sciences,* than any of the other nations of the new-world. The Toltecas are said to have begun their emigration towards the close of the sixth or the beginning of the seventh century of the Christian æra.† The Mexicans began their departure about the middle of the twelth century.‡ If these accounts, therefore, can be depended upon, it would appear that the works discovered by the Mexicans had been constructed sometime

* Astronomy. † About the year 596, according to Clavigero.

‡ According to Clavigero, in the year 1160. Dr. Robertson says, it was "towards the commencement of the thirteenth century." The History of America, Vol. III. p. 156. London: 1796.

between the beginning of the feventh and the middle of the twelfth century.

Whatever credit may be due to this traditional account concerning the Toltecas; whether they were the anceftors of the Peruvians, as I have fometimes been induced to believe;* whether they were an aboriginal or foreign colony whofe progeny excites no more, or whether the whole is a tale that has no foundation in truth, I fhall not paufe to inquire. Whatever may be the fate of thefe fpeculations concerning the Toltecas, I think no perfon that has minutely attended to the numerous veftiges which are daily difcovered in various parts of North-America, will hefitate to believe, that there has been a period when a great part of this continent was inhabited by nations who were more numerous than the prefent races of Indians, and who had attained to a confiderable degree of improvement in the arts.

The veftiges to which I allude are of various kinds. They are principally, however, mounds of earth of different forms and fizes; fome of them, undoubtedly, depofitories of human bones; whilft others appear to have been conftructed as the bafes of temples, that were erected during the extenfive reign of an hideous fuperftition in America. Others, again, and thefe are the principal,

* The empire of the Toltecas is faid to have terminated about the year 1052. The Spaniards firft arrived in Peru in the year 1526, at which time Huana Capac was the reigning monarch of the country. According to the Peruvian ftory, Huana was the twelfth monarch, in fucceffion, from Manco Capac, who is faid to have founded the Empire about four hundred years before. This period will carry us back to within lefs than one hundred years of the end of the Toltecan empire. My account of the Toltecas is taken from the Abbé Saverio Clavigero's *Hiftory of Mexico*, one of the moft valuable works that has ever been publifhed on the fubject of America. *The Hiftory of Mexico, collected from Spanifh and Mexican Hiftorians*, &c. tranflated from the original Italian by Charles Cullen, Efq. Vol. I. p. 83, 84, 85, 88, and 89. London: 1787. It is rather remarkable that Acofta makes no mention of the Toltecas.

appear

appear to have been intended as fortifications, or the walls of large towns.

Theſe remains are ſcattered over an immenſe extent of territory in North-America. They are, however, leſs numerous in what I call the eaſtern-diſtrict of this continent: I mean that diſtrict which, is included between the great ranges of our mountains and the Eaſtern or Atlantic Ocean. It is not difficult to explain the cauſe of this difference. In the weſtern-diſtrict, or the tract of country between the Alleghaney-mountains and the Miſſiſſipi, and from this river to the Pacific-Ocean, the moſt poliſhed nations of America, north of Mexico, reſided.* All the eaſtern nations appear to have migrated from the weſt, from the north-weſt, or from the ſouth-weſt.† This ſeems to me to be a fact, prominent and well eſtabliſhed in the hiſtory of the aboriginal Americans. It is particularly eſtabliſhed by the circumſtances of the greater population and the ſuperior poliſh of the weſtern nations, when they were firſt diſcovered; by the uniform traditional accounts of all the eaſtern nations; and by the actual march of many Indian tribes, who now occupy, or who within the laſt two hundred years, did occupy, ſome of the countries eaſt of the Alleghaney-mountains.

It has been a much agitated queſtion, in this country, by what people the veſtiges which I am conſidering were conſtructed? Nor has the queſtion remained unnoticed

* The earthen fortifications are very numerous in that extenſive tract of country which is bounded by the Miſſiſſipi on the eaſt, by the Miſſouri on the north, by a range of mountains on the weſt, and by the White-River on the ſouth.

† I do not except from this obſervation the Eſquimaux, who in the opinion of Dr. Robertſon, were emigrants from the north of Europe. *Hiſtory of America*, Vol. II. p. 40, 41, and 42. Profeſſor Blumenbach has well denominated this notion of the eloquent hiſtorian "paradoxa opinio." *De Generis Humani varietate nativa*, p. 318, nota. Gottingæ, 1795. *Paradoxæ opiniones* are very numerous in Dr. Robertſon's celebrated *Hiſtory of America*.

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in Europe. Some have ſuppoſed, that they were erected by the army of Fernando De Soto, before the middle of the ſixteenth century.* But this opinion was hardly worthy of a ſerious conſideration. By ſome they have been attributed to the Welſh, and by ſome to the Mexicans;† whilſt by others, again, they have been conſidered as proofs of the exiſtence of extenſive *civilized* nations in America, at ſome very remote period of time.‡

It is now about ten years ſince I firſt turned my attention to the ſubject of the American monuments, and ſince I began to collect materials for a work which is intended to involve the phyſical and moral hiſtory of the aboriginal Americans.§ In this work, the favourite ob-

* See *The American Magazine*, for December 1787, p. 15, 16, 17, 18, and 19. Alſo the ſame for January, 1788, p. 87, 88, 89, 90, 91, 92, and 93. for February, 1788, p. 146, 147, 148, 149, 150, 151, 152, 153, 154, 155, and 156.

† The conjecture which I formerly offered to the public concerning the original of theſe American monuments, I think it neceſſary to explain with more perſpicuity, becauſe it has evidently been miſrepreſented. My hypotheſis was briefly this, that the fortifications, and other artificial eminences in America, were conſtructed by the Toltecas, or ſome other American nation, and that the Danes were the anceſtors of that nation. I had alſo imagined that the Danes had *contributed* to the peopling of America. See *Obſervations on ſome Parts of Natural Hiſtory: to which is prefixed an Account of ſeveral remarkable veſtiges of an ancient date, which have been diſcovered in different parts of North-America.* Part firſt, p. 65. But I did not imagine, or aſſert, that this continent "was peopled from the north of Europe, probably by the Danes." See *the Critical Review*, for October, 1787, p. 260, and 261. On the contrary, in another place, I had mentioned it as a ſuppoſition more than probable, that America "has been peopled from a thouſand ſources;" ſee *Obſervations, &c.* p. 4, and had even hinted that the Iroquois came off from the north-eaſt parts of Aſia. *Ibid.* p. 66.

‡ This ſeems to be the opinion of Dr. Dunbar of Aberdeen. See his Eſſays on the Hiſtory of Mankind in rude and cultivated ages, p. 193. London: 1781.

§ My friends, if not the public at large, have often inquired concerning this work. I have never heſitated to aſſign the true reaſons for its delay. Tied down, by the neceſſities of life, to the practice of an anxious and an arduous profeſſion; depending upon this profeſſion for my daily bread and ſubſiſtence, it is obvious that I am not in poſſeſſion of that leiſure and of that ject of my earlier and my prefent days, I hope I fhall be able to demonftrate the phyfical antiquity of America; the remotenefs of its population;* the countries from which it was peopled; and the fewnefs of its radical languages. I truft, I fhall alfo be able to vindicate, from the afperfions of certain popular and eloquent writers,† the intellectual character of the Americans. And although I fhall not be able to fhew that *highly civilized*

that freedom of mind, which are neceffary even for the exact *arrangement* of thofe materials which my early enthufiafm, and my early labour put me in poffeffion of. But I have not relinquifhed the idea of publifhing this work. On the contrary, I am ftill affiduous in collecting new materials, and hope to publifh the whole in two or three years. Having greatly extended my original plan, I cannot flatter myfelf with the profpect of fubmitting my labours to the public much fooner than the period juft mentioned.

* The recent date of the population of America has been warmly contended for by feveral writers. I could wifh, that my excellent friend, the Reverend Dr. J. Belknap, had not leaned to this notion. See his *Differtation on the Colour af the Native Americans, and the Recent Population of this Continent*. Bofton: 1792. One of the moft curious arguments that I have heard urged in favour of the late peopling of America, was that of the able profeffor Dugald Stewart, of Edinburgh. In his lectures, he fpake of the great uniformity in the figure and complexion of the native Americans. He imagined that climate, or fituation, is the principal phyfical agent that varies the human form and complexion. But the Americans, from their uniform appearance, cannot, he faid, long have inhabited the countries of America; fo that the climates have not had time to produce their great effects. Without denying the immenfe influence of climate, &c. and believing, as I do, that the greater part of the Americans are *Afiatics*, I cannot help obferving that thofe writers who fuppofe that there is but very little difference between the Americans, in different parts of this vaft continent, are greatly miftaken. See what the Abbé Molina has faid on this fubject. "Rido fra me fteffo, quando leggo in certi fcrittori moderni riputati diligenti offervatori, che tutti gli Americani hanno un medefimo afpetto, e che quando fe ne abbia veduto uno, fi poffa dire di avergli veduti tutti. Codefti autori fi lafciarono troppo fedurre da certe vaghe apparenze di fomiglianza procedenti per lo più dal colorito, le quali fvanifcono tofto che fi confrontano gl' individui di una nazione con quelli dell' altra. Un Chilefe non fi differenzia meno nell' afpetto da un Peruviano, che un' Italiano da un Tedefco. Io ho veduto pur dei Paraguaj, de' Cujani, e dei Magellanici, i quali tutti hanno dei lineamenti peculiari, che li diftinguono notabilmente gli uni dagli altri." Sulla Storia Naturale del Chili. p. 336.

† Buffon, De Pauw, Raynal, and Robertfon.

nations had ever poſſeſſed the countries of America, previouſly to the diſcovery of Columbus, yet it will be eaſy to demonſtrate, that theſe countries were formerly poſſeſſed by nations much farther advanced in civilization, than the greater number of the nations north of the empire of Mexico: by nations who muſt have been extremely numerous.

I do not ſuppoſe that theſe more poliſhed nations of America have entirely paſſed away. Some of them, it is probable, are extinguiſhed. But of others, I ſuppoſe that it is chiefly the ſtrength and the glory that are no more. Their deſcendants are ſtill ſcattered over extenſive portions of this continent, ſubſiſting chiefly by fiſhing and by the chaſe; and contenting themſelves with a ſlender and imperfect agriculture, ſuch as is ſuited to the manners and the numbers of rude and uncultivated tribes.

In an inquiry into the hiſtory of the Americans, the mind, unbiaſſed by ſyſtem, calm and deliberate in its reſearch, cannot fail to diſcover unequivocal proofs of the ancient ſtrength and reſpectability of the anceſtors of many of the ſavage Indian tribes who now inhabit the countries of America. The limits of this letter will not permit me to exhibit a full view of the arguments which may be adduced in favour of this opinion. All that I ſhall attempt to do, is to mention ſome of the principal heads of proofs; and that in as conciſe a manner as I well can.

That many of the North-American tribes are the deſcendants of nations much more populous, and much more poliſhed, than themſelves, I infer from the following circumſtances: viz.

Firſt. From their traditions. According to theſe traditions, many Indian nations were much more numercus and improved in ancient times, than when the Europeans firſt became acquainted with them. On this ſubject, there is much uniformity in theſe traditions. The Indians ſpeak of the great power of their chiefs in thoſe days of higher improvement, and aſſure us that wars* and peſtilential diſeaſes† were the great cauſes of their

* Mr. James Adair, ſpeaking of the Indians, ſays, "Through the whole continent, and in the remoteſt woods, are traces of their ancient warlike diſpoſition." *The Hiſtory of the American Indians*, p 377. London: 1775. The numerous fortifications, that have been already diſcovered, ſeem ſtrongly to favour the idea, that the ancient nations of America were very warlike. From contemplating theſe fortifications, one is almoſt induced to ſay, what Florus has ſaid of the Sarmatæ, "that they knew not what peace was." "Tanta barbaries eſt," ſays the Roman hiſtorian, "ut pacem non intelligant." L. Annaei Flori Epitome Rerum Romanarum. Lib. iv. cap. xii.

† I am inclined to think that fevers, probably contagious fevers, had contributed very greatly to the depopulation of the American nations, before the arrival of the Europeans among them. I could adduce many facts, from the early writers, which would give ſupport to this ſuppoſition: but, at preſent, this is not neceſſary. The ſubject will be attended to in my memoir concerning the diſeaſes and remedies of our Indians. In the meanwhile, I ſhall mention only one of the many writers, whom I have examined on this ſubject.

Daniel Gookin, in his *Hiſtorical Collections of the Indians in New-England*, ſpeaking of the Pawkunnawkutts, who were once a populous nation in New-England, ſays, "This nation, a very great number of them, were ſwept away by an epidemical and unwonted ſickneſs, An. 1612 and 1613, about ſeven or eight years before the Engliſh firſt arrived in thoſe parts to ſettle the colony of New Plymouth. Thereby divine providence made way for the quiet and peaceable ſettlement of the Engliſh in thoſe nations. What this diſeaſe was, that ſo generally and mortally ſwept away, not only theſe but other Indians, their neighbours, I cannot well learn. Doubtleſs it was ſome peſtilential diſeaſe. I have diſcourſed with ſome old Indians, that were then youths; who ſay, that the bodies all over were exceeding yellow, deſcribing it by a yellow garment they ſhowed me, both before they died, and afterward." The ſame writer, ſpeaking of the Maſſachuſetts, ſays, "In An. 1612 and 1613, theſe people were alſo ſorely ſmitten by the hand of God with the ſame diſeaſe, before mentioned in the laſt ſection: which deſtroyed the moſt of them, and made room for the Engliſh people of Maſſachuſetts colony, which people this country, and the next called Pawtuckett. There are not of this people left at this day above three hundred men, beſides women and children." See the valuable *Collections of the Maſſachuſetts Hiſtorical Society, for the year* 1792. Vol. I. p. 148. Gookin's "Epiſtle Dedicatory" is dated December 7th, 1674.

 ſplitting

ſplitting into ſuch numerous tribes, and of their ſcattered diſperſion over this vaſt continent.

Secondly. Excluſively of a diminution in their numbers, many of the North-American tribes are much leſs poliſhed and improved now than they were two hundred years ſince, when the Europeans firſt became acquainted with them. Declining in induſtry, they have neglected, if not forgotten, ſome of the arts by which they were diſtinguiſhed. They are no longer ſtudious to preſerve the humble ſtory of their country; the ſublimeſt features of their religion, the acknowledgment of a great ſuperintending ſpirit, or God, and of a place of future repoſe or happineſs, are clouded in ignorance, and hardly known.* In ſhort, we behold them rapidly paſſing to a melancholy decay, without our being able, in many inſtances, to determine to what cauſes their declenſion is owing.† Does not this *known* declenſion from a more

* In Adair's *Hiſtory of the American Indians*, there is a greater collection of facts relative to the corruption or alteration of the religious notions and ceremonies of the Indians (particularly the Cheerake, Muſkohge, Choktah, Chikkaſah, and Katahba) than is to be found in any other work that I have ever met with. Adair had great opportunities of being acquainted with the Indians, and his work certainly contains many highly intereſting facts. I believe him to have been a man of veracity; but, in the fulneſs of his enthuſiaſm for a ſyſtem, he appears, in ſome inſtances, to have ſhaped and pared his facts to ſuit his purpoſe: he is, therefore, a guide who may miſlead. Still his work ought to be read by every perſon who is curious of Indian matters. The following facts are well calculated to ſhow the altered ſtate of ſome of the American tribes. The *Buſk*, or *green-corn-dance*, of the ſouthern Indians, was originally a very ſolemn religious inſtitution. But many Indians, who ſtill attend at the buſk, are entirely ignorant that it is an inſtitution of a religious kind. Some of the Indian tribes, which are well remembered to have offered up ſacrifices, offer them up no more. The Onondagoes have an annual ſacrifice. The animal which they make choice of for this purpoſe is a large tortoiſe, and in defect of this a bear. An intelligent Indian, who gave me this information, confeſſed that he could not tell me, whether the ſacrifice was made to the good or to the evil ſpirit. A little REVELATION would be of great uſe to ſuch people as theſe.

† "The greateſt part of the nations of Louiſiana had formerly their temples as well as the Natchez, and in all theſe temples a perpetual fire is kept

refpectable ftate of improvement, favour the opinion that, previoufly to our acquaintance with them, the Americans were both more numerous and cultivated than they have been at any fubfequent period? For it is certain that *we* have not been the fole inftruments concerned in their decline, and fall.

Thirdly. The mythology of many of the American tribes appears to be the remnant of the mythology of certain Afiatic nations, who are much farther advanced in civilization than any of the prefent Indian tribes of North-America.

Fourthly. The Mexicans are known to have made confiderable progrefs in the fcience of aftronomy. Among the rude nations of North-America, aftronomical *principles* were not found. But among thefe nations, we have difcovered certain fmall *fragments*, which appear to be parts of the Mexican fyftem, or of the fyftem of fome people to whom aftronomy, as a fcience, muft have been known, however remote the period.

Fifthly. The ftructure of the languages of many American tribes is favourable to the idea, that thefe people were, formerly, much more improved than they are at prefent. Moreover, many of thefe languages are much more fertile than has been commonly fuppofed.

kept up. It fhould even feem, that the *Maubilians* enjoyed a fort of primacy in religion, over all the other nations in this part of Florida; for when any of their fires happened to be extinguifhed through chance, or negligence, it was neceffary to kindle them again at theirs. But the temple of the Natchez is the only one fubfifting at prefent (viz. in 1721), and is held in great veneration by all the favages inhabiting this vaft continent, the decreafe of whofe numbers is as confiderable, and has been ftill more fudden, than that of the people of Canada, without its being poffible to affign the true reafon of this event. Whole nations have entirely difappeared within the fpace of forty years at moft; and thofe who ftill remain, are no more than the fhadow of what they were, when M. De Sale difcovered this country." Journal of a Voyage to North-America. By P. De Charlevoix, Vol. II. p. 273 and 274. Englifh Tranflation. London: 1761.

The falſehoods or the errors of De Pauw, on this ſubject, are numerous.*

Connected with this ſubject, it may not be improper to obſerve, that the American nations appear to be remarkably retentive of their languages; I think more ſo than moſt other nations in their ſtate of improvement.† Perhaps, this fact rather favours and ſtrengthens the opinion which I am endeavouring to eſtabliſh. In proportion to the original poverty of a language, will not that language be unſtable? In proportion to its original fertility or extent, or in other words to the ancient improvement of thoſe who ſpeak it, will it not be leſs liable to change, more likely to preſerve its genius and features?

Sixthly. It has been obſerved that among the Mexicans, a people much more poliſhed than any of our preſent Indian tribes, the reſpect paid by inferiors to their ſuperiors "was preſcribed with ſuch ceremonious accuracy, that it incorporated with the language, and influenced its genius and idiom. The Mexican tongue abounded in expreſſions of reverence and courteſy. The ſtile and appellations, uſed in the intercourſe between equals, would have been ſo unbecoming in the mouth of one in a lower ſphere, when he accoſted a perſon in higher rank, as to be deemed an inſult."‡ The Mexicans were not alone in

* See Recherches Philoſophiques ſur les Americains, &c. Tome II. A Berlin: 1777.

† Mr. William Stith talks of "the Unſtableneſs and vaſt Mutability of the *Indian* Tongues," &c. *The Hiſtory of the firſt Diſcovery and Settlement of Virginia.* p. 13. Williamſburg: 1747. If Mr. Stith had been at the trouble of comparing the Indian languages of his day with thoſe of the middle of the preceding century, he would not have made ſo precipitate an aſſertion.

‡ See Dr. Robertſon's Hiſtory of America, Vol. III. p. 165. "It is, ſays Robertſon, to P. Torribio de Benavente, that I am indebted for this curious obſervation. Palafox, biſhop of Ciudad de la Puebla los Angeles, confirms and illuſtrates it more fully. The Mexican (ſays he) is the only language in which a termination indicating reſpect, *ſilavas reverentiales y de corteſia,* may be affixed to every word. By adding the final ſyllable *zin* or

azin

this respect. The Natchez, who lived north of Mexico, had two languages; a language of the nobles, and a language of the common people.* Other North-American tribes, such as the Chippawas and Christianaw, make use of two languages. One of these, which is only spoken in the councils of the nation, is very different from the other, which is spoken out of the councils. I consider these facts as strong arguments in favour of my opinion.†

azin to any word, it becomes a proper expression of veneration in the mouth of an inferior. If, in speaking to an equal, the word Father is to be used, it is *Tatl*, but an inferior says *Tatzin*. One priest speaking to another, calls him *Teopixque*; a person of inferior rank calls him *Teopixcatzin*. The name of the emperor who reigned when Cortes invaded Mexico, was *Montezuma*, but his vassals, from reverence, pronounced it *Montezumazin*. Torribio, MS. Palaf. Virtudes del Indio, p. 65. The Mexicans had not only reverential nouns, but reverential verbs. The manner in which these are formed from the verbs in common use, is explained by D. Jos. Aug. Aldama y Guevara in his Mexican Grammar, N° 188." The History of America, Vol. III. note xxii. p. 368.

* "They (the Natchez) have two languages, that of the nobles and that of the people, and both are very copious. I will give two or three examples to shew the difference of these two languages. When I call one of the common people, I say to him *aquenan*, that is, *hark ye*: if, on the other hand, I want to speak to a Sun, or one of their nobles, I say to him, *magani*, which signifies, *hark ye*. If one of the common people call at my house, I say to him, *tachte—cabanacte*, *are you there*, or I am glad to see you, which is equivalent to our good-morrow. I express the same thing to a Sun by the word *apapegouaiché*. Again, according to their custom, I say to one of the common people, *petchi*, *sit you down*; but to a Sun, when I desire him to sit down, I say, *caham*. The two languages are nearly the same in all other respects; for the difference of expression seems only to take place in matters relating to the persons of the Suns and nobles, in distinction from those of the people." *The History of Louisiana, or of the Western Parts of Virginia and Carolina, &c.* By M. Le Page Du Pratz, p. 328. English Translation. London: 1774. From several circumstances, it appears very probable, that the Natchez were originally a part of the Mexican empire, and that they moved north-east, to the west and east sides of the Mississipi, *after* the arrival of Cortez in Mexico.—This once powerful, and (with respect, at least, to many of the surrounding nations) this cultivated, people is now no more. Their peculiar dialect (as far as we know) is lost. But, then, their hideous religion has also perished.

† Speaking of the peculiarity in the Mexican language, which I have just taken notice of, Dr. Robertson observes, " It is only in societies, which time

Seventhly. I have already hinted, that the radical languages in North-America are but few. I know, indeed, that a very oppofite opinion has been entertained by an enlightened American philofopher.† But the *dialects* of the American languages are very numerous. Thus there are, at leaft, forty dialects of the language of the Lennape, whom we call Delawares. Many of thefe dialects have receded fo little from the parent ftock, that we cannot hefitate to conclude, that the period is not very remote when the tribes who fpeak them were one and the fame people. Moreover, within the period of two hundred and fifty years, we have feen one nation of Indians, from various caufes, feparating into feveral, and the fame language fplitting into dialects. This was the cafe with fome of the fouthern nations, which are known to have migrated, from the weft, acrofs the Miffiffipi. Thefe circumftances, by pointing out the great confolidation of the Americans, in former ages, ftrongly fupport my opinion, that they were once much more cultivated than we have ever known them: for extenfive affociations of men cannot be formed, or, at leaft, cannot long fubfift, in the favage ftate.

time and the inftitution of regular government have moulded into form, that we find fuch an orderly arrangement of men into different ranks, and fuch nice attention paid to their various rights." *The Hiftory of America*, Vol. III. p. 165. Perhaps, this remark is not very *republican*, but it is, neverthelefs, ingenious and juft. Among the Natchez, the feparation of ranks was well eftablifhed; and it was once eftablifhed among many other Indian tribes, where, at prefent, it is hardly known. I have already faid (p. 189) that the Indians fpeak of the power of their chiefs in former times. This power of the chief even extended (in fome inftances) to the taking away the life of his fubject, without the form of judgment, or trial. The chiefdom, at prefent, is feldom, if ever, hereditary. But that it was once hereditary among many of our tribes, is a fact well eftablifhed by the teftimony of feveral of the early writers concerning America.

† Mr. Jefferfon. See his Notes on the State of Virginia, p. 164 and 165. London: 1787.

Eighthly.

Eighthly. There are ſeveral reaſons for believing, that the anceſtors of ſome of the preſent races of Indians were acquainted with a kind of hieroglyphick-writing, very ſuperior to the rude picture-writing now in uſe among them.* We diſcover the veſtiges of ſuch hieroglyphicks among the Mickmacks of Nova-Scotia, and among ſome other tribes. Moreover, we diſcover many proofs of the ancient exiſtence of hieroglyphicks in various parts of North-America.† In the weſtern parts of Virginia, I have examined a large ſtratum of rock, which is engraven with hundreds of hieroglyphicks.‡ They are, doubtleſs, very ancient; and muſt, I think, have been the work of a people acquainted with the uſe of iron inſtruments, or with hardened metallick inſtruments of ſome kinds. In examining the *China Illuſtrata* of the celebrated Athanaſius Kircher, and the *Hiſtorico-*

* See a paper, by Sir William Johnſon, in the *Philoſophical Tranſactions* of the Royal Society of London, Vol. LXIII. p. 143. alſo Bernard Romans's *Conciſe Natural Hiſtory of Eaſt and Weſt Florida, &c.* p. 102 and 103. New-York, 1776. On the ſubject of this picture-writing, the reader may conſult La Hontan, Lafitau, and others, who have written on the ſubject of America.

† See, not to mention other writers, on this ſubject, Profeſſor Kalm's Travels into North America, Vol. III. p. 123, 124, 125, 126, and 127. Engliſh Tranſlation. London: 1770 and 1771.

‡ Theſe inſcriptions are engraven on a large ſtratum of rocks, on the ſouth-eaſt ſide of the River-Ohio, about two miles below the mouth of Indian or King's-Creek, which empties itſelf into the Ohio about fifty miles below Fort-Pitt. The greater part of the rocks lies nearly horizontally, and ſo near to the edge of the river, that at times the water entirely covers them. At the diſtance of a few yards, however, from the bank of the river, there are ſeveral large *maſſes* of the ſame ſpecies of rock, on which alſo I obſerved inſcriptions: *theſe*, it is probable, have been formerly attached to the horizontal ſtratum, and have either been removed by the hand of man, or by ſome violent inundation of the river. It is, at leaſt, certain, that the inſcriptions upon both are of the ſame kind, and there can be little doubt that they have both been engraven at the ſame time.

The horizontal ſtratum of rocks extends, for a conſiderable diſtance, along the border of the Ohio: but, I cannot, with certainty, affirm how large a portion of it is engraven with the inſcriptions, or marks.

Geographical Defcription of Strahlenberg, I have difcovered that fimilar hieroglyphicks are found, both engraven and painted, upon rocks, in the northern parts of Afia. It was, partly at leaft, from a comparative view of thefe hieroglyphicks, that I was early led to believe that America has derived its inhabitants from Afia; an opinion which, I am perfuaded, will acquire additional probability and ftrength, in proportion as we fhall compare the phyfical appearances, the religions and mythology, and, above all, the languages of the Americans and northern Afiatics with each other.

That fuch hieroglyphicks were in ufe among the anceftors of our Indians, is rendered probable by another circumftance. Notwithftanding the authority of Kircher, and of Brianus Walton,* and the affertions of De Pauw,† it is certain, that the Mexicans, the Acolhuas, the Tlafcalans, and other more improved nations of the Mexican empire, among other fpecies of writing, were acquainted with that of hieroglyphicks.‡ This fact is attefted by the learned Acofta, by Torquemada, by Gomara, by Solis, by Boturini, and by feveral other writers,§ who

* In his Biblia Sacra Polyglotta, &c. Londini: 1657.

† Recherches Philofophiques fur les Americains, Tome II.

‡ See Clavigero's Hiftory of Mexico, Vol. II.

§ I have not had an opportunity of examining the works of Torquemada, Gomara, and Boturini; but what Acofta and Solis affert is decifive. "One of our company of Jefuites, a man very witty and wel experienced, did affemble in the province of *Mexico*, the Antients of *Tefcuco*, of *Talla*, and of *Mexico*, conferring at large with them, who fhewed unto him their books, hiftories and kalenders, things very woorthy the fight, bicaufe they had their figures and hierogliphicks, wherby they reprefented things in this manner: Such as had forme or figure, were reprefented by their proper images, and fuch as had not any, were reprefented by characters that fignified them, and by this meanes they figured and writ what they would." *The Naturall and Morall Hiftorie of the Eaft and Weft Indies, &c.* lib. 6. chap. 7. p. 446. Englifh Tranflation. London: 1604. Don Antonio De Solis, fpeaking of the Mexican paintings, fays the fame thing. To make their pictures "the more intelligible, they placed fome Characters here and there, with which they feemed to explain, and give the Signification of the Picture. This were well acquainted with the Mexicans, or with their hiſtory, and whoſe authority, with candid inquirers, will, certainly, weigh much more than the invective *Recherches* of De Pauw, the eloquent puerilities of Buffon, or the weak ſyſtematic tiſſue of Robertſon.

It would be eaſy, Sir, to adduce other proofs in favour of my poſition, that ſome of the preſent races of North-American Indians are the deſcendants of nations much more populous and poliſhed than themſelves. But the farther inveſtigation of this curious ſubject is not neceſſary at preſent: I reſerve the full diſcuſſion of it for my *Hiſtorical and Philoſophical Inquiry*.

I have already ſaid, that I ſuppoſe the articles which are the ſubject of my letter, were the work of the anceſtors of ſome of the preſent races of Indians; of the ſame people who conſtructed the extenſive earthen fortifications, large conical and other ſhaped mounds, and other ancient works, which are now found to be ſo numerous in many parts of our continent. At what period, or periods, theſe fortifications, &c. were conſtructed; at what periods they fell into ruins, and at what time the articles, which I am conſidering, were buried in the tumulus, in which they were found, are queſtions which I do not pretend to ſolve. Indeed, theſe are queſtions

This was their Manner of Writing; for they had not attained the Uſe of Letters, nor were they acquainted with thoſe Signs or Elements, invented by other Nations, to repreſent Syllables, and make Words viſible; but they explained themſelves by their Pencils, marking down material Things with their own proper Images, and the reſt with Numbers and ſignificant Signs, diſpoſed after ſuch a Manner, that the Number, Sign, and Figure formed the Idea, and fully explained the Meaning; an excellent Invention (which ſhewed their Capacity), like the Hyerogliphicks of the Egyptians, who boaſted of their Wit in that, which was common among the *Indians*, and which the *Mexicans* uſed with ſuch Dexterity, that they had whole Books of this Kind of Characters, and legible Pictures, in which they preſerved the Remembrance of their Antiquities, and left to Poſterity the Annals of their Kings." The Hiſtory of the Conqueſt of Mexico by the Spaniards. Book II. p. 73 and 74. Engliſh Tranſlation. London: 1724.

which, it is probable, we ſhall never be able to ſolve. Time is continually dropping, before our eyes, veils which the hand can never remove. In the moſt intereſting inquiries, whether hiſtorical, philoſophical, or moral, how often are we obliged to pauſe, to meet the clouds before us! Nor ſhould we pauſe without reverence; ſince we have numerous, and thoſe the moſt impreſſive, reaſons for ſuppoſing, that theſe clouds will be diſperſed in a future, and an happier, ſtate.

SECONDLY.

I AM now, Dear Sir, in the ſecond place, to offer my opinions concerning the uſes, or intentions, of the ſeveral articles, which are the ſubject of my letter. And here, I may obſerve, that although on this ſubject there may be ſome uncertainty, yet I think that the articles may, with propriety, be conſidered under the two heads of ornamental, and ſuperſtitious; with the exception, perhaps, of the mica, or iſinglaſs, and the lead-ore, of which I ſhall afterwards give my opinion.

I ſhall firſt ſpeak of the articles which I take to be ornamental, and in the next place of thoſe which I ſuppoſe to have been deſigned for ſome ſuperſtitious purpoſes.

The ornamental articles are thoſe which Mr. Sargent has numbered fig. 1, 2, 3, 4, 5 5, 7 7, 8, 9, and 10 10 (See the plates, with explanations). Of theſe articles it is not neceſſary that I ſhould give any deſcription, as this has already been done by Colonel Sargent, in the accompanying plate. I ſhall only obſerve, that the five ſtones (fig. 1, 2, 3, 4, and 5 5,) are each furniſhed with a groove, repreſented in the plate, by which groove, it is probable,

probable, they were appended to ſome part of the body of the deceaſed.

Perhaps, theſe ſtones were worn, or conſidered, as jewels by the perſon with whom they were buried. Acoſta ſays, that the places in which the dead among the Mexicans were buried, were their gardens, and the courts of their own houſes. Some of the dead bodies were carried to the places of ſacrifices, in the mountains. Others were burnt, and the aſhes were buried in the temples. Along with the bodies, they buried all their apparel, precious ſtones, and jewels. The aſhes of thoſe which were burnt were put into pots, together with the jewels, ſtones, and ear-rings of the dead, however precious they might be.*

Although I have conſidered theſe ſtones as merely intended for ornamental purpoſes, yet, it is not impoſſible, that they may have been uſed for ſuperſtitious purpoſes, like ſome of the articles, which I am afterwards to take notice of. Acoſta ſays, the Mexicans had an idol which was much eſteemed among them. This idol, which was their god of repentance, and of jubilees, and pardons for their ſins, was called Tezcallipuca. It was made of a black, ſhining ſtone, and had ear-rings of gold and ſilver, and through the lower lip a *cryſtal*, half a foot in length. This cryſtal was hollow, and they ſometimes put into it a green feather, and ſometimes an azure one, which made the cryſtal, at one time, reſemble an emerald, and, at another time, a turquois. At the neck, there hung a jewel, ſo large that it covered all the ſtomach: upon the arms, there were bracelets of gold, and at the navel a rich *green* ſtone.†

Clavigero ſays that among the Mexicans, "emeralds were ſo common, that no lord or noble wanted them, and none of them died without having one fixed to his

* Lib. 5. chap. 8. p. 348. Engliſh Tranſlation.
† Lib. 5. chap. 9. p. 353.

lip, that it might ſerve him, as they imagined, inſtead of a heart."*

The copper articles (fig. 9, and 10 10,) deſerve particular attention. I have ſaid, I ſuppoſe that they were ornamental.

It has long been known, that ſome of the American nations manufactured copper into certain articles, or utenſils. Acoſta expreſsly informs us that the Indians (he means the Mexicans and the Peruvians) uſed copper for their arms.† It would appear, from this learned writer, that after the arrival of the Spaniards in America, the practice of employing copper fell into a kind of diſuſe. The Indians buſied themſelves in ſearching for the more precious metals.‡ It does not appear that the Americans ever employed copper as a medium of commerce.

The Mexicans and the Peruvians were acquainted with the art of hardening copper, ſo as to render it a ſubſtitute for iron.

I am not ignorant that one of the ableſt antiquaries of the preſent century has ventured to ſuppoſe, that the art of hardening copper was not known to the Americans with whom the Spaniards became acquainted in the fifteenth and ſixteenth centuries. The Count de Caylus, the gentleman to whom I allude, imagined that the copper hatchet, which he examined, was the work of a people more ancient than the Incas, and that they inhabited the countries of Peru a long time before this race of monarchs. The angry Mr. De Pauw, who is continually differing from almoſt every other writer, and who is eternally committing miſtakes, and hazarding falſe and feeble ſpeculations, treats this opinion of Caylus with contempt.§ If, however, the Berlin philoſopher had re-

* The Hiſtory of Mexico. Vol. I. p. 422.
† Lib. iv. chap. 3. p. 209 and 210. ‡ Ibid.
§ Recherches Philoſophiques ſur les Americains, Tome II.

flected

flected as much as he appears to have read, the opinion of the French antiquary would have demanded more of his attention. He would have ſeen reaſons to conclude, that long before the foundation of the Peruvian monarchy, under the guidance of Manco Capac and his conſort Mama Ocollo, the countries of Peru had been inhabited by a race of people, who were probably more poliſhed than the Peruvians themſelves. Among theſe people, it is not improbable that the uſe of hardened copper was known: but to deny that it was alſo known to the Mexicans, and to the Peruvians, would be to diſpute the veracity of ſome of the moſt reſpectable and learned men who have written on the ſubject of the Americans. Such are Columbus himſelf, Acoſta, Solis, Don Ulloa, Mr. Condamine, and others.*

Hitherto, very few facts have been diſcovered to prove the exiſtence of copper implements among any of the nations of the higher latitudes of North-America; and none have been diſcovered that *unequivocally* prove the exiſtence of the art of hardening copper among theſe nations. But as my inquiries have led me to believe, that the ancient inhabitants of North-America were as poliſhed as the nations of South-America, ſo I cannot well entertain a doubt, that copper inſtruments were in uſe among the northern Americans, and that theſe latter, as well as the former, underſtood the art of hardening this metal. This opinion is rendered more probable, when it is remembered that one of the moſt poliſhed nations of America, I mean the Mexicans, migrated from certain countries ſituated north of the Vermillion-Sea; and that in the progreſs of their migration theſe tribes moved far to-

* The art of hardening copper was known to the Greeks, and to the Romans. It is ſaid to have been preſerved until the taking of Conſtantinople. See *Art des Siéges*, par M. Joly de Maizeroy, p. 4. 1778.

wards

wards the eaſt.* The Mexicans, a number of circumſtances have induced me to believe, were the anceſtors of the nations known by the name of Choktah, Chikkaſah, &c.

When Fernando de Soto was at Quaxule in Florida, he was told that " Northwards he would find the Province of *Chiſca*, where they melted Copper, and another metal of the ſame colour, but much more lively and perfect; that it was a metal that ſeemed to be more precious than Copper, but yet was not made uſe of, becauſe it was ſofter. This relation, continues my author, agreed with what S*oto* was told in *Cutifachiqui*, where we ſaw ſome little Axes of Copper, which they ſaid was mingled with Gold."†

The Peruvian hiſtorian, Garcillaſſo de la Vega, alſo takes particular notice of certain metals, which the Spaniards found at Coſaciqui. From the account of this hiſtorian (who, I am very ſorry to ſay, is not in every inſtance to be confided in), it would ſeem that the Floridians were acquainted with both copper and tin, with which metals, when united, they may have formed their axes. La Vega, as well as the Portugueſe author, expreſsly mentions copper; and it is not unlikely (admitting the truth of the ſtory) that the white metal was tin. It is ſaid to have been very light.‡

* See Clavigero's Hiſtory of Mexico. Vol. I. p. 115 and 116.

† A Relation of the Invaſion and Conqueſt of Florida by the Spaniards, under the command of Fernando de Soto. Written in Portugueſe by a gentleman of the town of Elvas. Engliſh Tranſlation: p. 75. London: 1686.

‡ I have not an opportunity of conſulting the original work in Spaniſh. My information is derived from Richelet's French Tranſlation, which was publiſhed at Leyden, in the year 1731. See *Hiſtoire de la conquete de la Floride: ou Relation de ce qui s' eſt paſſé dans la découverte de ce Païs par Ferdinand de Soto.* I ſhall quote as much of this work as relates immediately to my ſubject. " Durant les courſes d' *Aniaſco,* les autres *Eſpagnols* qui eſperoient tous de faire fortune en *Coſaciqui,* s' informerent avec ſoin des richeſſes qui s' y rencontroient, & le General commanda d' appeller les deux jeunes *Indiens*

The Baron Lahontan, though, in ſome reſpects, a credulous writer, may alſo be adduced as an authority in favour of the uſe of copper articles among ſome of the North-American tribes. This enterpriſing traveller was informed, that the Mozeemlek-nation of Indians, reſiding far weſt of the Miſſiſſipi, "made Stuffs, Copper Axes, and ſeveral other Manufactures." The baron even ſaw in the poſſeſſion of a Mozeemlek-ſlave "a reddiſh ſort of a Copper Medal hanging upon his Neck." He ſays he "had it melted by Mr. *de Ponti's* Gun-ſmith, who underſtood ſomething of Metals; but it became thereupon heavier, and deeper coloured, and withal ſomewhat tractable. I deſired the Slaves to give me a circumſtantial Account of theſe Medals; and accordingly they gave me to underſtand, that they are made by the *Tahuglauk*, who are excellent Artizans, and put a great Value upon ſuch Medals."*

Among the articles which were found in the tumulus, there was a quantity of iſinglaſs, or mica membranacea. It is not eaſy to aſcertain with what view this ſubſtance, ſo common in many parts of North-America, was thought worthy of a place in the tumulus, with the body of the deceaſed. I cannot learn that this mica is held in ſuperſtitious eſteem by any of the preſent Indians of

Indiens que l'on avoit amenez d' *Apalaché*. Il les envoia vers la Dame de *Coſaciqui*, la ſupplier de faire apporter des perles avec de ces métaux blancs & jaunes, dont trafiquoient les Marchands qu' ils avoient ſervis; l'aſſeurant que ſi elle obligeoit les *Eſpagnols* en cela, elle acheveroit de les combler de ſes graces. Cette Dame dépécha auſſi-tôt de ſes ſujets querir de ce métal; & ils raporterent du cuivre d' une couleur tres-dorée, avec de certains aix blancs, comme de l' argent, longs & larges d' une aune, épais de trois à quatre doigts, & toutefois tres-legers. Mais quand on les manioit ils ſe reduiſoient en poudre, à la façon d' une motte de terre fort ſeiche.———Ils (the Spaniards) ſe réjoüirent auſſi de voir que pluſieurs croioient qu' il y eût de l' or dans le cuivre; mais comme ils n' avoient ni eau forte, ni pierre de touche, ils n' en purent faire l' eſſai." Hiſtoire, &c. Tome I. Liv. IV. chap. xiv. p. 270 and 271.

* New Voyages to North-America. Vol. I. p. 125 and 126. London: 1735.

our continent, nor do I learn that it is ever ufed by them. But there are fome reafons for believing that formerly it was an article of ufe, or of fuperftitious regard, among the anceftors of the prefent Indians. My much-lamented friend, Major Jonathan Heart, who was killed in the defeat of General St. Clair's army, on the 4th of November 1791, informs us, that "a quantity of ifing-glafs" was found on the breaft of a fkeleton in one of the tumuli, among the great ancient works, near the junction of the rivers Ohio and Mufkingum.* The Abbé Clavigero fays, that "little looking-glaffes of the ftone Itztli," together with earthen pots, jars, &c, were found among the great ancient works, called Cafe-grandi, in California. This itztli was nothing more than the mica membranacea; and the works, juft mentioned, are faid to have been built by the Mexicans, in their peregrination towards the fouth.†

Among the Mexicans, no ftone was more common than the itztli, "of which, according to Clavigero, there is great abundance in many places of Mexico.‡" The Mexicans applied the itztli to various ufeful, and to fome fuperftitious, purpofes. Of this foffil they made "beautiful looking-glaffes fet with gold, and thofe extremely fharp razors which they fixed in their fwords, and which their barbers made ufe of. They made thofe razors with fuch expedition, that in the fpace of one hour an artificer could finifh more than a hundred."§ They alfo made lancets for bleeding of the itztli,|| knives, and fpears. "After the introduction of the gofpel they made facred ftones of it which were much valued."¶

* See the Columbian Magazine, or Monthly Mifcellany, for May, 1787. p. 427.

† The Hiftory of Mexico. Vol. I. p. 114 and 115.

‡ Ibidem, Vol. I. p. 17.

§ Ibid. p. 421.

|| Ibid. p. 428.

¶ Ibid. p. 17.

Perhaps,

Perhaps, the figures of children and birds, which were found in the tombs of Florida, by Soto, were made of the mica, or itztli. It is true, they are ſaid to have been made of "pearl."

Colonel Sargent ſays that "a few pounds of very rich lead-ore were found in the grave," along with the other articles. It is probable, that this ore was buried with the perſon, merely as a part of his property.

THE articles which I ſuppoſe to have been deſigned for ſuperſtitious purpoſes, are the repreſentation of a bird's head and beak, and the bone engraven with hieroglyphicks, or marks.

I ſhall firſt ſpeak of the bird's head. It is highly probable, that this is only part of an idol, or image, which belonged to the perſon whoſe bones were found, along with the articles, in the tumulus. But this is only a conjecture.

I am not able to aſcertain what bird this idol was deſigned to repreſent. Perhaps, it is not the actual repreſentation of any exiſtent bird, but a mere creation of fancy. I am inclined, however, to think that it is the likeneſs of ſome real ſpecies, though I am ſenſible that the imagination often paints new forms, and in particular that ſuch forms are painted by the minds of individuals, or of nations, when clouded by ſuperſtition. From what will afterwards be ſaid, it will not ſeem altogether unlikely, that the bird's head is part of an image, the body of which may have been the repreſentation of a man, or of ſome other animal, and not of a bird.

There are ſeveral reaſons for ſuppoſing, that this bird's head was an idol, and of courſe deſigned for ſome ſuperſtitious purpoſes. I cannot diſcover with what intention it was formed, and buried with the deceaſed in the tumulus.

lus, without admitting that it had ſome reference to the religious notions of the deceaſed. I know that ſeveral ſpecies of birds are objects of ſuperſtitious veneration, or regard, among many of the preſent races of North-American Indians; and I learn from Acoſta and other writers, that ſome of the Mexican idols had bird's heads.

I have ſaid that many of the Indians have a ſuperſtitious regard or veneration for certain ſpecies of birds. It may not be improper to take notice of ſome of theſe birds.

Among the Lennape, or Delawares, the owl is held in particular veneration, or regard. "I have frequently (ſays Mr. John Heckewelder*) been with them in the woods at night. When the owls have ſet up a noiſe, they, or one of the company, would immediately riſe up, and ſtrew ſome tobacco in the fire. Upon inquiry, I was told, that theſe were a guard over them by night, for they gave them warning, whenever an enemy approached, or was about to ſurround them, eſpecially when at war."†

* M. S. *penes me.*

† The following paſſage is ſo curious, as connected with my ſubject, that I ſhall give it, at length, in the words of Strahlenberg. "There are (ſays this induſtrious author, whoſe work Mr. Pinkerton is pleaſed to call "a prolix and weak work") a Sort of Owls in *Siberia,* not far from *Craſnoyahr,* which are as white as Snow, and as large as Hen-Turkeys; the *Ruſſians* call them *Lün,* and *Uiün;* the *Tartars, Ackia* and *Ackyk;* and the *Kalmucks* name them *Zagan Schub,* and alſo *Zagan Gorochun.* The latter hold them ſacred, and ſuffer no-body to ſhoot them. I never aſked them the Reaſon of it; but, I find, in *Hübner's Political Hiſtory of Tartary,* in an Extract of the Life and Actions of *Cingis-Chan,* Founder of the Monarchy of the *Mungal* and *Kalmuck Tartars,* the following Account: it happened that he, and his ſmall Army, were ſurprized, and put to Flight, by his Enemies; and ſeeking to conceal himſelf in a ſmall Coppice, where he might very eaſily have been diſcovered by the Enemy, an *Owl,* which is a very ſhy Bird, ſettled upon one of the Buſhes, which made his Purſuers deſiſt from looking for him there, not thinking any Man could be hid where this Bird would ſtay: this gave *Cingis-Chan* an Opportunity of making his Eſcape by the Favour of the Night. And ſeeing the Preſervation of his Life was entirely owing to the *Owl,* this Bird was, from that Time, looked upon ſo ſacred, that every one of them wore a Plume of *Owl's* Feathers on his Head. Now ſince in theſe Parts, there are white

The young priests, among the Creek-Indians, generally carry a stuffed owl about them. It is the badge of their profession.

Mr. Beverley speaks of a small bird, which was held in great regard by some of the Indians of Virginia. "They say, this is the Soul of one of their Princes; and on that score, they would not hurt it for the World. But there was once a profane *Indian* in the upper Parts of *James* River, who, after Abundance of Fears and Scruples, was at last brib'd to kill one of them with his Gun; but the *Indians* say, he paid dear for his Presumption, for in a few Days after he was taken away, and never more heard of."*

white *Owls* which are revered by the People, that historical Passage seems to carry along with it the Face of Truth. For this is certain, that the *Kalmucks*, when they celebrate any great Festival, always wear coloured *Owl's* Feathers in their Caps, and the *Woguletzi* have, among other Idols, a wooden *Owl*, to which they fasten the Legs of a natural One." An Historico-Geographical Description of the north and eastern parts of Europe and Asia, &c. p. 434 and 435. London: 1738.

* From another circumstance mentioned by Beverley, it is evident that the above bird must have been greatly esteemed by the Indians. "They (the Indians), says this faithful writer, erect Altars where-ever they have any remarkable occasion; and because their principal Devotion consists in Sacrifice, they have a profound Respect for these Altars. They have one particular Altar, to which, for some mystical Reason, many of their Nations pay an extraordinary Veneration;" of this sort was a crystal cube, which the Indians called *Pawcorance*, "from whence proceeds the great Reverence they have for a small Bird that uses the Woods, and in their Note continually sound that name. This Bird flies alone, and is only heard in the Twilight." *The History of Virginia, in Four Parts*. p. 184 and 185. London: 1722. I take the bird here spoken of to be the Caprimulgus virginianus of Gmelin, the Long-winged-Goatsucker of Pennant. This bird, which is very common in different parts of North-America, is best known by the name of *Whip-poor-Will*, from the supposed resemblance of one of its notes to these words. It is the *We coo lis* of the Delaware-Indians. Long before I knew that this bird was peculiarly regarded by any of our Indians, I used something like the following words, in some manuscript communications, which I made to my friend Mr. Pennant. "There is something so melancholy, and so truly solemn, in the evening call of the Caprimulgus, that I should not be surprised to find that this bird is an object of religious veneration, or regard, with some of our Indians, who are among the number of the most superstitious nations of mankind."

 Mr. Clayton,

The late Captain Carver mentions a bird, called the Wakon-bird, which is held in particular esteem by some of the north-west Indians of our continent. They say,

Mr. Clayton, in a letter to Mr. Catesby, says, "The *Indians* say these Birds were never known till a great massacre was made of their country folks by the *English*, and that they are the souls or departed spirits of the massacred *Indians*. Abundance of people here (in Virginia) look upon them as Birds of ill omen, and are very melancholy if one of them happens to light upon their house, or near their door, and set up his cry (as they will sometimes upon the very threshold); for they verily believe one of the family will die very soon after." *The Natural History of Carolina, &c.* Vol. II. p. 116. London: 1771. In this place, I take an opportunity of correcting an error into which I think my friend Mr. Pennant has fallen, on the subject of our Caprimulgus. After giving a good description of the Short-winged Goatsucker, as he calls it (the Caprimulgus carolinensis of Gmelin), this excellent writer says, "I received this species from Doctor GARDEN of *Charlestown, South Carolina*, where it is called, from one of its notes, *Chuck, Chuck Will's widow;* and in the northern provinces, *Whip-poor-Will*, from the resemblance which another of its notes bears to those words." *Arctic Zoology.* Vol. II. p. 133. London: 1792. But I believe, it is certain that the Chuck-Will's widow and the Whip-poor-Will are two distinct species of Caprimulgus. Their notes, or cries, are very different, as are also their places of residence during the season of incubation, which is the only time they sing. The Chuck-Will's widow dwells only near the sea-coast, and I believe not so far north as the Bay of Chesapeak. Mr. William Bartram informs me, that he never heard this bird farther north than Cape-Fear-River, in North-Carolina. It is seldom met with more than fifty miles from the sea-coast in Carolina and Florida, where they are almost constantly heard from evening to break of day. But the Night Hawk, or Whip-poor-Will, dwells only in the high, hilly, or mountainous countries of West-Florida, Georgia, the two Carolinas, and Virginia; though north of Virginia, it extends even to the sea-coast as far as Canada, and according to Mr. Pennant even still farther north. In these countries, the note of this bird is Whip-poor-Will, during the evening, and moon-light nights until day-break. "I have (says Mr. W. Bartram, M. S. *penes me*) heard this bird for a night or two, in the spring, in Carolina, on the sea-coast, when they were on their journey northward; and they are there in abundance, in the autumn, flying and darting about in the air, on their return southerly to pass the winter; and then they are called Night-Hawks, and are supposed by most people to be a distinct species from the Whip-poor-Will, and the Chuck Will's widow." Mr. Pennant is not alone in the mistake which I have mentioned. A very ingenious friend of mine observed to me, that it was curious that climate should so essentially alter the note of a bird, for, he said, about the latitude of Cape-Fear, the Whip-poor-Will uttered quite a different cry from what it does in the northern states. I have explained the error. The Reverend Mr. Morse (*American Universal Geography*, it is the bird of the Great-Spirit. The Naudoweſſie-Indians, our author informs us, " ſeemed to treat them as if they were of a ſuperior rank to any other of the feathered race."* There can be little doubt that the Wakon-bird is the very ſame bird which Briſſon has figured and deſcribed under the name of *Le Tyran a queue fourchue*, or Tyrannus cauda bifurca.† It is the Muſcicapa Tyrannus of Linnæus,‡ and the Fork-tail-Fly-catcher of Pennant.§

Mr. Roger Williams, in his curious work, entitled *A Key into the Language of America*, ſpeaking of the crow, ſays, " Theſe birds, although they do the corn ſome hurt, yet ſcarce will one native amongſt an hundred kill them; becauſe they have a tradition, that the crow brought them at firſt an Indian grain of corn in one ear, and an Indian or French bean in another, from the great God Cawtantowwit's field in the ſouth-weſt, from whence they hold came all their corn and beans.||"

" Though with all the Indian nations, ſays Adair, the raven is deemed an impure bird, yet they have a kind of ſacred regard to it, whether from the traditional knowledge of Noah's employing it while he was in the ark, or from that bird having fed Elijah in the wilderneſs (as ſome ſuppoſe) cannot be determined; however with our ſuppoſed red Hebrews the name points out an indefatiga-

Geography, &c. Part I. p. 192. Boſton: 1793) ſays, " Bartram conſiders the whip-poor-will and the night-hawk as the ſame bird (Caprimulgus Americanus) but they are well known to be different birds." Mr. Morſe, however, and not Mr. Bartram, is miſtaken.

* Three years Travels through the interior parts of North-America, &c. p. 244 and 245. Philadelphia: 1792.

† Ornithologia, &c. Vol. II. p. 395, 396, 397 and 398. Pariſiis: 1760.

‡ Syſtema Naturæ.

§ Arctic Zoology. Vol. II. p. 76.

|| See Collections of the Maſſachuſetts Hiſtorical Society, for the year 1794. Vol. III. p. 219.

ble,

ble, keen, ſucceſsful warior."* The ſame author tells us, that when the Indian phyſicians, or prieſts, viſit their patients " they invoke the raven, and mimichis croaking voice."† Mr. Pennant, on the authority of Mr. Hutchins, informs us that the " northern *Indians*, on the contrary, deteſt this and all the Crow kind."‡

The very faithful Portugueſe author, whom I have already quoted, informs us, that when Fernando de Soto was at Cutifachiqui in Florida, in the year 1540, the female cazique of the place having obſerved the unlimited appetite of the Spaniards for pearls, " ſhe bid the Governour (Soto) ſend and ſearch in ſome Tombs that were in her Town, telling him that he would find abundance there; and that if he cauſed thoſe alſo of the other Villages to be ſearched, they would furniſh Pearls enough to load all the horſes of the Army. The Tombs of the Town, continues our author, were indeed ſearched, where we got fourteen buſhels of Pearls; and the figures of Children and Birds made alſo of Pearl."§

I have ſaid that ſome of the Mexican idols were furniſhed with bird's heads. I ſhall now mention ſome of theſe idols.

In Cholula, the miſerable inhabitants worſhipped an idol, which was the god of trade or merchandize, the people of this commonwealth being much given to traffick. This idol was called Quetzaalcoalt. It was placed very high in a temple. It had about it gold, ſilver, jewels, very rich feathers, and habits of various colours. It had the form of a man, but the viſage of a little bird, with a red bill, and above a comb full of warts, having ranks of teeth, and the tongue hanging out. Acoſta, who is my

* The Hiſtory of the American Indians, p. 194.

† Ibid. p. 173. ‡ Arctic Zoology. Vol. I. p. 287.

§ A Relation of the Invaſion and Conqueſt of Florida, &c. p. 64 and 65.

authority

authority on this head, has given a more particular defcription of this god of merchandize.*

It may not be improper to mention, in this place, that earthen mounds, or pyramids, fimilar to many of thofe which are found in various parts of our weftern-country, are ftill to be feen in the neighbourhood of Cholula, and are fuppofed by Torquemada, and by Clavigero,† to have been erected by the Toltecas.

The principal idol of Mexico was called Vitziliputzli. It was an image cut out of wood, in the form of a man, fet upon an azure-coloured ftool. Upon its head, it had a rich plume of feathers, like the beak of a fmall bird.‡

" In a high mountain of Achiauhtla, in Mizteca, Benedict Fernandez, a celebrated Dominican miffionary, found a little idol called by the Miztecas the *heart of the people.* It was a very precious emerald, four inches long and two inches broad, upon which was engraved the figure of a bird, and round it that of a little fnake. The Spaniards offered fifteen hundred fequins for it; but the zealous miffionary before all the people, and with great folemnity reduced it to powder."§

The fculptured bone is one of the moft curious of all the articles that were found in the tumulus. I have already faid, that I fuppofe it was intended for fome fuperftitious purpofe. I am unwilling to hazard any farther conjectures concerning it, except this one, viz. that I conceive the marks to be fignificant hieroglyphicks. It is not an human bone.

The ancient inhabitants of Iceland ufed to write upon the bones of fifh, and other animals.

Colonel Sargent, in his letter to me, has mentioned the mouldered condition of the bones which were found

* Lib. 5. chap. 9. p. 354.

† The Hiftory of Mexico. Vol. I. p. 267 and 268.

‡ See Acofta's Naturall and Morall Hiftorie. Lib. 5. chap. 9. p. 352.

§ See Clavigero's Hiftory of Mexico. Vol. I. p. 259.

in

in the tumulus. I have had, however, an opportunity of examining a piece of the thigh-bone, and alſo a piece of the tibia, or principal bone of the leg. They beſpeak a body of the common ſize. I mention this laſt circumſtance, becauſe it has often been aſſerted, that gigantic *human* bones have been found in America. Some of the authors of theſe aſſertions are deſervedly eſteemed for their writings.* There is, certainly, no phyſical impoſſibility in the exiſtence of a race of giants. On the contrary, the general ſcheme of nature, with reſpect to the creation of the ſpecies of animals and vegetables, would lead us to expect a ſpecies of giants belonging to the human kind. At any rate, the exiſtence of giants is not a more improbable circumſtance than the exiſtence of certain ſmall races, ſuch as the Laplanders, who are well known to us. As far, however, as my inquiries have extended, all the human bones that have been found in our ancient tumuli, &c. are bones of the common ſize.

It is a mortifying circumſtance, that in proportion as we extend our acquaintance with the features or manners of rude nations, we are collecting materials for an hiſtory of human ſuperſtitions, and of mental miſeries. If, in the progreſs of our reſearches, we diſcover that in-

* Such are Hernandez, Acoſta, and Clavigero. The laſt of theſe writers mentions large bones being found in "tombs" in Mexico, and conſiders this circumſtance as a proof that they were human bones. *The Hiſtory of Mexico.* Vol. I. p. 84. But this cannot be conſidered as a decided proof. Did not the Egyptians carefully embalm and bury the bodies of the Ibis? The animal to which the large bones mentioned by Clavigero belonged, may have been (like the Ibis with the Egyptians) an object of veneration among the ancient Americans; or they may have been entombed from the ſuppoſition that they were human bones. It is known, that the bones of the Hippopotamus were "ſhewn in ſeveral cities of Greece for the bones of giants." See that curious book, *The Life of Sethos.* Vol. I. p. 73. London: 1732

ſtinct, reaſon, the light of nature, has taught to theſe nations the exiſtence of ſome great, ſuperintending being, the ſource of life and good: if we diſcover among them the unequivocal acknowledgment of a future ſtate of exiſtence, in which the warrior and the hunter, and the virtuous of either ſex, are thought to repoſe from all their cares, and to taſte, in fulneſs, unmixed phyſical pleaſures (the ſavage mind aſks no more), ſtill we diſcover them under the preſſure of that ſuperſtitious fabric, which is founded upon the innumerable follies and weakneſſes of men. In the midſt of the gloom, with which the contemplation of ſuch an abject ſtate of the ſpecies is too well calculated to over-cloud the mind of ſenſibility, we derive much conſolation from the reflection, that *all* nations are capable of improvement; and that in the general order of nature, there ſeems to be nothing to prevent the eſtabliſhment of a more juſt religion over the ſurface of the earth: a religion more juſt, becauſe it teaches us the relations of God to the univerſe; the relations of man to God; and the relations of men to each other.

In the range of human improvement, there is a ſingular point, marked by the hideous ſuperſtition of the people. The ſtate of ſociety to which I allude is that in which the Mexicans were diſcovered, and in which, at a later period, we have known the Natchez, and the people of Bogota. The Mexicans, there can no longer be any doubt, were acquainted with many of thoſe arts which we have ever been accuſtomed to conſider as the arts of a civilized people. Their aſtronomy, their police, their form of government, in ſeveral reſpects ſo ſimilar to that of the United-States, would ſeem to entitle them to a place among nations conſiderably civilized. In all theſe reſpects, they were ſuperior to moſt of the nations around them: they were greatly ſuperior to any of the Indian tribes now known to us. This higher degree of cultivation, however, did not ſecure the Mexicans from the errors and the miſeries of ſuperſtition. On the contrary, they were among the number of the moſt ſuperſtitious nations of mankind, and their innumerable human ſacrifices conſtitute one of the blackeſt features in the character of our ſpecies. I have been led, in the courſe of the preſent and of preceding inquiries, to ſuppoſe, that the ancient American remains are the work of a people nearly in the ſame ſtate of ſociety as the Mexicans; of a people who, like the Mexicans, were extremely ſuperſtitious. If this conjecture be well founded, we ought not to regret that the preſent races of Indians have declined from the ſuperior poliſh of their fore-fathers. We have reaſon to rejoice, that they have thus declined; ſince, if they have loſt ſome of the arts by which they were diſtinguiſhed, they have alſo loſt ſome of the worſt parts of their religion. In the fulneſs of their gratitude to the Great-Spirit, they at preſent content themſelves with offering up the fruits, the grains, and the flowers of their country: or if, for religious purpoſes, they deem it neceſſary to deprive exiſtences of life, they do not make ſacrifices of human beings, but offer up ſome of the wild or domeſticated animals around them. The annual offering of ſome of our ſouthern tribes is the earlieſt ripened maize of their country: but the anceſtors of theſe very tribes are known to have made, at the ſame period, offerings of their children.

Avarice, or the deſires for gain, ſometimes ſtimulates men to exertions, which prove beneficial to literature, or to the ſciences. It will be well if this deteſtable paſſion can, at any time, be turned to the better intereſts of men. In this country, as yet, the energies of ſcience are not great. The hiſtory of the aboriginal Americans, in particular, has been neglected; conſiderable tribes have mouldered away, and of their phyſical and moral features,

of

of their traditions, and languages, we hardly know any thing that is entitled to the name of certainty. But, with respect to all these subjects, much may still be done, and something may be done where we have least expected it. Let us open the tombs of the ancient Americans. In these dark abodes, the last asylums of man on this globe, we may discover materials that will enable us to throw some light upon the ancient history of the Americans. If we are not sufficiently animated by the love of science, let us remember, that in the tombs of the Mexicans and Peruvians, the Spaniards have discovered treasures of gold, of silver, and of precious stones; and that even in the tombs in Florida, valuable pearls are said to have been found. I think, there can be little doubt that the opening of the North-American tumuli will reward the labourers with valuable spoils.

I have taken up a good deal of your time; more than I contemplated when I undertook to give yon my sentiments on the subject of the tumular articles. I have been extremely desirous to afford you some amusement, and, if possible, some information.

I am, with the greatest respect,
Reverend and Dear Sir,
Your Humble and Obedient Servant,
And Affectionate Friend,

BENJAMIN SMITH BARTON.

To the Rev. Dr. Joseph Priestley.

N°. XXIV.

Barometrical Meaſurement of the Blue-Ridge, Warm-Spring, and Alleghany Mountains, in Virginia, taken in the Summer of the year 1791.

Mount Pleaſant on Schuylkill, Nov. 13, 1795.

DEAR SIR,

Read Nov. 20, 1795. THE encloſed Journal would have been long ſince preſented to the Philoſophical Society if I had been ſatisfied as to the accuracy of barometrical calculations, when applied to the meaſuring of heights in this climate; but finding much variation in repeated experiments made at a known height,* in winter, ſpring and ſummer, I grew diſcouraged, and my labours would have paſſed into oblivion if the advice of our worthy Preſident and yourſelf had not induced me to preſent my Journal as it is, diſregarding the errors incident to local circumſtances and ſmall elevations, which may not affect the principle of barometrical meaſurement on a more extended ſcale.

I have uſed the table you favoured me with, making proportionate calculations for every deviation from 30 inches of the mercury; this I have compared with the calculated height of the Andes, in Don Ulloa's Voyages, and the aſcent of Mr. Charles in the balloon at Paris, deſcribed by M. Faujas de St. Fond; the reſult of theſe compariſons has ſo well confirmed the accuracy of the table, that I have no doubt of my calculations being proportionate to thoſe of Europe and South-America.

* The higheſt acceſſible part of Chriſt's Church Steeple, inſide, is 166 feet from the ground. By barometrical meaſurement it varied from 105, to 243 feet.

4 I beg

I beg leave however to mention ſome circumſtances which, as far as I am informed, ſeem peculiar to this country.

1. The atmoſpherical changes in Europe, generally cauſe the mercury in the barometer to riſe and fall three inches, in the courſe of a year.

2. In Williamſburg (Virginia) the greateſt change in the courſe of a year has been obſerved to be 1.86.

2. At Monticello, about 20 miles eaſt of the Blue Ridge, and about 500 feet high, the greateſt variation in nine months was* 1.21: though the relative changes were ſimultaneous with thoſe at Williamſburg.

4. During 11 days reſidence at Staunton (1055 feet high) and 29 at the Red Springs (1512 feet high) the barometer at the former place did not vary more than 0.40, and at the latter only 0.20, while the thermometer varied more than 30 degrees, and the weather was at the extremes of clear and dry, and cloudy with heavy rains.

It ſeems therefore that the barometer is leſs ſuſceptible of change in Virginia than in England, and ſtill leſs as you aſcend among the mountains; and it may not be thought improbable that a regular meteorological journal kept at the ſame time in ſeveral places during a year, would ſhow a ſort of gradation in theſe changes.

The lower part of the atmoſphere, in addition to its own gravity, ſupports all above it; its moiſture is liable to expand prodigiouſly from the rays of the ſun reflected in every direction, and other caſes of heat; and to be very ſuddenly condenſed by cold: The winds, impeded by many obſtacles, ſuch as trees, rocks, and eminences of land, are generally irregular and violent, like water ruſhing over a rude and rapid deſcent; while, in a ſuperior region the air glides ſmoothly along like a current in the ocean. It

* Mr. Jefferſon's Notes, p. 83 and 85.

ſeems from theſe cauſes that inaccuracies are moſt likely to occur in the firſt ſtage of aſcent: Hence I am inclined to think that the Blue Ridge is not eſtimated high enough. Theſe objections do not occur in ſo great a degree in the high country.

Mr. Jefferſon ſuppoſes the Peaks of Otter to be about 4000 feet high. This place is about 60 miles S. W. of that where I croſſed, I had therefore no opportunity of ſeeing it.

Pleaſe to obſerve that my undertaking theſe experiments was incidental to my having occaſion to make the journey: had it been a preconcerted plan, more accurate and more minute obſervations might have been expected. If it ſhould be found intereſting enough to deſerve the notice of the Society, I ſhall be abundantly gratified.

I am, with great Reſpect and Eſteem,

Your moſt Obedient Servant.

JONATHAN WILLIAMS.

To Mr. R. Patterſon.

P. S. If on examination there ſhould be reaſon to believe theſe heights tolerably accurate, the height of Richmond from the ſea ſhould be added to each elevation.

A Me-

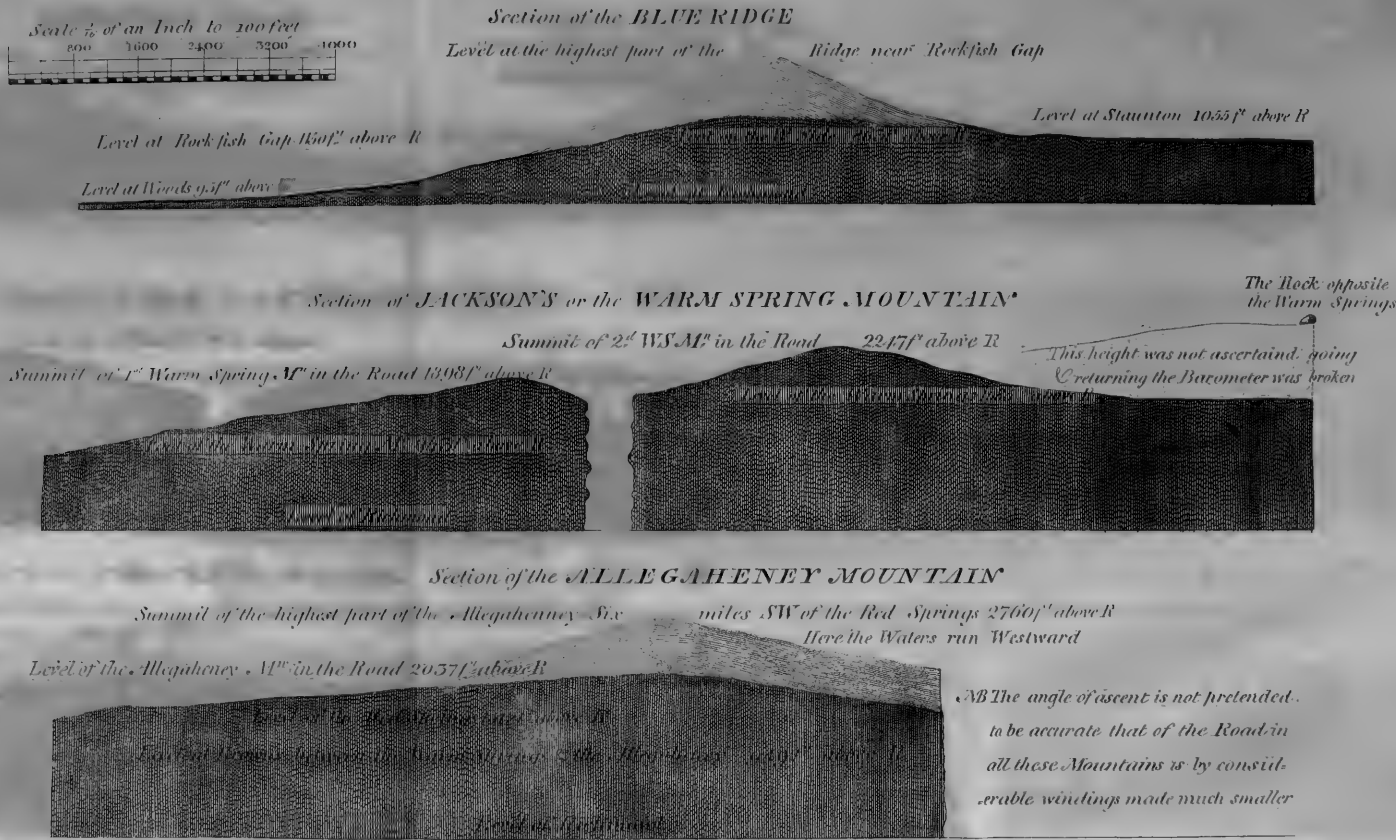
A Meteorological Journal, made with a View to discover the Height of the Country, at various Places, from Richmond, to the Alleghany Mountains in Bottetourt, Virginia.—1791.

Places.	Dates.	Time of Day.	Ther. Faren.	Rarometer. Inches.	Rarometer. 100ths.	Weather.	Dist. in Miles.	Diff. of Barometer. falling.	Diff. of Barometer. rising	Ascent in feet.	Descent in feet.	Height above Richmond.
Richmond	June 26	Sun rise	70	29	80	Fair	0					
Allen's		10 A. M.	78	29	80	Ditto	18					
Leake's		Sun set	75	29	60	Showers	32					
Ditto	27	Sun rise	66	29	60	Fair	Do.					
Payne's		Noon	75	29	60		40					
Charlotteville	28	Noon	78	29	80	Do.	80					
Wood's	29	Sun rise	65	29	70	Do.	90	0.10		95		95
a Rockfish Gap in the Blue Ridge		10 A. M.	76	28	60	Do.	104	1.10		1055		1150
Mountain Top		2 P. M.	79	27	90	Do.		070		672		1822
The Gap again		3 P. M.	79	28	60	Do.						
Ditto	30	Sun rise	70	28	40	Rain						
Foot of the Blue Ridge Westward		6 A. M.	75	28	90	Do.	105		1.00		959	863
b Staunton		Sun set	73	28	70	Cloudy	120	0.20		192		1055
Ditto	July 3	Sun rise	55	29	10	Fair						
Eckhard's	12	5 A. M.	60	28	92	Do.	128					
Alexander's		5 P. M.	66	28	50	Gust	140					
Huggard's.	13	5 A. M.	61	28	62	Fair	146					

A Meteorological Journal continued. 1791.

Places.	Dates.	Time of Day.	Therm. Faren.	Barometer. Inches.	Barometer. 100ths	Weather.	Dist. in Miles.	Diff. of Barometer Falling.	Diff. of Barometer Rising.	Ascent in Feet.	Descent in Feet.	Height above Richmond.
M. Clung's	July 13	9 P. M.	73	28	80	Fair	163		0.12		112	943
Foot of Mountain	14	5 A. M.	63	28	82		166		0.12		112	943
c Top of 1st Mountain		11 A. M.	82	27	80	Ditto	167	1.02		955		1898
d Top of 2d do.		Noon	77	27	44	Do.	168	0.36		349		2247
Foot of do.		1 P. M.	77	28	03	Showers	169		0.59		562	1685
Morris's		8 P. M.	61	28	67	Fair	178		0.64		614	1071
Brown's	15	8 A. M.	67	28	80	Foggy	201		0.13		122	949
Red Springs	16	9 P. M.	66	28	20	Rain	217	0.60		563		1512
e Ditto	24	6 A. M.	66	28	30	Fair	Do.					
Ditto	26	6 A. M.	65	28	11	Hardrain	Do.					
Alleghaney Mountain highest part of the Road	31	Noon	80	27	64	Fair	223	0.56		525		2037
f Top of the Ridge		5 P. M.	76	26	86	Do.	224	0.78		723		2760
g Red Springs again		8 P. M.	77	28	24	Do.	217					
Ditto	Aug. 14	6 A. M.	61	28	28	Do.						

Notes.



Height of VIRGINIA MOUNTAINS by Barometrical meafurement by Jon.n Williams Jun.r

Notes.

a. From the foot of the Blue-Ridge to the Gap is called two miles. From the Gap to the foot on the weſtern ſide it is called one mile.

b. During eleven days reſidence at Staunton and in 33 obſervations, the mercury in the barometer did not riſe above 29.10, nor fall below 28.70. The thermometer varied from 55 to 82, and the weather was ſometimes very clear and dry, and ſometimes very cloudy with hard rain.

c. Theſe are called Warm-Spring Mountains, the Warm Springs being near the foot at the weſtern ſide.

d. This is not the top of the Ridge; I could not go up at this time, and when I returned I had not my barometer: By ocular obſervation it appeared to me nearly, if not quite, as high above the road on the firſt mountain as the top of the Blue Ridge above the Gap.

e. During 29 days reſidence at the Red Springs, the mercury in the barometer varied only 0.19; the thermometer varied 31 degrees and the weather was at the two extremes of wet and dry.

POSTSCRIPT.

WHILE the foregoing was under the conſideration of the committee of ſelection and publication, I ſent a tranſcript to Mr. Jefferſon, requeſting him to furniſh me with ſuch facts on this ſubject as had been eſtabliſhed by experiment. In return he favoured me with the following anſwer.

Monticello, July 3d. 1796.

"DEAR SIR,

"I EXAMINED with great ſatisfaction your barometrical eſtimate of the heights of our mountains, and with the more as they corroborated conjectures on this ſubject, which I had made before. My eſtimates had made them a little higher than yours, (I ſpeak of the Blue Ridge). Meaſuring with a very nice inſtrument, the angle ſubtended vertically by the higheſt mountain of the Blue Ridge oppoſite to my own houſe, a diſtance of about 18 miles ſouth-weſtward, I made the height about 2000 feet, as well as I can remember, for I can no longer find the notes I then made. You make the ſouth ſide of the mountain near Rock-fiſh Gap 1727 feet above Wood's. You make the other ſide of the mountain 768 feet. Mr. Thomas Lewis deceaſed, an accurate man, with a good Quadrant made the north ſide of the higheſt mountain oppoſite to my houſe ſomething more (I think) than 1000 feet. But the mountain eſtimated by him and myſelf is probably higher than that next Rock-fiſh Gap. I do not remember from what principles I eſtimated the peaks of Otter at 4000 feet, but ſome late obſervations of Judge Tucker's coincided very nearly with my eſtimate. Your meaſures confirm another opinion of mine, that the Blue Ridge on its ſouth ſide is the higheſt in our country, compared with its baſe. I think your obſervations on theſe mountains well worthy of being publiſhed, and I hope you will not ſcruple to let them be communicated to the world.

I am, &c.

(Signed) TH: JEFFERSON."

When

When it is conſidered that in meaſuring a height by taking the vertical angle, the refraction of the rays of light in the atmoſphere muſt affect the appearance of the objects, it will occur that this mode muſt be ſubject to ſame inaccuracy; and as this refraction generally tends to *increaſe* the apparent height, a reaſonable allowance on the experiments mentioned by Mr. Jefferſon would probably bring them down to the barometrical meaſurement. This obſervation is beautifully illuſtrated by Mr. Jefferſon's account of a phenomenon reſembling, in ſome meaſure, an appearance which ſeamen call *looming*;* and which (ſo far as relates to apparent ſize at leaſt) may be accounted for by refraction. On the other hand barometrical meaſurement, is probably inaccurate in the lower ſtages of the atmoſphere; but this tends to *leſſen* the apparent height: The truth may therefore lie between.

I am, as before,
Dear Sir, ſincerely yours,
JONATHAN WILLIAMS.

Philadelphia, Aug. 18, 1796.

To Mr. R. Patterſon.

* Jefferſon's Notes, page 89.

N°. XXV.

Miſcellaneous Obſervations relative to the Weſtern Parts of Pennſylvania, particularly thoſe in the Neighbourhood of Lake Erie. By ANDREW ELLICOTT.

DEAR SIR,

Read Dec. 4, 1795. I TAKE the liberty of tranſmitting to you the following miſcellaneous obſervations, collected from my notes, relative to Lake Erie, and the Weſtern Country, the peruſal of which I flatter myſelf will not be unſatisfactory or unintereſting.

The ſituation of this lake is already well known, and therefore a particular topographical deſcription will here be unneceſſary; but a variety of phenomena which attend it, merit a more minute conſideration, and cannot fail to engage the attention of the philoſopher; phenomena which in all probability are common to all large lakes of freſh water.

In the ſummer ſeaſon fogs are ſeldom obſerved on the margin of the lake. The three ſummer months that I reſided at Preſqu' Iſle, no fogs were ſeen during the whole time. The horizon was generally clear, and the ſtars ſhone with remarkable luſtre. The moſt common winds here generally reſemble the ſea and land breezes, in the Weſt Indies. From the end of ſpring till the beginning of autumn, they blow, except at the time of ſtorms, from the lake upon the land during great part of the day, and from the land upon the lake during the night: The change generally takes place between the hours of ſeven and ten in the morning, and about the ſetting of the ſun in the evening. Theſe breezes, alternately blowing in oppoſite directions, render thoſe ſituations contiguous to the

the lake extremely pleaſant during the heat of the ſummer months, and have moſt probably a very ſalutary influence upon the atmoſphere.

A ſtrong eaſterly wind will occaſion a conſiderable depreſſion, and a ſtrong weſterly wind a conſiderable ſwell of the waters in Preſqu' Iſle Bay. In the former caſe, a portion of the water is driven towards the upper end, and in the latter, towards the lower end of the lake. To theſe cauſes we are to attribute thoſe ebbings, and flowings, which have ſo frequently been miſtaken for regular tides: for a little reflection will convince one, that the moon can have no ſenſible effect upon the waters of the lakes. When the wind ceaſes the waters return to reſtore the equilibrium, and an undulation will be viſible for ſeveral days after thoſe ſtorms, and appears to be but ſlightly affected by the alternate breezes already mentioned.

In the weſtern country, and eſpecially in the neighbourhood of the lakes, dews are very heavy. On the Ohio and Allegany rivers, and their numerous branches, fogs are very common, and of remarkable denſity; they do not however appear to contain any portion of thoſe noxious miaſmata, which are ſo frequently combined with the fogs on the eaſtern ſide of the mountains; nay the inhabitants of Pittſburgh conſider them as poſſeſſed of ſalubrious qualities. From a variety of obſervations I am convinced that the atmoſphere in the weſtern country, and particularly in the vicinity of the lakes, contains a greater quantity of moiſture than in the middle Atlantic ſtates. The wooden works which contained my inſtruments were always uncommonly ſwelled, and frequently very much injured in that country, though conſtantly defended from the rain, and occaſionally expoſed to the ſun. The ivory and wood of my ſectors with braſs joints, always expanded above the metal; this expanſion was

not

not ſudden, but effected by ſlow degrees. Whether this exceſs of moiſture ariſes from the extenſive foreſts which conſtantly preſerve the earth in a ſtate of humidity or from more permanent cauſes, future obſervations muſt determine.

Iron is here more ſuſceptible of ruſt, and braſs ſooner tarniſhed than in the Atlantic ſtates; but this ſuſceptibility of ruſt I obſerved to be greater in the foreſts than in thoſe parts of the country that had been cleared for cultivation, and from theſe circumſtances the probable cauſe is aſcertained.

The ſouthern ſhores of Lake Erie are generally high; in many places they are perpendicular, and various ſtrata of ſtone are conſiderably elevated above the ſurface of the water. The ſtreams which diſcharge themſelves into the lake over theſe ſtrata form a great variety of caſcades of a romantic appearance, which increaſe the beauty of the country, and muſt at ſome future period enhance the value of the lands.

At the lower end of the lake, and for ſome diſtance up it, theſe ſtrata conſiſt of lime-ſtone intermixed with flint and marine petrifactions, but the other ſtrata are generally ſlate and excellent freeſtone. About Preſqu' Iſle there is but little lime-ſtone to be ſeen, it lies in detached pieces, and is likewiſe interſperſed with flint and marine petrifactions.

In a large extent of country on the weſtern ſide of the Allegany Mountain, the ſtrata of ſtone are horizontally diſpoſed, except in ſome places where that poſition has been changed by the undermining of creeks and rivers. In theſe places where the ſtrata have been deprived of their ſupport, they have fallen from their original poſitions, and therefore deviate from the general rule. This law of nature is eſtabliſhed on the ſouth ſide of Lake Erie, but how far weſt of the mountains the ſame obtains,

tains, has never yet been afcertained. The horizontal pofition of the ftrata on that lake has a pleafing effect; the fofter lamina are worn away by the beating of the waves, the harder remain projected, and at a diftance refemble wainfcoting or mouldings.

From the horizontal difpofition of thefe ftrata the following conclufions may be deduced; firft, that the country has never been difturbed by thofe terrible convulfions which a great part of this globe muft have experienced at fome remote period of antiquity; and fecondly, that thofe naturalifts are deceived, who fuppofe that the ftrata were originally parallel to the axis of the earth.

Before I conclude my obfervations on this fubject, I fhall take the liberty of adding an account of the falls of Niagara which are in fome meafure connected with the horizontal difpofition of the ftrata in the Weftern and North Weftern Country.

This ftupendous cataract of water infinitely excels all other natural curiofities of the country, and exhibits a fpectacle fcarce equalled in grandeur by any object in the phyfical world. Lake Erie is fituated upon one of thofe horizontal ftrata in a region elevated about three hundred feet above the country which contains Lake Ontario. The defcent which feparates the two countries, is in fome places almoft perpendicular, and the immenfe declivity formed by thefe ftrata occafions both the cataract of Niagara and the great falls of Chenefeco. This remarkable precipice generally runs in a fouth-weftern direction from a place near the Bay of Toronto on the northern fide of Ontario, round the weftern angle of the lake; from thence it continues its courfe generally in an eaftern direction, croffing the ftrait of Niagara and the Chenefeco river, till it is loft in the country towards the Seneca Lake.

The waters of this cataract formerly fell from the northern fide of the flope, near the landing place; but the

the action of ſuch a tremendous column of water falling from ſuch an eminence, through a long ſucceſſion of ages, has worn away the ſolid ſtone for the diſtance of ſeven miles, and formed an immenſe chaſm which cannot be approached without horror. In aſcending the road from the landing to Fort Slauſer the eye is continually engaged in the contemplation of the awful, and romantic ſcenes which preſent themſelves, till the tranſcendent magnificence of the falls is diſplayed to view, the imagination is then forcibly arreſted, and the ſpectator is loſt in ſilent admiration! down this awful chaſm, the waters are precipitated with amazing velocity after they make the great pitch, and ſuch a vaſt torrent of falling water communicates a tremulous motion to the earth, which is ſenſibly felt for ſome poles round, and produces a ſound which is frequently heard at the diſtance of twenty miles. Many wild beaſts that attempt to croſs the rapids above this great cataract, are deſtroyed; and if geeſe or ducks inadvertently alight in theſe rapids, they are incapable of riſing upon the wing again, and are hurried on to inevitable deſtruction.

The great height of the banks renders the deſcent into the chaſm extremely difficult; but a perſon after having deſcended may eaſily proceed to the baſe of the falls, and a number of perſons may walk in perfect ſafety a conſiderable diſtance between the precipice and the deſcending torrent, where converſation is not much interrupted by the noiſe, which is not ſo great here as at ſome diſtance. A vapour or ſpray of conſiderable denſity, reſembling a cloud, continually aſcends, in which a rainbow is always ſeen when the ſun ſhines, and the poſition of the ſpectator is favourable. In the winter this ſpray attaches itſelf to the trees where it is congealed in ſuch quantities as to diveſt them of their ſmaller branches, and produces a moſt beautiful chryſtalline appearance;

 a cira circumſtance which attends the falls of Cheneſeco, as well as thoſe of Niagara.

A ſingular appearance is obſerved at theſe falls, which has never perhaps been noticed by any writer. Immediately below the great pitch a commixture of foam and water is puffed up in ſpherical figures, about the ſize of a common haycock. They burſt at the top, and diſcharge a column of ſpray to a prodigious height; they then ſubſide, and are ſucceeded by others which exhibit the ſame appearances. Theſe ſpherical forms are moſt conſpicuous about midway between the weſt ſide of the ſtreight, and the iſland which divides the falls, and where the largeſt column of water deſcends. This appearance is produced by the aſcenſion of the air, which is carried down by the column of falling water in great quantities to the bed of the river.

The river at the falls is about ſeven hundred and forty three yards wide, and the perpendicular pitch is one hundred and fifty feet in height. In the laſt half mile immediately above the falls the deſcent of the water is fifty eight feet; but the difficulty which would attend the buſineſs, prevented me from attempting to level the rapids in the chaſm below; though from conjecture, I concluded that the waters muſt deſcend at leaſt ſixty five feet, and from theſe reſults it appears that the water falls about two hundred and ſeventy three feet, in the diſtance of about ſeven miles and an half.

I am, Sir, with reſpect
Your friend,
ANDREW ELLICOTT.

To Robert Patterſon.

Omitted at the close of No. VI. *On Aberration.*

THE foregoing projection for aberration in right ascension and declination, and the rules for the application of the equations in right ascension, are only to be considered as strictly general for stars whose latitudes and declinations are both north. For a star whose declination is north and latitude south, or declination south, and latitude north; in place of beginning with the longitude of the star, begin with its opposite, that is with a point six signs distant. In the first case the signs must be laid off and numbered in a contrary direction to those in the projection which was used for β Medusæ whose latitude, and declination, were both north: and the aberration in right ascension will be positive when a point three signs behind the sun's place, falls on the right-hand side of the meridian of the star, the point of right ascension being held from you. In the second case the signs must be laid off and numbered in the same progressive manner as in the projection for β Medusæ, and the same rules are to be observed in the application of the equations: But in both those cases, the longitude of the star, and its point in right ascension, will be situated on contrary sides of a diameter at right angles to the meridian of the star. When the declination, and latitude of the star, are both south, the projection may be made as if they were both north, but the signs must then be laid off, and numbered in a contrary direction, and the contrary rule is to be observed in the application of the aberration in right ascension.

Observations

N°. XXVI.

Observations made on the Old French Landing at Presqu' Isle, to determine the Latitude of the Town of Erie. In a Letter from ANDREW ELLICOTT, *to* ROBERT PATTERSON *Secretary of the Society.*

Philadelphia, Sept. 15th, 1796.

DEAR SIR,

Read Sept. 16, 1796.

THE following observations were made on the old French landing at Presqu' Isle to determine the latitude of the Town of Erie on Lake Erie. The instrument I used was a small zenith sector of 20 inches radius.

Plane of the Sector East, 1795.

September 1st	Zenith distance	α Lyræ	3° 29 49″ S
	Do.	δ Cygni	2 32 28 N
4	Do.	Capella	3 39 53 N
	Do.	α Lyræ	3 29 46 S
	Do.	δ Cygni	2 32 34 N
5	Do.	Capella	3 39 50 N
	Do.	α Lyræ	3 29 44 S
	Do.	δ Cygni	2 32 22 N
6	Do.	Capella	3 39 47 N
		Plane of the Sector West.	
	Do.	α Lyræ	3° 33′ 53 S
	Do.	δ Cygni	2 28 8 N
9	Do.	Capella	3 35 35 N
	Do.	α Lyræ	3 34 1 S
	Do.	δ Cygni	2 28 7 N
10	Do.	Capella	3 35 44 N
	Do.	α Lyræ	3 34 1.5 S
	Do.	δ Cygni	2 28 0 N

From these observations the latitude of the landing appears to be 42° 8′ 17″ N. and the beginning of the Town being about

309 feet south from the landing, the latitude of the beginning of the Town must be 42° 8′ 14 N.—The magnetic variation at the Town of Erie when the foregoing observations were made, was 0° 43′ East.

I am, Sir, with respect
Yours, &c.
ANDREW ELLICOTT.

To Mr. Robert Patterson.

No. XXVII.

Hints relative to the Stimulant Effects of Camphor upon Vegetables. By BENJAMIN SMITH BARTON, M. D.

Read Sept. 16, 1796.

THE stimulant effects of camphor upon the human and some other animal bodies, are well known: but I have not met with any experiments concerning the influence of this singular substance upon living vegetables. Perhaps, the following loose hints on this subject may not be entirely unworthy of the notice of the Philosophical Society. I shall not fail to pursue the inquiry, at a future period.

On the 25th of last May, I put a piece of the woody stem of the Tulip-tree (Liriodendron Tulipifera) with one flower and two leaves, into eight ounces of water, with which I had triturated, for some time, one scruple of good camphor. The branch, which I speak of, was taken out of a pot of water, which contained several other flowers of the same plant, all, to appearance, in the same state. In a short time, I was struck with an unusually lively appearance of the flower in the camphor, whilst the others, although they had the benefit of a larger quantity of water, were sensibly drooping. The appearances exhibited by my invigorated plant were the following:

following: viz. the two leaves became confiderably elevated upon their footftalks; the flower expanded more than I had ever feen it in any inftance; the ftamina, or chives, receded from the piftillum; the three leaves of the calix, or flower-cup, were remarkably reflected back, and became extremely rigid, and elaftic. The internal furface of the petals of the flower perfpired confiderably, though I could not difcover a fimilar perfpiration from any of the flowers of the fame plant, in the fame room, and temperature. I did not perceive any perfpiration from the leaves of my camphorated plant.

At the very time of making this experiment, I was engaged in delivering, to my clafs, lectures on the *Irritability* of vegetables. I, therefore, took an opportunity of fhewing to the gentlemen, the plant which I have juft mentioned. Although it was not, at this time, fo lively as it had been before, they all agreed, that it exhibited remarkable appearances of life, health, and vigour. To me thefe appearances were very ftriking. I could not help comparing them to the effects of a certain quantity of ardent fpirits, or of opium, upon the human conftitution.

My camphorated plant continued in a very invigorated ftate for two whole days: after which it began to droop. The leaves drooped and decayed fooner than the flower. The other flowers and leaves of the tulip-tree, which were left in fimple water, did not live more than half the time of that in water with camphor.

Neither myfelf nor feveral other perfons were able to difcover the leaft odour of camphor in any part of the branch, except what was immerfed in the fluid. This circumftance feems to render it probable, that the camphor was not abforbed by the plant, but that it exerted its remarkable effects entirely through the folids to which it was immediately applied.

I have

I have made ſeveral other experiments relative to the effects of camphor upon plants. But I do not think it neceſſary to be particular as to the individual appearances produced by this ſubſtance. In every inſtance, it was evident, that the camphor acted as a powerful and wholeſome ſtimulant upon the plants. Thus a ſtalk of yellow Iris, with one expanded flower, was taken out of a vial of water, in which it had been placed, for upwards of a day. The flower had begun to droop. A very few minutes after I had placed it in a vial, of the ſame ſize, containing a few grains of camphor, the flower began to revive, and continued in a vigorous ſtate for many hours.

As camphor is but very ſparingly ſoluble in water, it is obvious to conclude, that the ſtimulant effects which I have obſerved were produced by a very ſmall part of the quantity which, in my experiments, was triturated with the water.

It is evident, from what I have ſeen, and related, that camphor exerts a conſiderable ſtimulant effect upon plants; greater, I think, than any other ſubſtance I am acquainted with. This diſcovery might induce us to make trials with camphor, as a manure, if it were not certain that the expence of the manure will prevent us from making the experiment upon a large ſcale. But may we not apply the camphor, in the manner I have mentioned, to uſeful purpoſes? A few grains of camphor, acting as a cordial, will revive a drooping plant, will increaſe its beauty, and prolong its exiſtence. In the eye of the floriſt, theſe are objects of no mean importance: why, then, ſhould we not chearfully lend him our aſſiſtance, ſince in an innocent and amiable purſuit, he robs no one of his happineſs, and increaſes his own?

I have

I have made ſome experiments with the view to form a comparative eſtimate of the wholeſome ſtimulating effects of camphor and of nitre upon plants put in water. The reſult of my experiments favours the idea, that camphor is a more wholeſome ſtimulant than nitre. Unleſs the doſe of this laſt ſubſtance is managed with very great care, it is apt to produce weakneſs, languor, and death. Even in that hardy evergreen, our Broad-Leaved laurel (Kalmia latifolia), I found that a few grains of nitre produced, in a ſhort time, not only a loſs of the green colour, but an appearance, which I would compare to that of ſphacelus, or mortification, in animals.

N°. XXVIII.

Supplementum Indicis Floræ Lancaſtrienſis. Auctore HENRICO MUHLENBERG. Communicated by Dr. BARTON.

Read Sept. 16, 1796.

CLASSIS 1.

Callitriche.
autumnalis.

CLASSIS 2.

Veronica.
ſcutellata.

CLASSIS 3.

Schoenus.
Mariſcus.

Cyperus.
ſpathaceus.
flaveſcens.
eſculentus.

Scirpus.
ſquarroſus.

Eriophorum.
virginicum.
polyſtachion.

Panicum.
geniculatum. N. S.
roſtratum.

roſtratum. N. S.
agroſtoides. N. S.
piloſum. N. S.
Phleum.
nodoſum.
Agroſtis.
ſcabra. N. S.
laxa. N. S.
ſobolifera. N. S.
clandeſtina. N. S.
Poa.
ſtolonifera.
rubra. N. S.

CLASSIS 4.

Potamogeton.
nervoſum. N. S.

CLASSIS 5.

Lyſimachia.
ſtricta.
quadrifolia.
Hydrophyllum.
canadenſe.
Polemonium.
dubium.
Hydrocotyle.
bipinnata. N. S.

CLASSIS 6.

Allium.
urſinum.
Lilium.
ſuperbum.
Convallaria.
ſtellata.
Juncus.
vernalis.
filiformis.

CLASSIS 10.

Arenaria.
lateriflora.
Stellaria.
uliginoſa.

CLASSIS 12.

Prunus.
nana.
Potentilla.
norvegica.
Spiraea.
alba.

CLASSIS 13.

Actaea.
ſpicata.
Anemone.
pennſilvanica.
Ranunculus.
bulboſus.

CLASSIS 14.

Melampyrum.
pratenſe.

CLASSIS 17.

Polygala.
cruciata.

CLASSIS 19.

Carduus.
altiſſimus.
Bidens.
frondoſa.
Gnaphalium.
uliginoſum.
Helianthus.
frondoſus.
Polymnia.
Canadenſis.
Silphium.
trifoliatum.

CLASSIS 20.

Ophrys.
lilifolia.
ſpiralis.
Limodorum.
tuberoſum.

CLASSIS 21.

Chara.
vulgaris.
flexilis.
Lemna.
triſulca.
Quercus.
illicifolia. Wangenheim.
ſtellata.
caſtanea. N. S.

CLASSIS 22.

Salix.
alba.
acuminata.
cuſpidata. N. S.
Acnida.
cannabina.

CLASSIS 23.

Andropogon.
Virginicum.
purpuraſcens. Clayton 602.
Parietaria.
officinalis ?

CLASSIS 24.

Aſplenium.
ruta muraria.
Polypodium.
novaboracenſe.
Porella.
pinnata. Dillen. 68, 1.
Phaſcum.
cuſpidatum. Dill. 32, 11.
ſubulatum. Hedwig, 1, 35.
criſpum. Hedwig, 1, 9.
patens. Hedwig, Crypt. 1, 10.
Fontinalis.
antipyretica. Dill. 33, 1.
Buxbaumia.
folioſa. Dill. 32, 13.
Mnium.
hygrometricum. Dill. 52, 75.

mega-

megapolitanum. Hedwig, 1, 31.
heteromallum. Hedw. 1, 26.
triquetrum. Hedw. 1, 21, 22.
fontanum. Dill. 44, 2.
cespiticium. Dill. 50, 66.
Bryum.
nutans. Hedwig, 1, t. 4.
mnioides. Hedwig, 1, t. 3.
capillaceum. Hedwig, 11. t. 26.
ciliatum. Dill. 35, 5.
pusillum. Hedwig, 1, 28.
murale. Dill. 45, 14.
apocarpum. Dill. 32, 4.
viridulum. Hedwig, 11, 5, B.
unguiculatum. Hedwig, 1, 23.
imberbe. Hedwig, 1, 24.
heteroptilum. Dill. 45, 11.
argenteum. Dill. 50, 62.
Hypnum.
taxifolium. Dill. 34, 2.
denticulatum. Dill. 34, 5.
bryoides. Dill. 34, 1.
polyanthos. Hedwig. 4, t. 2.
lutescens. Hedwig. 4, t. 16.
plumosum. Hedwig, 4, t. 15.
densum.
cuspidatum. Dill. 39, 34.
gracile. Hedwig, 4, t. 6.
pilosum. N. S. Dill. 85, 18.
obtusifolium. N. S.
fragile. N. S.
salebrosum. N. S.
Iungermannia.
sphagni.
polyanthos. Dill. 70, 9.
scalaris.
nemorosa. Dill. 71, 18.
complanata. Dill. 72, 26.
tamariscifolia. Dill. 72, 31.
tomentella. Dill. 73, 35.
ciliaris. Dill. 69, 3.
pusilla. Dill. 74, 46.
pinguis. Dill. 74, 42.
furcata. Dill. 74, 45.
Marchantia.
polymorpha.
hemisphaerica.
Anthoceros.
laevis. Dill. 68, 2.
punctatus. Dill. 68, 1.
Lichen.
botryoides.
farinosus.
scriptus.
fuscus.
pertusus.
muscorum.
albo ater.

limitatus.

limitatus.
argenteus.
ſubfuſcus.
ater.
varius.
umbrinus. N. S.
immerſus.
Parellus.
anguloſus.
candelaris.
caeſius.
tiliaceus.
centrifugus.
ſpecioſus.
ciliatus.
ſtellaris.
ſtellariformis.
olivaceus.
pulverulentus.
anguſtatus.
criſpus.
Tremella.
nigreſcens.
faſcicularis.
furfuraceus.
crocatus.
glaucus.
diſſectus.
rufus.
polydactylos.
ſilvaticus.
decipiens.
velleus.
puſtulatus, ſcutellis atris!
Jacquini pullus.

cornutus.
furcatus.
Dillenii, 82, 1.
barbatus.
radiciformis.
pubeſcens.
chalybeiformis.
hirtus, tuberculis fuſcis!

Conferva.
rivularis.
fontinalis.
gelatinoſa.

Byſſus.
flos aquæ.
nigra.
fulva.
candida.

Tremella.
Piſum.
arborea. Hofman, t. 8, f. 1.
undulata. Hofm. t. 7, f. 1.
et aliæ.

Agaricus.
maculatus. Schæffer, t. 90.
plumbeus. Schæffer, t. 85, 86.
badius. Schæffer, t. 245.
fulvus. Schæffer, t. 95.
procerus. Schæffer, t. 23.
bulboſus. Schæffer, t. 241.
excoriatus. Schæffer, t. 18, 19.

emeticus. Schæff. t. 15. 16.
mutabilis. Schæff. t. 9.
faſtigiatus. Schæff. t. 2.
craſſipes. Schæff. t. 87.
lactifluus. Schæff. t. 5.
piperatus. Schæff. t. 83.
melleus. Schæff. t. 45.
umbilicatus.Schæff. 1.207.
fuſceſcens. Schæff. t. 60.
coccineus. Schæff. 302.
Hyacinthus. Batſch. t. 28.
janthinus. Schæff. t. 13.
fibrilloſus. Schæff. t. 236.
campanella. Schæff. t. 230.
minutus. Hoffm. t. 6. f. 3, 4.
ſtriatellus. Schæff. t. 211.
furnus. Schæff. t. 63, 70, 229.
fuliginarius. Batſch. f. 40.
androſaceus. Schæff. t. 239.
ſtellatus. Hoffm. t. 6. f. 2.
papillatus. Hoffman, t. 3. f. 2.
conicus. Schæff. t. 52. f. 1—6.
ſulcatus. Schæff. t. 52. f. 7—9.
aqueus Schæff. t. 17.
Balanus. Schæff. t. 66.
porcellaneus. Schæff. t. 47, 48.
Digitalis. Batſch, f. 1.
depluens. Batſch, f. 122.
applicatus. Batſch. f. 125.
gelatinoſus. Schæff. t. 213.
tomentellus an plumatus?
Merulius.
Cantharellus. Shæff. t. 82.
pezizoides. Schæff. t. 165, 166.
Boletus.
zonatus. Schæffer fung. t. 125.
numularius?
viſcidus. Schæff. t. 103, 104.
bovinus. Schæff. t. 123, 126.
mutabilis. Schæff. t. 108. 133.
ſtrobiliformis?
lapidum. Schæff. t. 105.
agaricoides.
ſuberoſus.
igniarius. Schæff. t. 137, 138.
cinnabarinus.
ſulphureus. Schæff. t. 131, 132.
ſuaveolens. Schæff. t. 124.
hepaticus. Schæff. t. 117, 118.
annulatus. Schæff. t. 106. 136.
flabelliformis. Schæff. t. 113.

Hydnum.

Hydnum.
repandum. Schæff. t. 141.
zonatum. Schæff. t. 272.
candidum.
Thelephora.
ſpecies aliquot.
Clavaria.
ophiogloſſoides.
cornea. Batſch. f. 161.
faſtigiata. Schæffer. t. 172.
muſcoides. Schæffer, t. 173.
Helvella.
clavata, Shæffer, t. 149.
mitra ?
Octoſpora.
ſulphurea. Batſch. f. 147.
citrina. Hedwig. crypt. 11, t. 8, B.
albidula. *Ibid.* t. 9, B.
luteſcens. *Ibid.* t. 9, C.
infundibuliformis. Schæff. t. 152.
cochleata. Schæff. t. 150.
coccinea. Schæff. t. 148.
hiſpida. Schæff. t. 151.
ciliata. Schæff. t. 284.
leucoloma. Hedw. 11, t. 4, A.
auricula.
et 10 *aliæ.*
Peziza.
lævis. Schæff. t. 180.
ſtriata. Schæff. t. 178.
crucibuliformis. Schæff. t. 179, 181.
Lycoperdon.
circumſciſſum. Schæff. t. 186, 189.
ſpadiceum. Schæff. t. 188.
lacerum. Schæff. t. 193, 194.
muricatum. Schæff. t. 184.
radicans. Schæff. t. 182.
nitidulum. Schæff. t. 192.
admorſum. Schæff. t. 187.
Hemonitis.
nivea. Hoffman, t. 4, f. 1.
ſphærocephala. Hofm. t. 4, f. 2.
lichenoides. Hofm. t. 4. f. 3.
botrytis. Hofm. t. 1, f. 1.
graniformis. Hofm. t. 1, f. 2.
veſparia. Batſch. f. 172.
favoginea. Batſch. f. 173.
et multæ aliæ.
Sphæria.
clavata. Hofman, t. 4, f. 2.
tunicata. Tode, f. 130.
deuſta. Tode, f. 129.
pulvinata. Hofman, t. 2, f. 3.
diſciformis. Hofm. t. 4, f. 1.
fragiformis. Hofm. t. 5, f. 1.
confluens. Tode, f. 87.
acuta. Hofm. t. 5, f. 2.

ſpermoides. Hofm. t. 3, f. 3.
tuberculoſa.
punctata ſ. granuloſa.
mamillaris.
et aliæ.
Tubercularia.
vulgaris. Hofman, t. 6, f. 2.
Mucor.
flavus.
Mucedo.
Eryſiphe.

Extract of a Letter from the* Reverend Dr. HENRY MUHLENBERG, *to* Dr. BARTON, *relative to the preceding Supplement.*

" WITH great ſatisfaction, I acknowledge the aſſiſtance I had from ſome of my friends, in making this ſupplement, in particular from Dr. James Edward Smith, the learned, candid, and ingenious poſſeſſor of the Herbarium of the two Linnæi; from Dr. Hoffman, in Göttingen, and from Dr. Hedwig in Leipzig, both well known by their excellent works on Lichens and Moſſes.

" I add the names of ſome books, mentioned in this ſupplement. Dillenii Hiſtoria Muſcorum. Oxonii, 1741. Hoffman Vegetabilia Cryptogamica. Erlangæ, 1787, faſc. 1. 2. Schæfferi Fungorum Icones. Ratiſbonæ, 1780. Batſch Elenchus Fungorum. Halæ, 1783. cum continuatione, fig. 1—232. Hedwig Stirpes Cryptogamicæ. Lipſiæ, Vol. 1. ii. iii. iv. Tode Fungi Selecti Meklenburgenſes."

* Dated Lancaſter, Auguſt 29th 1796.

On

N°. XXIX.

On the Mode moſt eaſily and effectually practicable of drying up the Marſhes of the maritime Parts of North America. By THOMAS WRIGHT, *Licentiate of the College of Surgeons in Ireland, and Teacher of Anatomy.*

Read Nov. 21, 1794.

HAVING for ſome years during the American war (here ſo called) traverſed that continent in the exerciſe of my profeſſion, I uſually noted ſuch local circumſtances as it occurred to me might be improved upon, or in ſome manner applied to uſeful purpoſes. The health of the ſoldiery being my particular object, I neceſſarily contemplated the cauſes of ſickneſs, ſome of which were ſo univerſal, that few, either natives or others eſcaped their baleful influence; but chiefly the effluvia of ſwampy lands in producing ague almoſt as an epidemic.

It is uſeleſs to know cauſes, it is idle to deſcant on them, unleſs with the intention by their removal to obviate their effects: there are but two modes of drying up the great marſhes of America; the moſt effectual would be by draining them, but that is not an eaſy taſk, as the dead level of the coaſt country between the Apalachian Mountains and the Atlantic ſeems to defy the moſt determined induſtry; this I relinquiſh as impracticable except by many years labour. I ſhall therefore propoſe what I hope will prove a more prompt remedy, and poſſibly not leſs effectual.

Following the moſt obvious appearances of things, it is evinced in the moſt legible characters of nature, the ſhoaling coaſt, ſandy beach, ſwampy plains, large rivers, ſandy hills raiſed over heaps of the exuviæ of marine animals, that the eaſtern coaſt of North America has been of of very late Neptunian origin; and this crude ſtate of the land not yet fully relinquiſhed by its antient oceanic poſſeſſor is the probable cauſe of ague being endemic. The important queſtion ariſing on this ſtatement is, how may the ſuperfluous waters be removed?—I anſwer by evaporation. On this ſubject let facts ſpeak; they only can throw light on ſuch a ſubject, and lay a ſolid foundation for theory; if this agent be found effectual in proportion to the intenſity of the climate, and if it has produced the deſired effect in a much cooler climate than that of America, *a fortiori*, it will prove more efficacious and more quickly ſo in that country.

The temperature of Ireland though equable ſeldom affords three months ſummer weather, i. e. above 60° Fahrenheit's; yet after the moſt rainy winters (and frequently here fall 30 inches of rain in the year) the temporary pools called *Turloughs* which collect in all our confined valleys, evaporate wholly, even before ſummer comes on, though the atmoſphere is generally loaded with moiſture, nay almoſt ſaturated with it from the ocean and other ſeas around us: and though here are wanting the two great requiſites for evaporation viz. *Air chemically dry*, and *heat* comparatively ſpeaking, the cauſe of this happy effect is very partial, it is the continental wind which always viſits us periodically and with the ſun after winter; it is abſolutely dry though not hot; it in one months blowing, and ever without ſtorms, rids the whole iſland of its ſuperfluous water, and even leaves the fields parched, the roads almoſt impaſſable for the duſt, and the lips of the inhabitants chapped and cleft by the quick evaporation. This is the ſeaſon which reſtores tone to our bodies relaxed and debilitated by a warm wet winter; for we have no epidemic inflammatory diſeaſes until the continental wind comes from the Eaſt.

2 Here

Here then if a few weeks well exſiccate the whole iſland; what prevents more months in America from producing the ſame effect, where there is a dry and a hot wind, certainly the latter?

In Ireland the ague is never epidemic, nor endemic, as far as I know, except the ſeaſon ſhould ſometimes be ſuch as to induce it, and of this I remember but one inſtance. Yet the ague and the dyſentery have been both epidemic in Ireland, as the ancient Britiſh ſettlers ſeverely experienced; and when they were ſo, Ireland reſembled America, it was a wood.

I ſhall relate one truly remarkable inſtance of the effects of clearing the country of wood in promoting evaporation. Before the time of Cromwell, not yet two centuries, there was a furnace for ſmelting iron ore and a foundery at the town of Montrath in the Queens County; the iron was ſent down a *then navigable river*, the *Nore*, to the next ſeaport for exportation: at this day that river has not water ſufficient to float a canoe, and is a mere rivulet for many miles below that town; nor is there at this inſtant any perſon of the neighbourhood who remembers it otherwiſe. What has this ariſen from? As much rain falls as ever, the climate is ſtill as cool; yet the winds in March remove all the autumnal and hyemal collections of water, and thus rivers formerly navigable are dwindled into brooks. Aſſuredly the ſame eaſterly winds prevailed before the ſeventeenth century, but the country was then covered with wood; it is now clear and the harſh breeze ſweeps the bare boſom of the earth, and bears away the combining moiſture. Admitting this then to be the fact, it may be replied to by obſerving, that it is evidently inadmiſſible in America, a new country where the crude earth has not yet yielded ſo many crops of vegetables as to rot and form peat or combuſtible turf for fuel, therefore timber is an indiſpenſable neceſſary of life.—This I grant; yet I think that the felling of the woods may be ſo regulated as to render economy and utility perfectly compatable, viz. in the following manner.

Let it be ſuppoſed that the N. W. and S. E. are the aſſeclæ or prevailing winds of North America; let the ſurveyor general mark out a tract of ſay 100 or 200 miles in a right line to be cleared of trees; then every blaſt from theſe two oppoſite points will ventilate 200 miles of country, bearing along the fumes of all the marſhes, while the great *viſto* or avenue ſkirted with wood at both ſides would furniſh the moſt ſalubrious and conſequently valuable ſituation for ſettlers.

Nº. XXX.

A Memoir on the Diſcovery of certain Bones of a Quadruped of the Clawed Kind in the Weſtern Parts of Virginia. *By* THOMAS JEFFERSON, *Eſq.*

Read March 10, 1797. IN a letter of July 3d, I informed our late moſt worthy preſident that ſome bones of a very large animal of the clawed kind had been recently diſcovered within this ſtate, and promiſed a communication on the ſubject as ſoon as we could recover what were ſtill recoverable of them. It is well known that the ſubſtratum of the country beyond the Blue Ridge is a limeſtone, abounding with large caverns, the earthy floors of which are highly impregnated with nitre; and that the inhabitants are in the habit of extracting the nitre from them. In digging the floor of one of theſe caves, belonging to Frederic Cromer in the county of Greenbriar, the labourers at the depth of two or three feet, came to ſome bones, the ſize and form of which beſpoke befpoke an animal unknown to them. The nitrous impregnation of the earth together with a fmall degree of petrification had probably been the means of their prefervation. The importance of the difcovery was not known to thofe who made it, yet it excited converfation in the neighbourhood, and led perfons of vague curiofity to feek and take away the bones. It was fortunate for fcience that one of its zealous and well informed friends, Colonel John Stewart of that neighbourhood, heard of the difcovery, and, fenfible from their defcription, that they were of an animal not known, took meafures without delay for faving thofe which ftill remained. He was kind enough to inform me of the incident, and to forward me the bones from time to time as they were recovered. To thefe I was enabled accidentally to add fome others by the kindnefs of a Mr. Hopkins of New-York, who had vifited the cave. Thefe bones are,

1ft. A fmall fragment of the femur or thigh bone; being in fact only its lower extremity, feparated from the main bone at its epiphyfis, fo as to give us only the two condyles, but thefe are nearly entire.

2d. A radius, perfect.

3d. An ulna, or fore-arm, perfect, except that it is broken in two.

4th. Three claws, and half a dozen other bones of the foot; but whether of a fore or hinder foot, is not evident.

About a foot in length of the refidue of the femur was found, it was fplit through the middle, and in that ftate was ufed as a fupport for one of the falt petre vats, this piece was afterwards loft, but its meafures had been firft taken as will be ftated hereafter.

Thefe bones only enable us to clafs the animal with the unquiculated quadrupeds; and of thefe the lion being neareft to him in fize, we will compare him with that animal, of whofe anatomy Monfieur Daubenton has fur-

 nifhed

nished very accurate measures in his tables at the end of Buffon's Natural History of the lion. These measures were taken as he* informs us from "a large lion of Africa," in which quarter the largest † are said to be produced. I shall select from his measures only those where we have the corresponding bones, converting them into our own inch and its fractions, that the comparison may be more obvious: and to avoid the embarrassment of designating our animal always by circumlocution and description, I will venture to refer to him by the name of the Great-Claw or Megalonyx, to which he seems sufficiently entitled by the distinguished size of that member.

	Megalonyx. Inches	Lion. Inches.
Length of the ulna, or fore-arm	20.1	13.7
Height of the olecranum -	3.5	1.85
Breadth of the ulna, from the point of the coronoide apophysis to the extremity of the olecranum -	9.55	
Breadth of the ulna at its middle	3.8	
Thickness at the same place -	1.14	
Circumference at the same place	6.7	
Length of the radius - -	17.75	12.37
Breadth of the radius at its head	2.65	1.38
Circumference at its middle -	7.4	3.62
Breadth at its lower extremity -	4.05	1.18
Diameter of the lower extremity of the femur at the base of the two condyles - - -	4.2	2.65
Transverse diameter of the larger condyle at its base	3.	
Circumference of both condyles at their base - -	11.65	

* Buffon, XVIII. 38. Paris edition in 31 vols. 12mo.
† 2. De Manet, 117.

Diameter

	Megalonyx. Inches.	Lion. Inches.
Diameter of the middle of the femur	4.25	1.15
Hollow of the femur at the ſame place	1.25	
Thickneſs of the bone ſurrounding the hollow - -	1.5	
Length of the longeſt claw -	7.5	1.41
Length of the ſecond phalanx of the ſame	3.2	1.41

The dimenſions of the largeſt of the foot bones are as follow,

	Inches.
Its greateſt diameter, or breadth at the joint	2.45
Its ſmalleſt diameter, or thickneſs at the ſame place	2.28
Its circumference at the ſame place -	7.1
Its circumference at the middle -	5.3

	Of longeſt toe.	Middle ſized toe.	Shorteſt toe.
2d. Phalanx. Its length	3.2	2.95	
Greateſt diameter at its head or upper joint - -	1.84	2.05	
Smalleſt diameter at the ſame place - -	1.4	1.54	
Circumference at the ſame place	5.25	5.8	
3d. Phalanx. Its length -	*7.5	†5.9	3.5
Greateſt diameter at its head or upper joint -	2.7	2.	1.45
Smalleſt diameter at the ſame place - -	.95	.9	.55
Circumference at the ſame place	6.45	4.8	

Were we to eſtimate the ſize of our animal by a compariſon with that of the lion on the principle of *ex pede Herculem*, by taking the longeſt claw of each as the mo-

* It is actually 6¾ inches long, but about ¾ inch appear to have been broken off.

† Actually 5.65 but about ¼ inch is broken off.

dule

dule of their meaſure, it would give us a being out of the limits of nature. It is fortunate therefore that we have ſome of the larger bones of the limbs which may furniſh a more certain eſtimate of his ſtature. Let us ſuppoſe then that his dimenſions of height, length and thickneſs, and of the principal members compoſing theſe, were of the ſame proportions with thoſe of the lion. In the table of M. Daubenton an ulna of 13.78 inches belonged to a lion $42\frac{1}{2}$ inches high over the ſhoulders : then an ulna of 20.1 inches beſpeaks a megalonyx of 5 feet 1.75 inches height, and as animals who have the ſame proportions of height, length, and thickneſs have their bulk or weights proportioned to the cubes* of any one of their dimenſions, the cube of 42.5 inches is to 262 lb. the height and weight of M. Daubenton's lion as the cube of 61.75 inches to 803 lb. the height and weight of the magalonyx; which would prove him a little more than three times the ſize of the lion. I ſuppoſe that we ſhould be ſafe in conſidering, on the authority of M. Daubenton, his lion as a large one. But let it paſs as one only of the ordinary ſize, and that the megalonyx whoſe bones happen to have been found was alſo of the ordinary ſize. It does † appear that there was diſſected for the academy of ſciences at Paris, a lion of 4 feet $9\frac{3}{8}$ inches height. This individual would weigh 644 lb. and would be in his ſpecies, what a man of eight feet height would be in ours. Such men have exiſted. A megalonyx equally monſtrous would be 7 feet high, and would weigh 2000 lb. but the ordinary race, and not the monſters of it, are the object of our preſent enquiry.

I have uſed the height alone of this animal to deduce his bulk, on the ſuppoſition that he might have been formed in the proportions of the lion. But theſe were

* Buffon xxii. 121.

† Buffon xviii. 15.

not

not his proportions, he was much thicker than the lion in proportion to his height, in his limbs certainly, and probably therefore in his body. The diameter of his radius, at its upper end, is near twice as great as that of the lion, and, at its lower end, more than thrice as great, which gives a mean proportion of $2\frac{1}{2}$ for 1. The femur of the lion was lefs than $1\frac{1}{4}$ inch diameter. That of the megalonyx is $4\frac{1}{4}$ inches, which is more than three for one. And as bodies of the fame length and fubftance have their weights proportioned to the fquares of their diameters, this excefs of caliber compounded with the height, would greatly aggravate the bulk of this animal. But when our fubject has already carried us beyond the limits of nature hitherto known, it is fafeft to ftop at the moft moderate conclufions, and not to follow appearances through all the conjectures they would furnifh, but leave thefe to be corroborated or corrected by future difcoveries. Let us only fay then, what we may fafely fay, that he was *more* than three times as large as the lion: that he ftood as pre-eminently at the head of the column of clawed animals as the mammoth ftood at that of the elephant, rhinoceros, and hippopotamus: and that he may have been as formidable an antagonift to the mammoth as the lion to the elephant.

A difficult queftion now prefents itfelf. What is become of the great-claw? Some light may be thrown on this by afking another queftion. Do the wild animals of the firft magnitude in any inftance fix their dwellings in a thickly inhabited country? fuch, I mean, as the elephant, the rhinoceros, the lion, the tyger? as far as my reading and recollection ferve me, I think they do not: but I hazard the opinion doubtingly, becaufe it is not the refult of full enquiry. Africa is chiefly inhabited along the margin of its feas and rivers. The interior defart is the domain of the elephant, the rhinoceros, the lion,

lion, the tyger. Such individuals as have their haunts nearest the inhabited frontier, enter it occasionally, and commit depredations when pressed by hunger: but the mass of their nation (if I may use the term) never approach the habitation of man, nor are within reach of it. When our ancestors arrived here, the Indian population, below the falls of the rivers, was about the twentieth part of what it now is. In this state of things, an animal resembling the lion seems to have been known even in the lower country. Most of the accounts given by the earlier adventurers to this part of America make a lion one of the animals of our forests. Sir John Hawkins * mentions this in 1564. Thomas Harriot, a man of learning, and of distinguished candor, who resided in Virginia in 1587 † does the same, so also does Bullock in his account of Virginia,‡ written about 1627, he says he drew his information from Pierce, Willoughby, Claiborne, and others who had been here, and from his own father who had lived here twelve years. It does not appear whether the fact is stated on their own view, or on information from the Indians, probably the latter. The progress of the new population would soon drive off the larger animals, and the largest first. In the present interior of our continent there is surely space and range enough for elephants and lions, if in that climate they could subsist; and for mammoths and megalonyxes who may subsist there. Our entire ignorance of the immense country to the West and North-West, and of its contents, does not authorise us to say what it does not contain.

Moreover it is a fact well known, and always susceptible of verification, that on a rock on the bank of the

* Hakluyt, 541. edition of 1589.
† Ibid. 757, and Smith's History of Virginia, 10.
‡ Bullock, page 5.

Kanhawa, near its confluence with the Ohio, there are carvings of many animals of that country, and among these one which has always been considered as a perfect figure of a lion. And these are so rudely done as to leave no room to suspect a foreign hand. This could not have been of the smaller and maneless lion of Mexico and Peru, known also in Africa both in * ancient and † modern times, though denied by ‡ M. de Buffon: because like the greater African lion, he is a tropical animal; and his want of a mane would not satisfy the figure. This figure then must have been taken from some other prototype, and that prototype must have resembled the lion sufficiently to satisfy the figure, and was probably the animal the description of which by the Indians made Hawkins, Harriot, and others conclude there were lions here. May we not presume that prototype to have been the great-claw?

Many traditions are in possession of our upper inhabitants, which themselves have heretofore considered as fables, but which have regained credit since the discovery of these bones. There has always been a story current that the first company of adventurers who went to seek an establishment in the county of Greenbriar, the night of their arrival were alarmed at their camp by the terrible roarings of some animal unknown to them: that he went round and round their camp, that at times they saw his eyes like two balls of fire, that their horses were so agonised with fear that they couched down on the earth, and their dogs crept in among them, not daring to bark. Their fires, it was thought, protected them, and the next morning they abandoned the country. This was little more than 30 years ago.—In the year 1765, George Wilson and John Davies, having gone to hunt

* Aristot. Animal, 9. 4. Pliny, 8. 16. † Kolbe. ‡ Buffon, xviii. 18.

on Cheat river, a branch of the Monongahela, heard one night, at a diſtance from their camp, a tremendous roaring, which became louder and louder as it approached, till they thought it reſembled thunder, and even made the earth tremble under them. The animal prowled round their camp a conſiderable time, during which their dogs, though on all other occaſions fierce, crept to their feet, could not be excited from their camp, nor even encouraged to bark. About day light they heard the ſame ſound repeated from the knob of a mountain about a mile off, and within a minute it was anſwered by a ſimilar voice from a neighbouring knob. Colonel John Stewart had this account from Wilſon in the year 1769, who was afterwards Lieutenant Colonel of a Pennſylvania regiment in the revolution-war; and ſome years after from Davies, who is now living in Kentucky.

Theſe circumſtances multiply the points of reſemblance between this animal and the lion. M. de la Harpe of the French Academy, in his abridgment of the General Hiſtory of Voyages, ſpeaking of the Moors, ſays * "it is remarkable that when, during their huntings, they meet with lions, their horſes, though famous for ſwiftneſs, are ſiezed with ſuch terror that they become motionleſs, and their dogs equally frightened, creep to the feet of their maſter, or of his horſe." Mr. Sparrman in his voyage to the Cape of Good Hope, chap. 11. ſays, "we could plainly diſcover by our animals when the lions, whether they roared or not, were obſerving us at a ſmall diſtance. For in that caſe the hounds did not venture to bark, but crept quite cloſe to the Hottentots; and our oxen and horſes ſighed deeply, frequently hanging back, and pulling ſlowly with all their might at the ſtrong ſtraps with which they were tied to the waggon. They

* Gentleman's, and London Magazines, for 1783.

also

alſo laid themſelves down on the ground, and ſtood up alternately, as if they did not know what to do with themſelves, and even as if they were in the agonies of death." He adds that " when the lion roars, he puts his mouth to the ground, ſo that the ſound is equally diffuſed to every quarter." M. de Buffon (xviii. 31.) deſcribes the roaring of the lion as, by its echoes reſembling thunder: and Sparrman c. 12. mentions that the eyes of the lion can be ſeen a conſiderable diſtance in the dark, and that the Hottentots watch for his eyes for their government. The phoſphoric appearance of the eye in the dark ſeems common to all animals of the cat kind.

The terror excited by theſe animals is not confined to brutes alone. A perſon of the name of Draper had gone in the year 1770, to hunt on the Kanhawa. He had turned his horſe looſe with a bell on, and had not yet got out of hearing when his attention was recalled by the rapid ringing of the bell. Suſpecting that Indians might be attempting to take off his horſe, he immediately returned to him, but before he arrived he was half eaten up. His dog ſcenting the trace of a wild beaſt, he followed him on it, and ſoon came in ſight of an animal of ſuch enormous ſize, that though one of our moſt daring hunters and beſt markſmen, he withdrew inſtantly, and as ſilently as poſſible, checking and bringing off his dog. He could recollect no more of the animal than his terrific bulk, and that his general outlines were thoſe of the cat kind. He was familiar with our animal miſcalled the panther, with our wolves and wild beaſts generally, and would not have miſtaken nor ſhrunk from them.

In fine, the bones exiſt: therefore the animal has exiſted. The movements of nature are in a never ending circle. The animal ſpecies which has once been put into a train of motion, is ſtill probably moving in that train. For if one link in nature's chain might be loſt, another and another might be loſt, till this whole ſyſtem of things ſhould evaniſh by piece-meal; a concluſion not warranted by the local diſappearance of one or two ſpecies of animals, and oppoſed by the thouſands and thouſands of inſtances of the renovating power conſtantly exerciſed by nature for the reproduction of all her ſubjects, animal, vegetable, and mineral. If this animal then has once exiſted, it is probable on this general view of the movements of nature that he ſtill exiſts, and rendered ſtill more probable by the relations of honeſt men applicable to him and to him alone. It would indeed be but conformable to the ordinary economy of nature to conjecture that ſhe had oppoſed ſufficient barriers to the too great multiplication of ſo powerful a deſtroyer. If lions and tygers multiplied as rabbits do, or eagles as pigeons, all other animal nature would have been long ago deſtroyed, and themſelves would have ultimately extinguiſhed after eating out their paſture. It is probable then that the great-claw has at all times been the rareſt of animals. Hence ſo little is known, and ſo little remains of him. His exiſtence however being at length diſcovered, enquiry will be excited, and further information of him will probably be obtained.

The Coſmogony of M. de Buffon ſuppoſes that the earth and all the other planets primary and ſecondary, have been maſſes of melted matter ſtruck off from the ſun by the incidence of a comet on it: that theſe have been cooling by degrees, firſt at the poles, and afterwards more and more towards their Equators: conſequently that on our earth there has been a time when the temperature of the poles ſuited the conſtitution of the elephant, the rhinoceros, and hippopotamus: and in proportion as the remoter zones became ſucceſſively too cold, theſe animals have retired more and more towards the Equatorial regions, till now that they are reduced to

the

the torrid zone as the ultimate ſtage of their exiſtence. To ſupport this theory, he * aſſumes the tuſks of the mammoth to have been thoſe of an elephant, ſome of his teeth to have belonged to the hippopotamus, and his largeſt grinders to an animal much greater than either, and to have been depoſited on the Miſſouri, the Ohio, the Holſton, when thoſe latitudes were not yet too cold for the conſtitutions of theſe animals. Should the bones of our animal, which may hereafter be found, differ only in ſize from thoſe of the lion, they may on this hypotheſis be claimed for the lion, now alſo reduced to the torrid zone, and its vicinities, and may be conſidered as an additional proof of this ſyſtem; and that there has been a time when our latitudes ſuited the lion as well as the other animals of that temperament. This is not the place to diſcuſs theories of the earth, nor to queſtion the gratuitous allotment to different animals of teeth not differing in any circumſtance. But let us for a moment grant this with his former poſtulata, and aſk how they will conſiſt with another theory of his "qu'il y a dans la combinaiſon des elemens et des autres cauſes phyſiques, quelque choſe de contraire a l'aggrandiſement de la nature vivante *dans ce nouveau monde;* qu'il y a des obſtacles au developpement et peutetre a la formation des grands germes †." He ſays that the mammoth was an elephant, yet ‡ two or three times as large as the elephants of Aſia and Africa: that ſome of his teeth were thoſe of a hippopotamus, yet of a hippopotamus § four times as large as thoſe of Africa: that the mammoth himſelf, for he ſtill conſiders him as a diſtinct animal,‖ "was of a ſize ſuperior to that of the largeſt elephants. That he was the primary and greateſt of all terreſtrial

* Buffon, Epoq. 2. 233, 234. † Buffon, xviii. 145. ‡ 2. Epoq. 223. § 1. Epoq. 246. 2. Epoq. 232. ‖ 2. Epoq. 234, 235.

animals."

animals." If the bones of the megalonyx be aſcribed to the lion, they muſt certainly have been of a lion of more than three times the volume of the African. I delivered to M. de Buffon the ſkeleton of our palmated elk, called orignal or mooſe, 7 feet high over the ſhoulders, he is often conſiderably higher. I cannot find that the European elk is more than two thirds of that height: conſequently not one third of the bulk of the American. He* acknowledges the palmated deer (daim) of America to be larger and ſtronger than that of the Old World. He † conſiders the round horned deer of theſe States and of Louiſiana as the roe, and admits they are of three times his ſize. Are we then from all this to draw a concluſion, the reverſe of that of M. de Buffon. That nature, has formed the larger animals of America, like its lakes, its rivers, and mountains, on a greater and prouder ſcale than in the other hemiſphere? Not at all, we are to conclude that ſhe has formed ſome things large and ſome things ſmall, on both ſides of the earth for reaſons which ſhe has not enabled us to penetrate; and that we ought not to ſhut our eyes upon one half of her facts, and build ſyſtems on the other half.

To return to our great-claw; I depoſit his bones with the Philoſophical Society, as well in evidence of their exiſtence and of their dimenſions, as for their ſafe-keeping; and I ſhall think it my duty to do the ſame by ſuch others as I may be fortunate enough to obtain the recovery of hereafter.

TH: JEFFERSON.

Monticello, Feb. 10th, 1797.

* Buffon, xxix. 245. † Ibid. xii. 91. 92. xxix. 245. Vide Suppl. 201.

P. S.

P. S. *March* 10*th*, 1797. After the preceding communication was ready to be delivered in to the Society, in a * periodical publication from London I met with an account and drawing of the ſkeleton of an animal dug up near the river La Plata in Paraguay, and now mounted in the cabinet of Natural Hiſtory of Madrid. The figure is not ſo done as to be relied on,. and the account is only an abſtract from that of Cuvier and Roume. This ſkeleton is alſo of the clawed-kind, and having only four teeth on each ſide above and below, all grinders, is in this account claſſed in the family of unquiculated quadrupeds deſtitute of cutting teeth, and receives the new denomination of megatherium, having nothing of our animal but the leg and foot bones, we have few points for a compariſon between them. They reſemble in their ſtature, that being 12 feet 9 inches long, and 6 feet 4½ inches high, and ours by computation 5 feet 1.75 inches high: they are alike in the coloſſal thickneſs of the thigh and leg bones alſo. They reſemble too in having claws: but thoſe of the figure appear very ſmall, and the verbal deſcription does not ſatisfy us whether the claw-bone, or only its horny cover be large. They agree too in the circumſtance of the two bones of the fore-arm being diſtinct and moveable on each other; which however is believed to be ſo uſual as to form no mark of diſtinction. They differ in the following circumſtances, if our relations are to be truſted. The megatherium is not of the cat form, as are the lion, tyger, and panther, but is ſaid to have ſtriking relations in all parts of its body with the bradypus, daſypus, pangolin, &c. According to analogy then, it probably was not carnivorous, had not the phoſphoric eye, nor leonine roar. But to ſolve ſatisfactorily the queſtion of identity, the diſcovery of

* Monthly Magazine, Sep. 1796.

fore-teeth, or of a jaw bone ſhewing it had, or had not, ſuch teeth, muſt be waited for, and hoped with patience. It may be better, in the mean time, to keep up the difference of name.

N°. XXXI.

A Letter from Mr. JOHN HECKEWELDER *to* BENJAMIN SMITH BARTON, M. D. *containing an Account of an Animal called the Big Naked Bear.*

DEAR SIR,

Read March 10, 1797.

I HAVE now to communicate to you, what came to my knowledge reſpecting an animal, which the Mohican Indians called Ahamagachktiât Mecehquá, and the Delawares (if I recollect right) Amangachktîát. *The Big Naked Bear.* Their reports run thus: That among all animals that had been formerly in this country, this was the moſt ferocious. That it was much larger, than the largeſt of the common bears, and remarkably long-bodied: all over, (except a ſpot of hair on its back of a white colour,) naked. That it attacked and devoured man and beaſt, and that a man, or a common bear, only ſerved for one meal to one of theſe animals. That with its teeth it could crack the ſtrongeſt bones. That it could not ſee very well, but in diſcovering its prey by ſcent, it exceeded all other animals. That it purſued its prey with unremitting ravenouſneſs, and that there was no other way of eſcaping, but by taking to a river, and either ſwimming down the ſame, or ſaving one's ſelf by means of a canoe. That its heart being remarkably ſmall, it could ſeldom be killed with the arrow. That the ſureſt way of deſtroying him was to break his back-bone. That when a party went out to deſtroy

 this

this animal, they firſt took leave of their friends and relations at home, conſidering themſelves as going on an expedition, perhaps never to return again. That when out, they ſought for his track, carefully attending to the courſe the wind blew, and endeavouring to keep as near as poſſible to a river. That every man of the party knew at what part of the body he was to take his aim. That ſome were to ſtrike at the back-bone, ſome at the head, and others at the heart. That the laſt of theſe animals known of, was on the eaſt ſide of the Mohicanni Sipu. (Hudſon's River) where, after devouring ſeveral Indians that were tilling their ground, a reſolute party, well provided with bows and arrows, &c. fell upon the following plan, in which they alſo ſucceeded, viz. knowing of a large high rock, perpendicular on all ſides, and level on the top, in the neighbourhood of where the naked bear kept, they made ladders, (Indian ladders) and placing theſe at the rock, they reconnoitred the ground around, and ſoon finding a freſh track of the animal, they haſtily returned, getting on the top of the rock, and drawing the ladders up after them. They then ſet up a cry, ſimilar to that of a child, whereupon this animal made its way thither, and attempted to climb the rock, the Indians pouring down their arrows in different directions, all the while upon him. The animal now grew very much enraged, biting with its teeth againſt the rock, and attempting to tear it with its claws, until at length they had conquered it.

The hiſtory of this animal uſed to be a ſubject of converſation among the Indians, eſpecially when in the woods a hunting. I have alſo heard them ſay to their children when crying: 'Huſh! the naked bear will hear you, be upon you, and devour you.' From the nature of their converſation on this ſubject, I was led to believe the ſtory had foundation. Old Indians whom I queſtioned on this matter, aſſured me it was fact, relying on the authenticity of their forefathers' relations. Further reports reſpecting this animal have *in part* ſlipped my memory, wherefore I omit making any mention of the ſame.

The panther is not conſidered by the Indians as *ſuch* a ravenous animal, as by the white people he is reported to be. I know but of one inſtance, where an Indian was nigh being attacked by one of them, but this was owing to the Indian's approaching his den. The Indian however found means of killing him, and taking the young, which he brought down to Philadelphia, which was about the year 1770. This animal, the Indians ſay, lives chiefly on deer, which it either by ſlyneſs catches itſelf, purſues after they have been crippled by the hunters, or takes from the wolves after they have caught them.

If hereafter, I ſhall have an opportunity of getting further information reſpecting the naked bear; I will freely communicate the ſame to you.

Believe me to be, Dear Sir,

Your truly affectionate friend, &c.

JOHN HECKEWELDER.

N°. XXXII.

Experiments and Obſervations on Land and Sea Air. By ADAM SEYBERT, M. D.

Read March 10, 1797.

AN endeavour to add any facts or obſervations to a branch of knowledge, which has been treated of by many of the moſt enlightened philoſophers of the preſent century, may be deemed a hazardous attempt. But although we have many accounts of eudiometrical experiments by Prieſtley, Fontana, Ingenhouſz

houſz and others; the ſubject is not exhauſted, and an extenſive field continues open for him who wiſhes to engage in this intricate branch of Pneumatic Philoſophy.

The purity of the air is not intereſting to us merely as an object of curioſity, but demands our attention as phyſicians and philoſophers. In proportion to the number of aſcertained facts, the certainty of inference is increaſed. The ſhort life of any one individual, together with his local ſituation, will prevent him from completing this department of ſcience. It is merely from repeated experiments made under different circumſtances, that we can expect to arrive at truth. The more we multiply facts the more decided may we be in our concluſions. Such are the reflections, which induced me to engage in a ſeries of experiments, which ſhall be related in the following pages.

Our atmoſphere having been ſo ſucceſsfully analyzed by the celebrated Lavoiſier, and being found to conſiſt of fluids poſſeſſing very different and oppoſite qualities; chemiſts ſoon began to enquire whether its ingredients might not be in various proportions in different ſituations; and, particularly, whether it differed in point of purity in different ſituations on land and on the ocean.

Moſt of the experiments of which we have an account were made on land: The Memoir of Dr. Ingenhouſz publiſhed in the 70th volume of the Philoſophical Tranſactions is the only eſſay I have ſeen containing experiments made at ſea: but his traverſe was ſo ſhort, that he had not an opportunity of examining the air in different latitudes. He, however is of opinion that ſea air is, cæteris paribus, purer than land air; but he appears to have found ſome ſeeming contradictions of his general inference. He ſays, page 364, that air taken from the middle of the channel was of an inferior quality to that at the mouth of the Thames; and that air near the ſea ſhore at Oſtend was nearly as good as that at the mouth of that river. Although we may, to a great degree, adopt his ſentiments, nevertheleſs I think it probable that this increaſed purity does not entirely depend upon the ocean; for I have found the air over the Bays of Cheſapeak and Delaware of the ſame degree of purity with the atmoſphere of the ocean. And hence I am inclined to think, that the air over a large body of water is always purer, cæteris paribus, than that of the adjoining land, owing perhaps to a decompoſition, which the water may ſuffer from the action of the Sun's rays; and this may likewiſe be aſſiſted by its alſo abſorbing many foreign matters which on land are more or leſs intimately mixed with the air in a mechanical way. This opinion is confirmed by Dr. White's experiments, who ſays: " the air over the river Ouze was conſtantly purer than that of the garden by 2 or 3 degrees." Philoſophical Tranſactions, vol. 68.—And in the ſame paper he obſerves, that the ſame happened with the air of the foſs when the marſhes were overflowed.

When I firſt engaged in theſe experiments it was my intention to perform them only on ſea air; but I ſoon found it neceſſary to repeat them on land air for the ſake of compariſon. The ſubject increaſed on my hands. The atmoſphere of marſhes preſented itſelf as worthy of ſerious inveſtigation. I therefore performed ſome experiments upon it; but proper length of time is neceſſary to their repetition; and for this reaſon I muſt omit them for the preſent, and merely relate thoſe I performed on the air of this city, its environs, and on the ocean.

I ſhall firſt proceed to the enquiry whether the atmoſphere differs in purity in different ſituations on land?

The opinion that the air is purer in the country and on the tops of mountains than it is in towns, is adopted by many; therefore in aſſerting the contrary we muſt prepare

prepare to meet with oppofition, particularly from thofe who have formed opinions from reafoning alone, unfupported by experiments. In doubtful matters it is chiefly by the clafhing of opinion, that truth is finally difcovered. This fhall be both my confolation and apology, if the refult of my experiments fhall be found to have induced me to differ from others. Neverthelefs it will afford me confiderable fatisfaction to agree with thofe whofe decifions reft upon the fame firm bafis. I fhall therefore briefly mention the authors who agree with me in opinion.

Dr. Prieftley concludes from his own experiments, that the difference of the air in different places, fuch as is indicated by a mixture of nitrous air, is in general very inconfiderable. He mentions that the air of Harthill near Manchefter and that of Wiltfhire were about the fame.

The compilers of the Encyclopædia fay; " that the general mafs remains upon all occafions pretty much the fame." And Scheele is much of the fame opinion.

But the accurate Fontana fpeaks with more confidence, and is more explicit. His affertions are founded upon the refult of many experiments, and he is inclined to believe, that the flight variations mentioned by fome philofophers, are rather to be attributed " to the fallacious effects of uncertain methods" than to any real difference in the air itfelf. He found the air of Iflington and London to fuffer an equal diminution from the mixture with nitrous air. The air taken at different heights in London and Paris did not differ in purity. Air at the height of 313 and 202 feet in London, differed fcarcely at all; and no difference was perceptible between the air of thefe heights and that of the ftreet adjoining.

The more I reflect on this fubject, the more I am inclined to adopt the following fentiment of this laft mentioned

tioned gentleman; viz. "The difference in the purity of the air at different times, is much greater than the difference between the air of the different places." Indeed moſt of the experiments related by Dr. Ingenhouſz alſo tend to confirm it. In general the difference in the air of different places at the ſame time was by no means conſiderable.

I ſhall now with more confidence relate the experiments I myſelf performed: but previous to this recital I ſhall give a brief account of the method I purſued.

It is neceſſary to remark, that every experiment I ſhall relate is the reſult of at leaſt two different trials.

Moſt authors who have engaged in this ſubject uſed eudiometers of a different conſtruction; I adopted the moſt ſimple as the beſt. Thoſe who deſire a particular deſcription of theſe inſtruments may be ſatisfied by referring to the Encyclopædia and different parts of Dr. Prieſtley's Treatiſe on Air. Mine is as follows;*

I had a glaſs tube about 14 inches in length, and in diameter nearly half an inch, provided with a graduated ſcale, made ſo as to ſlide upon the tube up or down as occaſion required. This ſcale was divided into one hundred equal parts.

My meaſure was a ſmall ſmelling bottle, containing ʒj. and gr. xvj. of clear pump water. The ſpace occupied in the tube by a bulk of air which this meaſured,

* The atmoſphere is proved by inconteſtable experiments to conſiſt in general of,

Oxygen gas 0.27
Azotic gas 0.72 and
Carbonic acid 0.01.

It is a fact well known to chemiſts, that nitious air will combine with oxygen gas and form a compound, viz. the nitric acid. As theſe two gaſes combine they aſſume a ſtate approaching nearer to that of a ſolid and conſequently occupy leſs ſpace than they did before their union. Upon this diminution of bulk depends our eſtimation of the purity of the air. The greater the contraction, the purer we ſuppoſe the air under trial.

could

could contain was equal to the hundred divifions of the graduated fcale.

My water trough on board of the fhip was the common water bucket; on fhore it was a common houfe bucket or tub.

The nitrous gas was prepared from diluted nitric acid and brafs filings.

At fea I ufed fea water in the trough; on land common pump water: for from different trials made by Dr. Ingenhoufz it is evident this circumftance could not produce a variation in the refult of the experiments.

My method of operating is as follows: After having introduced two meafures of the air, whofe purity I defired to afcertain, into the glafs tube, I introduce one meafure of nitrous gas; then, fuffering the tube to remain undifturbed for about a minute, I noted down how far the water afcended without agitation; this is what I have called, upon mixture: I then agitated the tube three fucceffive times, after the manner of M. de Sauffure, and noted how high the water rofe. In many inftances I added a fecond meafure of nitrous gas, and thereby completely faturated the air under examination.

I was particularly cautious of avoiding miftakes from hurry or inattention, and took fome pains to guard againft all the circumftances Dr. Ingenhoufz mentions as liable to produce a variation in the refult of experiments of this kind.

My firft experiment on land air was performed Auguft 2d, 1796. Two meafures of air in the yard of my lodging, when mixed with one meafure of nitrous air, left upon mixture 2.48 of a meafure; and after fhaking the tube 1.79. I then added another meafure of nitrous air and 2.65 remained.

I then fubmitted air to the teft of the eudiometer which I had previoufly collected in different ftreets of this city, viz. in Water between Market and Arch Streets; in Spruce near Fourth Street; in Chefnut near Fifth; and, in Market between Second and Third Streets. Each of thefe airs gave nearly the fame refult, and generally agreed with that of the air of the yard of my lodging: None of the experiments fhew a difference of 0.02 of a meafure.

Similar experiments I have fince repeated and the refult was the fame.

Auguft 3d. I collected air on the top of the hill whereupon Dr. Smith's Obfervatory ftands at the Falls of Schuylkill, five miles from Philadelphia. In another phial I received air from above the middle of the road directly at the foot of the hill. And immediately on my return home I fubmitted them and the air of the yard to experiment and found them to agree exactly as follows;

Upon mixture 2.48

After fhaking the tube 1.78 and upon adding a fecond meafure of nitrous air 2.63 remained.

Auguft 5th. I collected air from above two different marfhy fituations immediately below the rope-walks to the fouth of this city. It is of confequence to remark that thefe marfhes are overflowed by the tide. Another phial I filled immediately before entering the city in Front Street. Thefe airs fuffered an equal diminution from a mixture of nitrous gas, viz. 2.47 upon mixture; after fhaking the tube 1.79; and after adding a fecond meafure of nitrous gas 2.64 remained.

The air near my lodging yielded upon mixture 2.49; after fhaking the tube 1.78; and upon the addition of a fecond meafure of nitrous gas 2.62.

I performed fome experiments on air collected in other fituations about the city; but, finding the refult fo much the fame as thofe above related, I did not make any note of them, and remain perfectly fatisfied that Fontana's affertion is well founded.

To theſe experiments I will ſubjoin thoſe I made on the ocean during a paſſage from Bourdeaux to Philadelphia. It appeared to me preferable to connect them in the form of a table, as thereby I ſhould avoid a needleſs repetition; and place before the reader a ſhort though accurate view of all the experiments at the ſame time.

The experiments I performed on the River Elk and Bay of Cheſapeak perfectly agree with each other; and the reſult was the ſame with thoſe performed on the 7th of July &c. as mentioned in the table. The wind blew from the North and the ſky was partially cloudy. They were performed in Auguſt laſt.

My experiments at ſea ſufficiently prove that the atmoſphere is conſiderably purer there than it is on land. Though there are ſome trifling differences in the reſults of ſeveral experiments, I have no reaſon to believe that they were owing to the different ſituation in point of latitude or longitude in which they were performed. I can form no ſyſtem reſpecting ſuch variations. Winds, temperature, rain, &c. do not ſeem to have produced them. As they did not obſerve any regularity in their occurrence, they may perhaps be attributed to certain unperceived errors which are unavoidably attendant on ſuch trials.

That the air at ſea ſhould appear nearly of the ſame purity in different latitudes does by no means aſtoniſh me; for if land air has certain matters mixed with it they are *perhaps* abſorbed; and if my ſuppoſition be true, that the influence of the Sun's rays on the water tends to encreaſe its purity, the opinion I entertain is not ſurpriſing. For when once purified, there are perhaps none, or few cauſes to render the air noxious after it is wafted from our towns and cities over a large body of water.

It occurred to me that probably the purity of the air at ſea varied at different periods of the day: to ſatisfy myſelf on this point I made ſeveral trials on the 10th and 17th of June laſt. On the 10th I performed them at 9 o'clock A. M. at 12, and at 6 o'clock P. M. On the 17th at 9 A. M. and at 12 o'clock. The reſult of all the experiments of the ſame day was exactly ſimilar, at leaſt not perceptibly different.

Whether or not ſea air might be rendered more pure by agitation with water, appeared to me to be a queſtion worthy of being aſcertained. Particularly as ſome celebrated men reaſon that it has this effect, and muſt hence be looked upon as one of the greateſt reſources which we have for purifying the atmoſphere. Sir John Pringle and Dr. Ingenhouſz are of this opinion. But ſome of Dr. Prieſtley's experiments ſeem to contradict it; and ſo does the following aſſertion of the celebrated Scheele, who ſays; "L'air ne s'unit pas avec l'eau commune." Traité de l'air and du feu, p. 51.

My experiments on this head are as follow: On the 26th and 28th of June, the 2d and 5th of July, equal bulks of ſea water and air were agitated for half an hour in my eudiometer tube; but I never diſcovered any abſorption to have taken place; neither was the air rendered purer, as was evident from a mixture with nitrous air.

It now appeared probable to me that ſea water was already ſaturated with all the gaſeous particles it could abſorb; and that freſh water when agitated with ſea air might diminiſh its bulk or alter its purity. In conſequence of this ſuppoſition, equal bulks of ſea air and freſh water were agitated as above; but it was not in the leaſt altered. Not entirely ſatisfied of the fallacy of my conjecture, I boiled ſea water a ſufficient time to purge it of the air it might contain. I then agitated ſea air with this boiled water as above mentioned and found no difference in reſult from the other experiments. Theſe reſults tend to confirm me in my belief that if ſea water

I purifies

purifies the air, it is rather by adding a ſomewhat than by abſorbing any conſiderable quantity of effluvia floating therein. Though by this I do not mean to ſay that certain matters foreign to our atmoſphere do not float therein on land. If they exiſt, perhaps they may be ſubject to abſorption by water.

TABLE

TABLE OF EXPERIMENTS *performed on the Atmosphere at Sea.* By ADAM SEYBERT, M. D.

Month.	Time of Day.	Ther.	Longitude.	N. Latitude.	Winds.	Eudiometer.	General State of the Weather, &c.
June. 5	12h	68°	33°47′	42°01′	SW. by S.	2.37 upon mixture, 1.68 after ſhaking the tube. 2.54 upon adding a ſecond meaſure of nitrous air.	High ſea: Sky clear.
10	12	ibid.	39 09	43 32	E.	2.37 upon mixture, 1.67 after ſhaking the tube.	For three days paſt had heavy gales, with a very high ſea: The winds were variable and the weather in general hazy and cloudy.
11	12	ibid.	40 35	43 20	SW. by S.	2.39 upon mixture, 1.70 after ſhaking the tube.	Sea moderate: Cloudy.
14	12	69	42 09	42 43	S. W.	2.40 upon mixture, 1.67 after ſhaking the tube.	Sea moderate: Cloudy.
16	12	68	43 05	43 26	S. W.	2.38 upon mixture, 1.70 after ſhaking the tube.	Sea moderate: Cloudy: Rained early this morning: The experiments were performed immediately after a heavy ſhower of rain.
17	12	ibid.	44 55	43 29	N. E.	2.38 upon mixture, 1.70 after ſhaking the tube, 2.54 upon adding a ſecond meaſure of nitrous air.	Sea perfectly calm: Partially cloudy.

Continuation

Continuation of TABLE OF EXPERIMENTS ON SEA AIR.

Month.	Time of Day.	Ther.	Longitude.	N. Latitude.	Winds.	Eudiometer.	General State of the Weather, &c.
June. 19	12h	69°	46°00′	42°40′	S. W.	2.40 upon mixture, 1 70 after ſhaking the tube.	Sea moderate: Sky clear.
22	12	ibid.	47 03	42 35	S. W.	2.39 upon mixture, 1.70 after ſhaking the tube.	Sea moderate: Cloudy.
23	12	67	49 05	41 59	N. E.	2.37 upon mixture, 1.67 after ſhaking the tube.	Rained during all laſt night, accompanied with thunder and lightning: Rain continued this morning: Sea moderate.
25	12	70	53 08	38 07	N. by E.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.55 upon adding a ſecond meaſure of nitrous air.	Sea moderate: Partially cloudy.
26	12	ibid.	54 09	38 15	S. E.	2.37 upon mixture, 1.70 after ſhaking the tube.	Sea moderate: Partially cloudy.
28	12	68	55 11	39 00	N.	2.37 upon mixture, 1.69 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Laſt night heavy rain with thunder and lightning: Sea moderate: Sky clear.

Continuation

Continuation of TABLE OF EXPERIMENTS ON SEA AIR.

Month.	Time of Day.	Ther.	Longi-tude.	N. Lati-tude.	Winds.	Eudiometer.	General State of the Weather, &c.
July. 1	12	68°	56°45'	38°39'	N. W.	2.37 upon mixture, 1.69 after ſhaking the tube, 2.54 upon adding a ſecond meaſure of nitrous air.	Sea moderate: Sky clear: Heavy rain laſt night with thunder and lightning.
2	12	69	57 17	37 28	N. W	2.37 upon mixture, 1.70 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Sea calm: Sky clear.
3	12	70	57 38	37 05	S. W.	2.37 upon mixture, 1.70 after ſhaking the tube, 2.57 upon adding a ſecond meaſure of nitrous air.	Sea perfectly calm: Sky clear.
4	12	69	58 33	37 17	N. W.	2.37 upon mixture, 1.69 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Sea ſmooth: Sky clear.
5	12	70	59 30	37 16	N. W.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Sea ſmooth: Cloudy.

Continuation

Continuation of TABLE OF EXPERIMENTS ON SEA AIR.

Month.	Time of Day.	Ther.	Longitude.	N. Latitude.	Winds.	Eudiometer.	General State of the Weather, &c.
July. 6	12h	68°	60°33′	38°40′	W.N.W.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Sea ſmooth : Sky clear.
7	12	70	61 43	38 44	S. S. W.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Sea ſmooth : Sky clear.
13	12	69	72 25	39 11	S. W.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.55 upon adding a ſecond meaſure of nitrous air.	At 3 o'clock A. M. had ſoundings in 33 fathoms water : Sea ſmooth : Cloudy : Thunder at a diſtance.
14	12	70	73 00	39 09	S. by E.	2.37 upon mixture, 1.69 after ſhaking the tube, 2.54 upon adding a ſecond meaſure of nitrous air.	Soundings in 20 fathoms water : Laſt night heavy rain with thunder and lightning : Sea ſmooth : Sky clear.
15	12	ibid.		39 00	S. W.	2 37 upon mixture, 1.70 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	About 5 leagues from the land : Sea ſmooth : Sky clear.

Continuation

Continuation of TABLE OF EXPERIMENTS ON SEA AIR.

Month.	Time of Day.	Ther.	Longitude.	N. Latitude.	Winds.	Eudiometer.	General State of the Weather, &c.
July. 16	12h	70°			S. W.	2.37 upon mixture, 1.67 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Cape-May in ſight: Land diſtant about 2 leagues: Sea ſmooth: Cloudy.
17	12	ibid.			S.	2.37 upon mixture, 1.70 after ſhaking the tube, 2.56 upon adding a ſecond meaſure of nitrous air.	Entering the capes: Sea moderate: Sky clear. At 6 o'clock P. M. off Bombay Hook: Experiments were performed and the reſult was the ſame as of thoſe at 12 o'clock.
18	11	ibid.			S. E.	2.37 upon mixture, 1.69 after ſhaking the tube, 2.55 upon adding a ſecond meaſure of nitrous air.	Oppoſite New-Caſtle: Partially cloudy: at 4 o'clock P. M. oppoſite Cheſter: Experiments were again performed, and the reſult was the ſame as of thoſe of the morning. A heavy thunder ſtorm ſucceeded in the evening.

I

Tranſlation

N°. XXXIII.

Translation of a Memoir on a new Species of Siren. By M. de BEAUVOIS.

Read Feb. 19, 1796. AMPHIBIOUS animals properly ſo called, ſo dreadful and hideous to the vulgar, but ſo different to the eyes of the naturaliſt to whom all the productions of nature are equally intereſting, offer us an infinite ſcope for diſcovery. Naturaliſts therefore not ſtopped by the thoughtleſs repugnance of the vulgar to animals infinitely leſs dangerous than they ſuppoſe, and conſiderably more uſeful than ignorance (which is continually aſking to what purpoſe are all theſe things) can imagine; naturaliſts I ſay have left us data reſpecting theſe beings, which with time, muſt lead us to a more correct knowledge of, and a more intimate acquaintance with them. The animal to be treated of in this memoir is a proof of what I advance.

In examining Mr. Peale's collection, I had occaſion to remark amongſt the amphibiæ one which I have not ſeen deſcribed by any author. It appeared to me entirely new, and the more intereſting as tending to determine our ideas of the Inguana, which has by ſome been claſſed amongſt the amphibiæ, by others with fiſh; but which we find to be an intermediate claſs connecting theſe two.

After having examined, deſcribed, and drawn this new animal, Mr. Peale and I have thought proper to ſpeak of it to this Society before the publication of his catalogue which will ſoon take place.

Linnæus, the celebrated Linnæus, whom jealouſy is ſometimes pleaſed to criticiſe generally without cauſe; Linnæus whoſe errors, always exaggerated by his detractors, are (let my admiration for the merits of this great man excuſe the expreſſion) for the greater part marked with a ray of genius; Linnæus I ſay had formed a ſeparate order of the Inguana (A) diſcovered in South Carolina by Dr. Garden, ſince whoſe death other naturaliſts amongſt whom was Mr. Compſer, (B) have made ſome new obſervations reſpecting it. It was regarded by him, Bonnaterre, (B) and Gmelin the laſt editor of the works of Linnæus as a fiſh. The latter naturaliſt conſequently ſuppreſſed the order of Meantes; and the *Siren lacertina* is now found placed amongſt the *Muræna* under the name of *Muræna Siren.* Although this animal has much analogy to a fiſh, being furniſhed with gills, Gmelin has obſerved that in the formation of them, the Inguana and Muræna are diſtinguiſhable by the numbers of rays. He therefore ſuppoſes it ſhould be placed amongſt the branchioſtegæ whatever relation it might otherwiſe have with the Muræna.

Such is the laſt opinion reſpecting the Inguana (C) of which we will give a deſcription in order that we may compare it with that of the new animal which is principally the object of this memoir.

Deſcription of the Inguana, called Mud Inguana *by the Americans,* Siren lacertina *by Linnæus, and* Muræna Siren *by Gmelin.*

Head flat at top, rounded at the noſe, eyes ſmall, noſtrils ſmall and placed near the end of the ſnout which is ſometimes marked with a brown ſpot, colour cheſnut, fig. 1. A B C D.

Mouth furniſhed with a row of ſmall teeth, fig. 2. Auricular hole nearly in the form of a ſemicircle, furniſhed on the exterior with three ſhort, thick fringed lobes adhering to three ſerrated rays on the interior with opercula, fig. 1. E.

Only

Only two ſhort fore feet, each furniſhed with four toes terminated each by a ſmall ſharp nail, fig. 1. F.

Body nearly round, *ſhrunk*, and ſtreaked on the ſides, covered with ſmall ſcales thinly ſpread and faintly ſeen, fig. 1. G.

Tail flat, furniſhed both above and below with a ſimple membrane, without either points or prickles, fig. 1. H.

Deſcription of a New Animal, found in a Swamp in Jerſey near the Delaware, not very diſtant from the Middle Ferry oppoſite the City of Philadelphia.

HEAD flat, rounded at its extremity, eyes and noſtrils as in the former, except that the latter are rather nearer together, fig. 3. A B C D.

Mouth large, extending further back than the eyes, furniſhed with a row of ſmall teeth as in the former, fig. 3. E.

Auricular hole large, bordered on the upper part by three ſharp fringed lobes, adhering at one end to three ſerrated rays placed in the interior and of which they are a continuation, fig. 3. F.

Under the head two opercula united, forming but one piece, fig. 4. Four feet, thoſe before furniſhed with four toes, thoſe behind with five. I preſume they were furniſhed with nails, the animal being preſerved in ſpirits of wine has been ſomewhat changed in its parts, fig. 3. I.

Body ſomewhat flattened, ſtreaked on the ſides, flatteſt above and below; which gives it a ſquare appearance, fig. 3. G.

Tail flat, furniſhed on the top with a ſimple membrane, which commences nearly at the neck, and extends itſelf under the tail as far as the anus, fig. 3. H.

Mr. Peale has preſerved the latter animal alive in water for nearly thirty ſix hours, at the end of which time it died. He obſerved, that as long as it lived it continued ſwimming, making uſe of its feet and principally of its tail; that the lobes which terminate the gills were continually floating and in motion; either, by a power of motion belonging to them, or perhaps rather the effect of the motion which the animal cauſed with its feet and tail, and which was communicated to all parts of the body. He does not recollect whether the *opercula* opened and cloſed as in fiſh, but judging from the conformation of thoſe parts I am led to believe they do not.

As long as the Inguana only, was known, incertitude reſpecting its nature might have placed it rather with fiſh, to which it is true it bears an affinity by an eſſential character, gills, than with the amphibiæ to which it ſeems to belong by all the other parts of its body. But now a new individual of the ſame kind, furniſhed with four feet like lizards, ſeems to indicate that it cannot belong to fiſh.

On this diſcovery three very important queſtions ariſe. I do not flatter myſelf I ſhall be able to reſolve them, but will endeavour to diſcuſs them and give my opinion.

Are theſe animals fiſh? Do they belong to the amphibiæ? Or do they form in the order of nature a new intermediate claſs.

If we form our opinion of the animals we have been deſcribing merely from their gills, there is not a doubt but that we muſt conſider them as fiſh. Meſſrs Vicq D'azir and D'Aubenton, aſcribe the following characters to fiſh, That they are furniſhed with gills which give admittance to the air, that they have not lungs, viſcera which are wanting in all oviparous animals, except birds and the amphibiæ. But if we judge from the entire conformation of all their parts, can we call thoſe animals fiſh whoſe bodies, head, tails, and feet are ſimilar

to

Fig. 1.

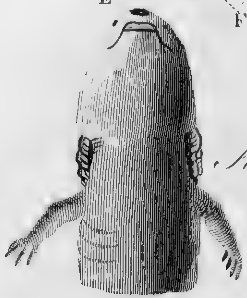


Fig. 2.

Fig. 4.

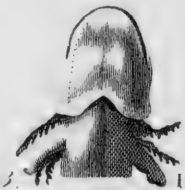


Fig. 3.

to thofe of lizards? Can we fay with Gmelin that the feet of the Inguana are but digitated pectoral fins? and in defcribing the new animal upon the fame principles, fhall we call its hind feet digitated abdominal fins? On the other fide fhall we rank animals whofe gills are exactly fimilar to thofe of fifh with lizards? No. I think that both thefe opinions would be equally improper; and it appears to me more natural to believe that thefe animals thus organized, appertaining in a certain degree to each, fhould form an intermediate and well marked clafs between lizards and fifh. And until more obfervations be made, and other difcoveries of new individuals fhall enable us to form this clafs, I think it would be beft to revive the order of *Meantes* eftablifhed by Linnæus, and improperly fuppreffed by other naturalifts.

It remains to confider whether thefe animals are of the fame, or whether they form between themfelves a diftinct genus. It is certain that in comparing them, fenfible difference may be obferved; but thefe differences appear only fpecific, and fhould yield to the common character of having three exterior fringed lobes attached to three ferrated interior rays, and feet. I will call the firft then, with Linnæus, Siren Lacertina, and the other Siren operculata.

An

No. XXXIV.

An Attempt to investigate the Causes why the Winters in North America are colder than the Winters in Europe, in the same Latitudes; and why the Eastern sides of both the Northern Continents are colder than the Western. By Dr. WILLIAM BARNWELL——Should have been inserted here, and some part of it was printed; but in the time of the Yellow Fever the copy was mislaid and it was unavoidably postponed.

N°. XXXV.

Obſervations intended to favour a ſuppoſition that the Black Color (as it is called) of the Negroes is derived from the Leprosy. *By* Dr. Benjamin Rush.

Read at a Special Meeting July 14, 1792.

DR. SMITH in his elegant and ingenious Eſſay upon the Variety of Color and Figure in the Human Species has derived it from four cauſes, viz. climate, diet, ſtate of ſociety, and diſeaſes. I admit the Doctor's facts, and reaſonings as far as he has extended them, in the fulleſt manner. I ſhall only add to them a few obſervations which are intended to prove that the color and figure of that part of our fellow creatures who are known by the epithet of negroes, are derived from a modification of that diſeaſe, which is known by the name of Leproſy.

Many facts recorded by hiſtorians, as well as phyſicians ſhow the influence of unwholſome diet in having produced the leproſy in the middle and northern parts of Europe in the 13th and 14th centuries. The ſame cauſe, combined with greater heat, more ſavage manners, and bilious fevers, probably produced this diſeaſe in the ſkin among the natives of Africa. But I will not reſt the proofs of the color and figure of the negroes being a leproſy ſimply upon its cauſes. Other circumſtances make it much more probable. I ſhall briefly enumerate them.

1. The leproſy is accompanied in ſome inſtances with a black color of the ſkin. Of this I have met with a ſatisfactory proof in Dr. Theiry's account of the diſeaſes of Aſturia in Spain. I ſhall inſert a tranſlation of his own words upon this ſubject. " There are (ſays this excellent phyſician) above twenty hoſpitals for lepers in this province, and I have obſerved ſix ſpecies of the diſorder.

One of them, viz. the ſecond, is called the *black albaras* of the Arabians. The ſkin becomes black, thick and greaſey.—There are neither puſtules, nor turbercles, nor ſcales, nor any thing out of the way on the ſkin. The body is not in the leaſt emaciated. The breathing is a little difficult, and the countenance has ſome fierceneſs in it. They exhale perpetually a peculiar and diſagreeable ſmell, which I can compare to nothing but the ſmell of a mortified limb."* This ſmell mentioned by Dr. Theiry continues with a ſmall modification in the native African to this day.

2. The leproſy is deſcribed in the Old Teſtament, and by many ancient writers as imparting a preternatural whiteneſs to the ſkin. Perſons thus marked, have lately received the name of *albanos*. Solitary inſtances of this diſeaſe are often met with it upon the Alps, but travellers tell us that it is one of the endemics of Java, Guinea and Panama where it is perpetuated through many generations. Mr. Hawkins in his travels into the interior parts of Africa has deſcribed the perſons afflicted with this diſeaſe in the following words. "They go entirely naked; their ſkin is white, but has not that animated appearance ſo perceptible in Europeans. It has a dull deathlike whitiſh caſt that conveys an idea more of ſickneſs, than of health. Their hair is red, or aſhes-coloured, yellowiſh wool, and their eyes are uniformly white, in that part by which others are diſtinguiſhed into the black, grey and blue eyes. They are ſet deep in the head, and very commonly ſquint, for as their ſkin is deprived of the black mucous web, the diſtinguiſhing characteriſtic of theſe Africans, ſo their eyes are deſtitute of that black matter reſembling a pigment, ſo univerſally found in people of all

* Obſervations de Phyſique et de Medecine faites en differens lieux de l'Eſpagne. Vol. ii. p. 130.

coun-

countries, and ſo uſeful in preventing the eye from being injured in caſes of expoſure to ſtrong light."* This artleſs traveller does not ſtop here. The idea of this peculiarity in the color and features of theſe people being a diſeaſe, and even its ſpecific nature did not eſcape him, hence he adds "These people rendered unfortunate by the prejudices of their countrymen, are born of black parents; they have all the features of other inhabitants, but differ from them only in the above circumſtances. The difference of color cannot ariſe from the intercourſe of whites and blacks, for the whites are very rarely among them, and the reſult of this union is well known to be the yellow color, or mulatto. Many of the natives aſſert that they are produced by the women being debauched in the woods by the large baboon, ourang-outang, and by that ſpecies in particular called the guaga mooroos. No ſatisfactory diſcovery has been made to account for ſuch ſingular, but not unfrequent phænomena in the ſpecies. It may perhaps be aſcribed to *diſeaſe*, and that of the *leprous* kind, with more reaſon than to any other cauſe that has been yet aſſigned."† Mr. Bernardin concurs with Mr. Hawkins in aſcribing this morbid whiteneſs in the ſkins of the Africans wholly to the leproſy.‡ However oppoſed it may be to their morbid blackneſs, it is in ſtrict conformity to the operations of nature in other diſeaſes. The ſame ſtate of malignant fever is often marked by oppoſite colors in the ſtools, by an oppoſite temperature of the ſkin, and by oppoſite ſtates of the alimentary canal.

The original connection of the black color of the negroes with the leproſy is further ſuggeſted by the following fact taken from Bougainville's voyage round the world.§

* P. 116. 117.
† P. 117. 118.
‡ Studies of Nature, vol. ii. p. 2.
§ Page 294.

He tells us that on an iſland in the Pacific Ocean which he viſited, the inhabitants were compoſed of negroes and mulattoes. They had thick lips, woolly hair, and were ſometimes of a yellowiſh color. They were ſhort, ugly, ill proportioned, and moſt of them infected with the leproſy, a circumſtance from which he called the iſland they inhabit, the Iſle of Lepers.

3. The leproſy ſometimes appears with white and black ſpots blended together in every part of the body. A picture of a negro man in Virginia in whom this mixture of white and black had taken place, has been happily preſerved by Mr. Peale in his muſeum.

4. The leproſy induces a morbid inſenſibility in the nerves. In countries where the diſeaſe prevails, it is common to ſay that a perſon devoid of ſenſibility, has no more feeling than a leper. This inſenſibility belongs in a peculiar manner to the negroes. Dr. Moſeley ſays, " they are void of ſenſibility to a ſurprizing degree. They ſleep ſound in every diſeaſe, nor does any mental diſturbance ever keep them awake. They bear ſurgical operations much better than white people, and what would be a cauſe of inſupportable pain to a white man, a negro would almoſt diſregard. I have amputated the legs of many negroes, who have held the upper part of the limb themſelves."* This morbid inſenſibility in the negroes diſcovers itſelf further in the apathy with which they expoſe themſelves to great heat, and the indifference with which they handle coals of fire.

5. Lepers are remarkable for having ſtrong venereal deſires. This is univerſal among the negroes, hence their uncommon fruitfulneſs when they are not depreſſed by ſlavery; but even ſlavery in its worſt ſtate does not always ſubdue the venereal appetite, for after whole days, ſpent in hard

* Treatiſe upon Tropical Diſeaſes, p. 475.

labor

labor in a hot ſun in the Weſt Indies, the black men often walk five or ſix miles to comply with a venereal aſſignation.

6. The big lip, and flat noſe ſo univerſal among the negroes, are ſymptoms of the leproſy. I have more than once ſeen them in the Pennſylvania hoſpital.

7. The woolly heads of the negroes cannot be accounted for from climate, diet, ſtate of ſociety, or bilious diſeaſes, for all thoſe circumſtances, when combined have not produced it in the natives of Aſia and America who inhabit ſimilar latitudes. Wool is peculiar to the negro. Here the proofs of ſimilarity in the ſymptoms of leproſy, and in the peculiarities of the negro body appear to fail, but there is a fact in the hiſtory of the leproſy which will probably throw ſome light upon this part of our ſubject. The Trichoma, or Plica Polonica of the Poles is a ſymptom of leproſy. This is evident not only from the cauſes which originally produced it, but from its ſymptoms as deſcribed in a late publication by F. L. De La Fontaine.* From this fact it would ſeem that the leproſy had found its way to the covering of the head, and from the variety of its effects upon the ſkin, I ſee no difficulty in admitting that it may as readily have produced wool upon the head of a negro, as matted hair upon the head of the Poles.

But how ſhall we account for the long duration of this color of the ſkin through ſo many generations and even ages?—I anſwer—1. That the leproſy is the moſt durable in its deſcent to poſterity, and the moſt indeſtructable in its nature of any diſeaſe we are acquainted with. In Iceland Dr. Van Troil tells us, it often diſappears in the ſecond and third, and appears in the fourth generation.† 2dly. No more happens here than what happens to many nations

* Surgical and medical treatiſes upon various ſubjects reſpecting Poland.
† Letters on Iceland, p. 122.

who

who are diftinguifhed by a peculiarity of figure, in any part of the body. Many of the inhabitants of the highlands of Scotland, have the fame red hair, and the fame high cheek bones which are afcribed to their anceftors by Tacitus after the invafion of Britain. Even the tumors in the throat in the Cretins who inhabit the Alps, are tranfmitted from father to fon, through a long fucceffion of generations. Madnefs, and confumption in like manner are hereditary in many families, both of which occupy parts of the body, much more liable to change in fucceffive generations, than the fkin.

Should it be objected to this theory that the leprofy is an infectious diforder, but that no infectious quality exifts in the fkin of the negro, I would reply to fuch objection by remarking in the firft place, that the leprofy has in a great degree ceafed to be infectious, more efpecially from contact, and fecondly that there are inftances in which fomething like an infectious quality has appeared in the fkin of a negro. A white woman in North Carolina not only acquired a dark color, but feveral of the features of a negro, by marrying and living with a black hufband. A fimilar inftance of a change in the color and features of a woman in Buck's county in Pennfylvania has been obferved and from a fimilar caufe. In both thefe cafes, the women bore children by their black hufbands.

It is no objection to the theory I have attempted to eftablifh, that the negroes are as healthy, and long lived as the white people. Local difeafes of the fkin feldom affect the general health of the body, or the duration of human life. Dr. Theiry remarks that the itch, and even the leprofy, did not impair longevity in thofe people who lived near the fea-fhore in the healthy climate of Galicia.*

The facts and principles which I have delivered, lead to the following reflections.

* Vol. II. p. 171.

1. That

1. That all the claims of ſuperiority of the whites over the blacks, on account of their color, are founded alike in ignorance and inhumanity. If the color of the negroes be the effect of a diſeaſe, inſtead of inviting us to tyranniſe over them, it ſhould entitle them to a double portion of our humanity, for diſeaſe all over the world has always been the ſignal for immediate and univerſal compaſſion.

2. The facts and principles which have been delivered, ſhould teach white people the neceſſity of keeping up that prejudice againſt ſuch connections with them, as would tend to infect poſterity with any portion of their diſorder. This may be done upon the ground I have mentioned without offering violence to humanity, or calling in queſtion the ſameneſs of deſcent, or natural equality of mankind.

3. Is the color of the negroes a diſeaſe? Then let ſcience and humanity combine their efforts, and endeavour to diſcover a remedy for it. Nature has lately unfurled a banner upon this ſubject. She has begun ſpontaneous cures of this diſeaſe in ſeveral black people in this country. In a certain Henry Moſs who lately travelled through this city, and was exhibited as a ſhow for money, the cure was nearly complete. The change from black to a natural white fleſh color began about five years ago at the ends of his fingers, and has extended gradually over the greateſt part of his body. The wool which formerly perforated the cuticle has been changed into hair. No change in the diet, drinks, dreſs, employments, or ſituation of this man had taken place previouſly to this change in his ſkin. But this fact does not militate againſt artificial attempts to diſlodge the color in negroes, any more than the ſpontaneous cures of many other diſeaſes militate againſt the uſe of medicine in the practice of phyſic. To direct our experiments upon this ſubject I ſhall throw out the following facts.

1. In

1. In Henry Mofs the color was firft difcharged from the fkin in thofe places, on which there was moft preffure from cloathing, and moft attrition from labor, as on the trunk of his body, and on his fingers. The deftruction of the black color was probably occafioned by the abforption of the coloring matter of the rete mucofum, or perhaps of the rete mucofum itfelf, for preffure and friction it is well known aid the abforbing action of the lymphatics in every part of the body. It is from the latter caufe, that the palms of the hands of negro women who fpend their lives at a wafhing tub, are generally as fair as the palms of the hands in labouring white people.

2. Depletion, whether by bleeding, purging, or abftinence has been often obferved to leffen the black color in negroes. The effects of the above remedies in curing the common leprofy, fatisfy me that they might be ufed with advantage in that ftate of leprofy which I conceive to exift in the fkin of the negroes.

3. A fimilar change in the color of the negroes, though of a more temporary nature, has often been obferved in them from the influence of fear.

4. Dr. Beddoes tells us that he has difcharged the color in the black wool of a negro by infufing it in the oxygenated muriatic acid, and leffened it by the fame means in the hand of a negro man. The land-cloud of Africa called by the Portuguefe Ferrino Mr. Hawkins tells us has a peculiar action upon the negroes in changing the black color of their fkins to a dufky grey.* Its action is accompanied, he fays, with an itching and prickling fenfation upon every part of the body which increafes with the length of expofure to it fo as to be almoft intolerable. It is probably air of the carbonic kind, for it uniformly extinguifhes fire.

5. A

* P. 120. 121.

5. A citizen of Philadelphia upon whofe veracity I have perfect reliance,* affured me that he had once feen the fkin of one fide of the cheek inclining to the chin, and of part of the hand in a negro boy, changed to a white color by the juice of unripe peaches (of which he ate a large quantity every year) falling, and refting frequently upon thofe parts of his body.

To encourage attempts to cure this difeafe of the fkin in negroes, let us recollect that by fucceeding in them, we fhall produce a large portion of happinefs in the world. We fhall in the firft place deftroy one of the arguments in favor of enflaving the negroes, for their color has been fuppofed by the ignorant to mark them as objects of divine judgments, and by the learned to qualify them for labor in hot, and unwholfome climates.

Secondly, We fhall add greatly to *their* happinefs, for however well they appear to be fatisfied with their color, there are many proofs of their preferring that of the white people.

Thirdly, We fhall render the belief of the whole human race being defcended from one pair, eafy, and univerfal, and thereby not only add weight to the Chriftian revelation, but remove a material obftacle to the exercife of that univerfal benevolence which is inculcated by it.

June 17, 1797.

* Mr. Thomas Harrifon.

No. XXXVI.

An Improvement in Boats, for River-Navigation, deſcribed in a Letter to Mr. ROBERT PATTERSON, *by* NICHOLAS KING.

City of Waſhington, Sept. 28, 1797.

SIR,

Read Nov. 11, 1797.

AS ſurveyor of the city of Waſhington I was called upon, this ſpring, to aſcertain the difference of level, and the diſtances along the courſe of the intended canal at the Great Falls of the Potomac; that the lock-ſeats might be fixed, and the neceſſary excavations made. While engaged in this buſineſs, my thoughts were unavoidably led to the conſideration of the moſt elegible mode of navigating the American rivers above tide-water; and in ſituations ſimilar to this, where the falls are large and impaſſable, what mode might be adopted to facilitate the navigation, with the leaſt expence to the perſons intereſted therein.

The nature of the country, the rugged courſes of moſt of the rivers, and the ſudden ſwells they are liable to, from the heavy rains, render the lock-navigation, with towing-paths along the banks of the rivers, as in Europe, in moſt caſes impracticable, or very expenſive. Nature ſeems here to have precluded the inhabitants from other aſſiſtance in navigating rivers, than manual labour, expenſive and tedious. The difficulties attending the navigation of our rivers, againſt the current are ſuch as to render them much leſs ſerviceable than rivers in general are; and you are under the neceſſity of having the boats of great length and narrow; and of ſending with them double the number of hands required to navigate them when loaded, in order to enable them to *ſet* the boat up againſt the current

rent on their return. Theſe boats are more difficult to ſteer and manage, in intricate and rapid parts of the rivers, on account of their length; are ſubject to receive damage from ſtriking on rocks and ſand-banks, and from the uneven ſurface and motion of the water where the deſcent is rapid, or the weather boiſterous; and frequently get twiſted and ruined when the water ſubſides and leaves them on the ſhore.

Suppoſing that the lock-navigation, or overcoming the deſcent of the river by means of locks, could be generally reduced to practice, yet the length of theſe locks, in proportion to the tonnage of the boats, would render the expence of their conſtruction more than the revenue ariſing from the tolls would warrant: but few indeed are the rivers in which the navigation by locks is practicable, on account of the rapidity of the waters and violence of the freſhes. Hence it follows, that notwithſtanding this has hitherto been the moſt prevalent mode of overcoming ſuch obſtructions in rivers, it ought not to be attempted here, if better modes can be pointed out.

The improvements which have been made in canal-navigation within theſe few years in England and other parts of Europe, have, in my opinion, furniſhed us with the means of overcoming, at a little expence, the preſent impediments to our inland navigation; and, by facilitating the intercourſe, of joining the people of the United States in bonds more indiſſoluble than thoſe formed by legiſlative acts. Among the foremoſt of theſe improvements is that of *inclined planes*, over which the boat and cargo are carried from one level to the other. Theſe are conſtructed at much leſs expence than locks generally are; may be rendered more durable, and are the means of overcoming the greateſt difference of level; as it is attended with very little more labour or waſte of time to aſcend one hundred feet, than to aſcend twenty. The machines for tranſport-ing boats up theſe acclivities may be conſtructed in ſuch a manner, that the boats may ſtill continue to float therein; and all the danger of their being injured in removing from the water upon carriages avoided. Theſe things conſidered, it would certainly be highly advantageous to adopt the inclined plane at ſuch places in the rivers of America as have the navigation thereof impeded by falls: but the difficulty is, in ſo conſtructing them that boats of ſufficient length to be ſet up by men againſt the general current, may be drawn over the plane without injuring the boat or ſhifting the cargo. This cannot be done on account of the weight of the boats and their cargoes, and the length of the carriages on which they would have to be tranſported, as the angle of the carriage muſt be equal to that of the plane or ſlope on which they had to aſcend or deſcend.

The boat hereafter deſcribed is deſigned to anſwer every purpoſe of the *Durham-boat*, or that at preſent in uſe, with the advantage of being eaſily tranſported along an inclined ſurface, ſo as to overcome any impediment of fall in a navigable river without ſhifting the cargo, or injuring the boat. If it be found to poſſeſs this advantage, it will, notwithſtanding its novelty, be adopted by an enterprizing people; and who, from the nature of their country, are highly intereſted in promoting the means of internal communication.

Experience has proved to the perſons navigating the rivers in America, that boats from eighty to ninety feet in length; from ſix to eight feet in width, and eighteen inches or two feet deep, are the moſt proper: for, although boats of half that length would be eaſier navigated *with* the ſtream, they could not return on account of their wanting a ſufficient walk for the men who puſh them up *againſt* it, neceſſary to prevent their loſing way by the action of the ſtream. The boat I would recommend in

their

their ſtead is agreeable to the annexed drawings and deſcription:

Fig. 1. Repreſents four boats connected together, ſo as to form a boat of eighty feet in length.

Fig. 2. Shews the ſame boats as floating on the water; and the ſlopes of their ends, to admit their riſing or falling at the joints in rough water, or on ſtriking the ſand banks, paſſing a *riſt*, &c.

Fig. 3. Shews the manner of connecting the boats by hinges.

By thus dividing the preſent Durham-boat, into four diſtinct ones that may be uſed ſeparate or connected, at pleaſure, each part, with its loading, may be paſſed up an inclined plane with facility; as neither its weight nor length will be ſuch as to render the carriage for it unwieldy or unſafe. When uſed for bringing produce down the river, it may be divided at the middle, and the two parts, each forming a boat, navigated by three men, two to row and one to ſteer. By this diviſion they will come down with greater ſafety, be more eaſily managed in ſuch parts of the river as are difficult of navigation, from rocks, ſands or rapids; as it is allowed, by all thoſe who perform ſuch navigations, that a ſhort boat is conducted with infinitely more eaſe and ſafety than a long one. And as theſe boats, ſingly, are of a length eaſily managed, they may be kept in conſtant uſe, in fiſhing, ferrying, and the carrying of articles ſhort diſtances by oars only, at ſuch times as they are not all wanted in a lengthy tranſportation of commodities. They will be ſtronger with the ſame timbers as they are diminiſhed in length; and can at any time be eaſily drawn on land for the purpoſes of cleaning and repairing. When we add to theſe, the advantages attendant on this diviſion of the boat, when all the parts are uſed in conjunction and as one, I believe none will deny it a preference to thoſe of the old conſtruction: the benefits ariſing from the

the length are retained—by being divided and connected by hinges, each part may riſe or fall conſiderably without affecting the reſt, and can yield to the preſſure of any extraneous body without endangering the ſafety of any part.

If you think the communication of the ideas contained in this letter will in the leaſt contribute to the advantage of thoſe concerned in the internal navigations of America, it will be an additional motive with me to proſecute the ſubject; and in a little time I may be able to give you drawings of a ſimple machine for ſetting boats up againſt the ſtream, without the violent exertions now required, and which at the ſame time will prevent them from making ſtern way.

NICHOLAS KING.

MR. ROBERT PATTERSON,
Philadelphia,

General

No. XXXVII.

General Principles and Conſtruction of a Sub-marine Veſſel, communicated by D. Buſhnell of Connecticut, the inventor, in a letter of October, 1787, to THOMAS JEFFERSON *then Miniſter Plenipotentiary of the United States at Paris.*

Read June 8, 1798.

THE external ſhape of the ſub-marine veſſel bore ſome reſemblance to two upper tortoiſe ſhells of equal ſize, joined together; the place of entrance into the veſſel being repreſented by the opening made by the ſwell of the ſhells, at the head of the animal. The inſide was capable of containing the operator, and air, ſufficient to ſupport him thirty minutes without receiving freſh air. At the bottom oppoſite to the entrance was fixed a quantity of lead for ballaſt. At one edge which was directly before the operator, who ſat upright, was an oar for rowing forward or backward. At the other edge, was a rudder for ſteering. An aperture, at the bottom, with its valve, was deſigned to admit water, for the purpoſe of deſcending; and two braſs forcing-pumps ſerved to eject the water within, when neceſſary for aſcending. At the top, there was likewiſe an oar, for aſcending or deſcending, or continuing at any particular depth—A water-gauge or barometer, determined the depth of deſcent, a compaſs directed the courſe, and a ventilator within, ſupplied the veſſel with freſh air, when on the ſurface.

The entrance into the veſſel was elliptical, and ſo ſmall as barely to admit a perſon. This entrance was ſurrounded with a broad elliptical iron band, the lower edge of which was let into the wood of which the body of the veſſel was made, in ſuch a manner, as to give its utmoſt ſupport to the body of the veſſel againſt the preſſure of the water. Above the upper edge of this iron band, there was a braſs crown, or cover, reſembling a hat with its crown and brim, which

which ſhut water tight upon the iron band: the crown was hung to the iron band with hinges ſo as to turn over ſidewiſe, when opened. To make it perfectly ſecure when ſhut, it might be ſcrewed down upon the band by the operator, or by a perſon without.

There were in the braſs crown, three round doors, one directly in front, and one on each ſide, large enough to put the hand through—when open they admitted freſh air; their ſhutters were ground perfectly tight into their places with emery, hung with hinges and ſecured in their places when ſhut. There were likewiſe ſeveral ſmall glaſs windows in the crown, for looking through. and for admitting light in the day time, with covers to ſecure them. There were two air pipes in the crown. A ventilator within drew freſh air through one of the air pipes, and diſcharged it into the lower part of the veſſel; the freſh air introduced by the ventilator, expelled the impure light air through the other air pipe. Both air pipes were ſo conſtructed, that they ſhut themſelves whenever the water roſe near their tops, ſo that no water could enter through them, and opened themſelves immediately after they roſe above the water.

The veſſel was chiefly ballaſted with lead fixed to its bottom; when this was not ſufficient, a quantity was placed within, more or leſs, according to the weight of the operator; its ballaſt made it ſo ſtiff, that there was no danger of overſetting. The veſſel with all its appendages, and the operator, was of ſufficient weight to ſettle it very low in the water. About two hundred pounds of the lead, at the bottom, for ballaſt, would be let down forty or fifty feet below the veſſel; this enabled the operator to riſe inſtantly to the ſurface of the water, in caſe of accident.

When the operator would deſcend, he placed his foot upon the top of a braſs valve, depreſſing it, by which he opened a large aperture in the bottom of the veſſel, through which the water entered at his pleaſure; when he had admitted

mitted a ſufficient quantity, he deſcended very gradually; if he admitted too much, he ejected as much as was neceſſary to obtain an equilibrium, by the two braſs forcing pumps, which were placed at each hand. Whenever the veſſel leaked, or he would aſcend to the ſurface, he alſo made uſe of theſe forcing pumps. When the ſkilful operator had obtained an equilibrium, he could row upward, or downward, or continue at any particular depth, with an oar, placed near the top of the veſſel, formed upon the principle of the ſcrew, the axis of the oar entering the veſſel; by turning the oar one way he raiſed the veſſel, by turning it the other way he depreſſed it.

A glaſs tube eighteen inches long, and one inch in diameter, ſtanding upright, its upper end cloſed, and its lower end, which was open, ſcrewed into a braſs pipe, through which the external water had a paſſage into the glaſs tube, ſerved as a water-gauge or barometer. There was a piece of cork with phoſphorus on it, put into the water-gauge. When the veſſel deſcended the water roſe in the water-gauge, condenſing the air within, and bearing the cork, with its phoſphorus, on its ſurface. By the light of the phoſphorus, the aſcent of the water in the gauge was rendered viſible, and the depth of the veſſel under water aſcertained by a graduated line.

An oar, formed upon the principle of the ſcrew, was fixed in the forepart of the veſſel; its axis entered the veſſel, and being turned one way, rowed the veſſel forward, but being turned the other way rowed it backward; it was made to be turned by the hand or foot.

A rudder, hung to the hinder part of the veſſel, commanded it with the greateſt eaſe. The rudder was made very elaſtic, and might be uſed for rowing forward. Its tiller was within the veſſel, at the operator's right hand, fixed, at a right angle, on an iron rod, which paſſed through the ſide of the veſſel; the rod had a crank on its

 outſide

outſide end, which commanded the rudder, by means of a rod extending from the end of the crank to a kind of tiller, fixed upon the left hand of the rudder. Raiſing and depreſſing the firſt mentioned tiller turned the rudder as the caſe required.

A compaſs marked with phoſphorus directed the courſe, both above and under the water; and a line and lead ſounded the depth when neceſſary.

The internal ſhape of the veſſel, in every poſſible ſection of it, verged towards an ellipſis, as near as the deſign would allow, but every horizontal ſection, although elliptical, yet as near to a circle, as could be admitted. The body of the veſſel was made exceedingly ſtrong; and to ſtrengthen it as much as poſſible, a firm piece of wood was framed, parallel to the conjugate diameter, to prevent the ſides from yielding to the great preſſure of the incumbent water, in a deep immerſion. This piece of wood was alſo a ſeat for the operator.

Every opening was well ſecured. The pumps had two ſets of valves. The aperture at the bottom, for admitting water, was covered with a plate, perforated full of holes to receive the water, and prevent any thing from choaking the paſſage, or ſtopping the valve from ſhutting. The braſs valve might likewiſe be forced into its place with a ſcrew, if neceſſary. The air pipes had a kind of hollow ſphere, fixed round the top of each, to ſecure the air-pipe valves from injury: theſe hollow ſpheres were perforated full of holes for the paſſage of the air through the pipes: within the air-pipes were ſhutters to ſecure them, ſhould any accident happen to the pipes, or the valves on their tops.

Wherever the external apparatus paſſed through the body of the veſſel, the joints were round, and formed by braſs pipes, which were driven into the wood of the veſſel, the holes through the pipes were very exactly made, and the iron rods, which paſſed through them, were turned in

a

a lathe to fit them ; the joints were alſo kept full of oil, to prevent ruſt and leaking. Particular attention was given to bring every part, neceſſary for performing the operations, both within and without the veſſel, before the operator, and as conveniently as could be deviſed ; ſo that every thing might be found in the dark, except the water-gauge and the compaſs, which were viſible by the light of the phoſphorus, and nothing required the operator to turn to the right hand, or to the left, to perform any thing neceſſary.

No. 2.

Deſcription of a magazine and its appendages, deſigned to be conveyed by the ſub-marine veſſel to the bottom of a ſhip.

In the forepart of the brim of the crown of the ſub-marine veſſel, was a ſocket, and an iron tube, paſſing through the ſocket ; the tube ſtood upright, and could ſlide up and down in the ſocket, ſix inches: at the top of the tube, was a wood-ſcrew (A) fixed by means of a rod, which paſſed through the tube, and ſcrewed the wood-ſcrew faſt upon the top of the tube: by puſhing the wood-ſcrew up againſt the bottom of a ſhip, and turning it at the ſame time, it would enter the planks ; driving would alſo anſwer the ſame purpoſe ; when the wood-ſcrew was firmly fixed, it could be caſt off by unſcrewing the rod, which faſtened it upon the top of the tube.

Behind the ſub-marine veſſel, was a place, above the rudder, for carrying a large powder magazine, this was made of two pieces of oak timber, large enough when hollowed out to contain one hundred and fifty pounds of powder, with the apparatus uſed in firing it, and was ſecured in its place by a ſcrew, turned by the operator. A ſtrong piece of rope extended from the magazine to the wood-ſcrew (A) above mentioned, and was faſtened to both.

When the wood-ſcrew was fixed, and to be caſt off from its tube, the magazine was to be caſt off likewiſe by unſcrewing it, leaving it hanging to the wood-ſcrew; it was lighter than the water, that it might riſe up againſt the object, to which the wood-ſcrew and itſelf were faſtened.

Within the magazine was an apparatus, conſtructed to run any propoſed length of time, under twelve hours; when it had run out its time, it unpinioned a ſtrong lock reſembling a gun lock, which gave fire to the powder. This apparatus was ſo pinioned, that it could not poſſibly move, till, by caſting off the magazine from the veſſel, it was ſet in motion.

The ſkilful operator could ſwim ſo low on the ſurface of the water, as to approach very near a ſhip, in the night, without fear of being diſcovered, and might, if he choſe, approach the ſtem or ſtern above water, with very little danger. He could ſink very quickly, keep at any depth he pleaſed, and row a great diſtance in any direction he deſired, without coming to the ſurface, and when he roſe to the ſurface, he could ſoon obtain a freſh ſupply of air, when, if neceſſary, he might deſcend again, and purſue his courſe.

No. 3.

Experiments made to prove the nature and uſe of a ſub-marine veſſel.

The firſt experiment I made, was with about two ounces of gun powder, which I exploded 4 feet under water, to prove to ſome of the firſt perſonages in Connecticut, that powder would take fire under water.

The ſecond experiment was made with two pounds of powder, incloſed in a wooden bottle, and fixed under a hogſhead, with a two inch oak plank between the hogſhead and

and the powder; the hogſhead was loaded with ſtones as deep as it could ſwim; a wooden pipe deſcending through the lower head of the hogſhead, and through the plank, into the powder contained in the bottle, was primed with powder. A match put to the priming, exploded the powder, which produced a very great effect, rending the plank into pieces; demoliſhing the hogſhead; and caſting the ſtones and the ruins of the hogſhead, with a body of water, many feet into the air, to the aſtoniſhment of the ſpectators. This experiment was likewiſe made for the ſatisfaction of the gentlemen above mentioned.

I afterwards made many experiments of a ſimilar nature, ſome of them with large quantities of powder; they all produced very violent exploſions, much more than ſufficient for any purpoſe I had in view.

In the firſt eſſays with the ſub-marine veſſel, I took care to prove its ſtrength to ſuſtain the great preſſure of the incumbent water, when ſunk deep, before I truſted any perſon to deſcend much below the ſurface: and I never ſuffered any perſon to go under water, without having a ſtrong piece of rigging made faſt to it, until I found him well acquainted with the operations neceſſary for his ſafety. After that, I made him deſcend and continue at particular depths, without riſing or ſinking, row by the compaſs, approach a veſſel, go under her, and fix the *wood-ſcrew* mentioned in No. 2, and marked A, into her bottom, &c. until I thought him ſufficiently expert to put my deſign into execution.

I found, agreeably to my expectations, that it required many trials to make a perſon of common ingenuity, a ſkilful operator: the firſt I employed, was very ingenious, and made himſelf maſter of the buſineſs, but was taken ſick in the campaign of 1776, at New-York, before he had an opportunity to make uſe of his ſkill, and never recovered his health ſufficiently, afterwards.

Experiments

Experiments made with a ſub-marine veſſel.

After various attempts to find an operator to my wiſh, I ſent one who appeared more expert than the reſt, from New-York, to a 50 gun ſhip lying not far from Governor's Iſland. He went under the ſhip, and attempted to fix the wooden ſcrew into her bottom, but ſtruck, as he ſuppoſes, a bar of iron, which paſſes from the rudder hinge, and is ſpiked under the ſhip's quarter. Had he moved a few inches, which he might have done, without rowing, I have no doubt but he would have found wood where he might have fixed the ſcrew; or if the ſhip were ſheathed with copper, he might eaſily have pierced it: but not being well ſkilled in the management of the veſſel, in attempting to move to another place, he loſt the ſhip; after ſeeking her in vain, for ſome time, he rowed ſome diſtance, and roſe to the ſurface of the water, but found day light had advanced ſo far, that he durſt not renew the attempt. He ſays that he could eaſily have faſtened the magazine under the ſtem of the ſhip, above water, as he rowed up to the ſtern, and touched it before he deſcended. Had he faſtened it there, the exploſion of one hundred and fifty pounds of powder, (the quantity contained in the magazine), muſt have been fatal to the ſhip. In his return from the ſhip to New-York, he paſſed near Governor's Iſland, and thought he was diſcovered by the enemy, on the iſland; being in haſte to avoid the danger he feared, he caſt off the magazine, as he imagined it retarded him in the ſwell, which was very conſiderable. After the magazine had been caſt off one hour, the time the internal apparatus was ſet to run, it blew up with great violence.

Afterwards, there were two attempts made in Hudſon's river, above the city, but they effected nothing. One of them was by the aforementioned perſon. In going towards

wards the ſhip, he loſt ſight of her, and went a great diſtance beyond her: when he at length found her, the tide ran ſo ſtrong, that as he deſcended under water, for the ſhip's bottom—it ſwept him away. Soon after this, the enemy went up the river, and purſued the boat which had the ſub-marine veſſel on board—and ſunk it with their ſhot. Though I afterwards recovered the veſſel, I found it impoſſible, at that time, to proſecute the deſign any farther. I had been in a bad ſtate of health, from the beginning of my undertaking, and was now very unwell; the ſituation of public affairs was ſuch, that I deſpaired of obtaining the public attention, and the aſſiſtance neceſſary. I was unable to ſupport myſelf, and the perſons I muſt have employed, had I proceeded. Beſides, I found it abſolutely neceſſary, that the operators ſhould acquire more ſkill in the management of the veſſel, before I could expect ſucceſs; which would have taken up ſome time, and made no ſmall additional expenſe. I therefore gave over the purſuit for that time, and waited for a more favorable opportunity, which never arrived.

Other Experiments made with a deſign to fire Shipping.

In the year 1777, I made an attempt from a whale-boat, againſt the Cerberus frigate, then lying at anchor between Connecticut river and New London, by drawing a machine againſt her ſide, by means of a line. The machine was loaded with powder, to be exploded by a gun-lock, which was to be unpinioned by an apparatus, to be turned by being brought along ſide of the frigate. This machine fell in with a ſchooner at anchor, aſtern of the frigate, and concealed from my ſight. By ſome means or other, it was fired, and demoliſhed the ſchooner and

and three men—and blew the only one left alive, overboard, who was taken up very much hurt.

After this, I fixed ſeveral kegs, under water, charged with powder, to explode upon touching any thing, as they floated along with the tide: I ſet them afloat in the Delaware, above the Engliſh ſhipping at Philadelphia, in December, 1777. I was unacquainted with the river, and obliged to depend upon a gentleman very imperfectly acquainted with that part of it, as I afterwards found. We went as near the ſhipping as he durſt venture; I believe the darkneſs of the night greatly deceived him, as it did me. We ſet them adrift, to fall with the ebb, upon the ſhipping. Had we been within ſixty rods, I believe they muſt have fallen in with them immediately, as I deſigned; but as I afterwards found, they were ſet adrift much too far diſtant, and did not arrive, until after being detained ſome time by froſt, they advanced in the day time, in a diſperſed ſituation, and under great diſadvantages. One of them blew up a boat, with ſeveral perſons in it, who imprudently handled it too freely, and thus gave the Britiſh that alarm, which brought on *the battle of the Kegs.*

The above Veſſel, Magazine, &c. were projected in the year 1771, but not completed, until the year 1775.

D. BUSHNELL.

The

No. XXXVIII.

The description of a Mould-board of the least resistence, and of the easiest and most certain construction, taken from a letter to Sir John Sinclair, President of the board of agriculture at London.

Philadelphia, March 23, 1798.

Dear Sir,

Read May 4, 1798. I have to acknowledge the receipt of your two favours of June 21, and July 15, and of several separate parcels containing the agricultural reports. These now form a great mass of information on a subject, of all in the world, the most interesting to man: for none but the husbandman makes any thing for him to eat; and he who can double his food, as your exertions bid fair to do, deserves to rank, among his benefactors, next after his Creator. Among so many reports of transcendent merit, one is unwilling to distinguish particulars. Yet the application of the new chemistry, to the subject of manures, the discussion of the question on the size of farms, the treatise on the potatoe, from their universality have an advantage in other countries over those which are topographical. The work which shall be formed, as the result of the whole, we shall expect with impatience.

Permit me, through you, to make here my acknowledgments to the board of agriculture for the honour they have been pleased to confer on me by, associating me to their institution. In love for the art, I am truly their associate: but events have controuled my predilection for its practice, and denied to me that uninterrupted attention, which alone can enable us to advance in it with a sure step. Perhaps I may find opportunities of being useful to you as a centinel at an outpost, by conveying intelligence of whatever may occur here new and interesting to agriculture. This duty I shall perform with pleasure, as well in respectful return for the notice of the board, as from a zeal for improving the condition of human life, by an interchange of its comforts, and of the information which may increaſe them.

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In a former letter to you I mentioned the conſtruction of the mould-board of a plough which had occurred to me, as advantageous in its form, as certain and invariable in the method of obtaining it with preciſion. I remember that Mr. Strickland of York, a member of your board, was ſo well ſatisfied with the principles on which it was formed that he took ſome drawings of it; and ſome others have conſidered it with the ſame approbation. An experience of five years has enabled me to ſay, it anſwers in practice to what it promiſes in theory. The mould-board ſhould be a continuation of the wing of the ploughſhare, beginning at its hinder edge, and in the ſame plane. Its firſt office is to receive the ſod horizontally from the wing, to raiſe it to a proper height for being turned over, and to make, in its progreſs, *the leaſt reſiſtence poſſible;* and conſequently to require a minimum in the moving power. Were this its only office, the wedge would offer itſelf as the moſt * eligible form in practice. But the ſod is to be turned over alſo. To do this, the one edge of it is not to be raiſed at all; for to raiſe this would be a waſte of labour. The other edge is to be raiſed till it paſſes the perpendicular,

* I am aware that were the turf only *to be raiſed* to a given height in a given length of mould-board, and not to be turned over, the form of leaſt reſiſtence would not be rigorouſly a wedge with both faces ſtraight, but with the upper one curved according to the laws of the ſolid of leaſt reſiſtence deſcribed by the mathematicians. But the difference between the effect of the curved and of the plain wedge, in the caſe of a mould-board, is ſo minute, and the difficulty in the execution which the former would ſuperinduce on common workmen is ſo great, that the plain wedge is the moſt eligible to be aſſumed in practice for the firſt element of our conſtruction.

perpendicular, that it may fall over with its own weight. And that this may be done ſo as to give alſo the leaſt reſiſtence, it muſt be made to riſe gradually from the moment the ſod is received. The mould-board then, in this ſecond office, operates as a tranſverſe, or riſing wedge, the point of which ſliding back horizontally on the ground, the other end continues riſing till it paſſes the perpendicular. Or, to vary the point of view, place on the ground a wedge of the breadth of the ploughſhare, of its length from the wing backwards, and as high at the heel as it is wide. Draw a diagonal on its upper face from the left angle at the point to the right upper angle of the heel. Bevil the face from the diagonal to the right-bottom-edge which lies on the ground. That half is then evidently in the beſt form for performing the two offices of raiſing and turning the ſod gradually, and with the leaſt effort: and if you will ſuppoſe the ſame bevil continued acroſs the left ſide of the diagonal, that is, if you will ſuppoſe a ſtraight line whoſe length is at leaſt equal to the breadth of the wedge, applied on the face of the firſt bevil and moved backwards on it parallel with itſelf and with the ends of the wedge, the lower end of the line moving along the right-bottom-edge, a curved plane will be generated, whoſe characteriſtic will be a combination of the principle of the wedge in croſs directions, and will give what we ſeek, the *mould-board of leaſt reſiſtence.* It offers too this great advantage, that it may be made by the coarſeſt workman, by a proceſs ſo exact that its form ſhall never be varied a ſingle hair's breadth. One fault of all other mould-boards is that, being copied by the eye, no two will be alike. In truth it is eaſier to form the mould-board I ſpeak of with preciſion, when the method has been once ſeen, than to deſcribe that method either by words or figures. I will attempt however to deſcribe it. Whatever may not be intelligible from the deſcription may be ſupplied from the model I ſend you.

Let the breadth and depth of the furrow the farmer uſually opens, as alſo the length of his plough-bar, from where it joins the wing to the hinder end, be given; as theſe fix the dimenſions of the block of which the mould-board is to be made. Suppoſe the furrow 9 inches wide, 6 inches deep, and the plough-bar 2 feet long. Then the block, Fig. 1. muſt be 9 inches wide at bottom (*b. c.*) 13½ inches wide at top, (*a. d.*) becauſe if it were merely of the ſame width with the bottom as *a. e.* the ſod, only raiſed to the perpendicular, would fall back into the furrow by its own elaſticity. I find from experience, that, in my ſoil, the top of the mould-board ſhould overjet the perpendicular 4½ inches in a height of 12 inches, to inſure that the weight of the ſod ſhall preponderate over its elaſticity. This is an angle of nearly 22°. The block muſt be 12 inches high, becauſe, unleſs the mould-board be in height double the depth of the furrow, in ploughing friable earth, it will be thrown in waves over the mould-board: and it muſt be 3 feet long, one foot of which is added to form a tail-piece, by which it may be made faſt to the plough-handle. The firſt operation is to give the firſt form to this tail-piece, by ſawing the block, Fig. 2. acroſs from *a. b.* on its left ſide, (which is 12 inches from its hinder end) along the line *b. c.* to *c.* within 1½ inches of the right ſide, and to the correſponding point in the bottom, 1½ inches alſo from the ſide. Then ſaw in again at the hinder end from *d. e.* (1½ inches from the right ſide) along the line *d. c.* The block *a. b. c. d. e. f. g.* drops out and leaves the tail-piece *c. d. e. h. i. k.* 1½ inches thick. The fore part of the block *a. b. c. k. l. m. n.* is what is to form the real mould-board. With a carpenter's ſquare make a ſcribe all round the block at every inch. There will of courſe be 23 of them. Then from the point *k.* Fig. 2. and 3. draw the diagonals *k. m.* on the top, and *k. o.* Fig. 3. on the right ſide. Enter a ſaw at the

the point *m.* being the left-fore-upper corner, and ſaw in, guiding the hinder part of the ſaw along the diagonal *m. k.* (Fig. 2. 3.) and the fore part down the left edge of the block at the fore-end *m. l.* (Fig. 2.) till it reaches *k.* and *l.* in a ſtraight line. It will then have reached the true central diagonal of the block *k. l.* Fig. 5. then enter the ſaw at the point *o.* being the right-fore-bottom corner, and ſaw in, guiding the hinder part of the ſaw along the diagonal *o. k.* (Fig 3.) and the fore part along the bottom edge of the fore end *o. l.* till it again reaches *k. l.* Fig. 5. the ſame central diagonal to which you had cut in the other direction. Conſequently the pyramid *k. m. n. o. l.* Fig. 4. drops out and leaves the block in the form Fig. 5. You will now obſerve that if in the laſt operation, inſtead of ſtopping the ſaw at the central diagonal *k. l.* we had cut through the block in the ſame plane, we ſhould have taken off a wedge *l. m. n. o. k. b.* Fig. 3. and left the block in the form of a wedge alſo *l. o. k. b. a. p. k.* which, when ſpeaking of the principle of the mould-board, I obſerved would be the moſt perfect form if it had only to raiſe the ſod. But as it is to be turned over alſo, the left half of the upper wedge is preſerved, to furniſh on the left ſide, the continuation of the bevil which was propoſed to be made on the right half of the bottom wedge. We are now to proceed to the bevil, for which purpoſe the ſcribes round the block were formed before the pyramidal piece was taken out; and attention muſt be uſed not to miſmatch or miſtake them, now that they are disjoined by the withdrawing of that piece. Enter the ſaw on the two points of the 1ſt ſcribe where it has been disjoined, which is exactly where it interſected the two ſuperficial diagonals, and ſaw acroſs the hollow of the block, guiding the ſaw, both before and behind, along the ſame ſcribe, till the fore part of the ſaw reaches the bottom edge of the right ſide, and the middle of the ſaw reaches the central diagonal; the

the hinder part will of courſe continue the ſame ſtraight line, which will iſſue ſomewhere on the top of the block. Then enter the ſaw in like manner on the two projecting points of the 2d ſcribe, and ſaw in, along the ſcribe, before and behind, till it reaches the ſame bottom edge of the right ſide, and the central diagonal. Then the 3d, 4th, 5th, &c. ſcribes ſucceſſively. After cutting in ſeveral of the earlier ſcribes, the hinder part of the ſaw will iſſue at the left ſide of the block, and all the ſcribes being cut, the ſaw will have left ſtraight lines from the bottom edge of the right ſide of the block, acroſs the central diagonal. With an adze dub off all the ſawed parts to the bottoms of the ſaw-marks, juſt leaving the traces viſible, and the face of the mould-board is finiſhed. Theſe traces will ſhew how the croſs wedge riſes gradually on the face of the direct wedge, which is preſerved in trace of the central diagonal. A perſon may repreſent to himſelf, ſenſibly and eaſily the manner in which the ſod is raiſed on this mould-board, by deſcribing on the ground a parallelogram 2 feet long and 9 inches broad, as *a. b. c. d.* Fig. 6. then reſt one end of a ſtick 27½ inches long on the ground at *b.* and raiſe the other 12 inches high at *e.* which is 4½ inches from *d*, and repreſents the overhanging of that ſide of the mould-board. Then preſent another ſtick 12 inches long from *a.* to *b.* and move it backwards parallel with itſelf from *a. b.* to *d. c.* keeping one end of it always on the line *a. d.* and letting the other riſe as it recedes along the diagonal ſtick *b. e.* which repreſents our central diagonal. The motion of the croſs ſtick will be that of our riſing wedge, and will ſhew how every tranſverſe line of the ſod is conducted from its firſt horizontal poſition, till it is raiſed ſo far beyond the perpendicular as to fall reverſed by its own weight. But to return to our work. We have ſtill to form the under ſide of the mould-board. Turn the block bottom up. Enter the

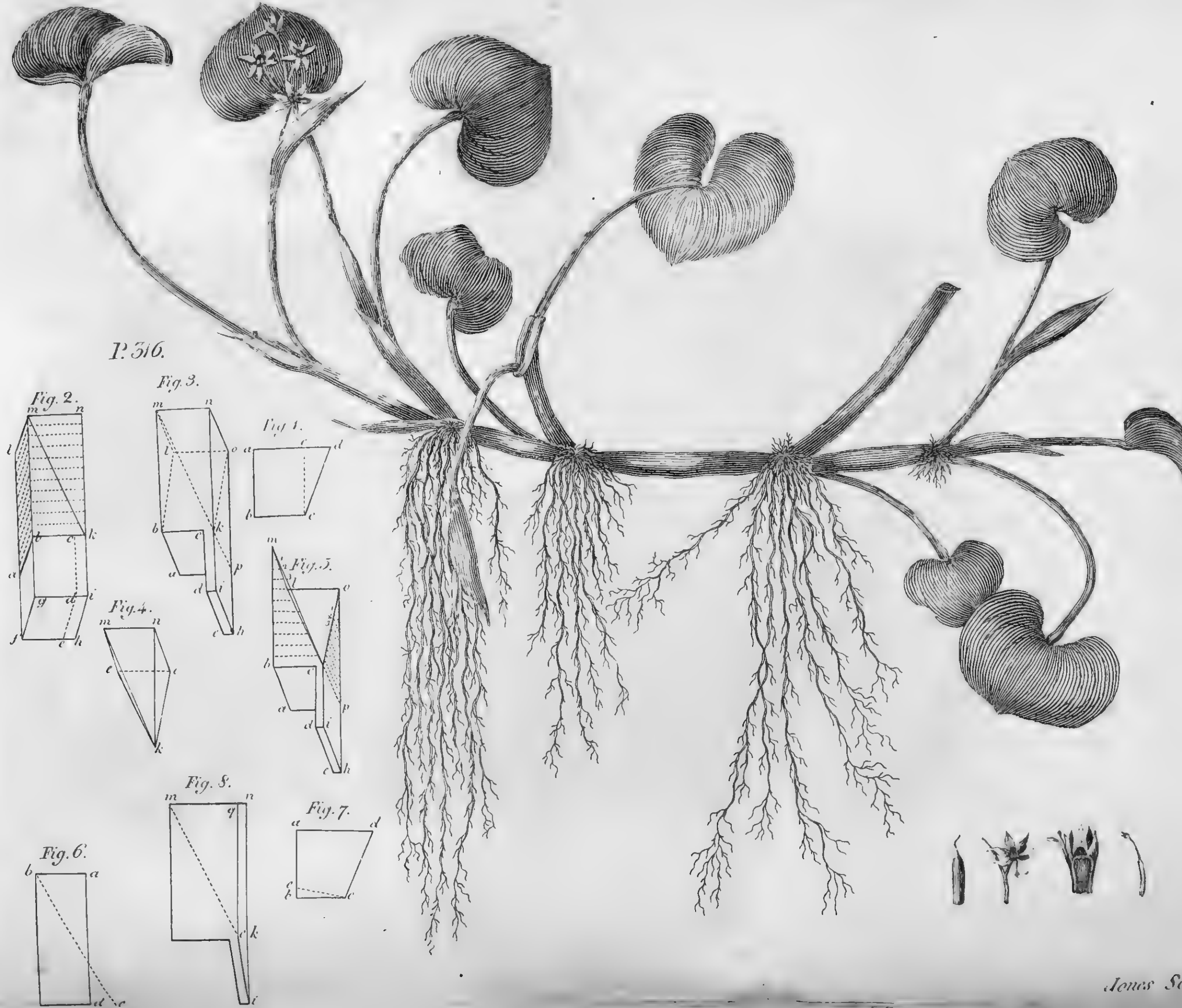
the ſaw on the 1ſt ſcribe, at what was the bottom edge of the left ſide, and cut in, guiding the inſtrument at both ends by the ſcribe, till it has approached within an inch, or any other diſtance according to the thickneſs you chooſe, of the face. Then cut in like manner all the other ſcribes, and with the adze dub out the ſawed parts, and the mould-board is done. It is to be made faſt to the plough by reſting the toe in the hinder edge of the wing, which muſt be made double like a comb-caſe, to receive and protect the fore end of the mould-board. Then paſs a ſcrew through the mould-board and helve of the ploughſhare where they touch each other, and two others through the tail-piece of the mould-board and right handle of the plough, and cut off ſo much of the tail-piece as projects behind the handle, diagonally, and the whole is done.

I have deſcribed this operation in its ſimpleſt mode, that it might be the more eaſily underſtood. But, in practice, I have found ſome other modifications of it advantageous. Thus, inſtead of firſt forming my block as *a. b. c. d.* Fig. 7. where *a. b.* is 12 inches, and the angle at *b.* a right one, I cut a wedge-like piece *b. c. e.* off of the bottom through the whole length of the block, *b. e.* being equal to the thickneſs of the bar of the ſhare (ſuppoſe $1\frac{1}{2}$ inches) becauſe the face of the wing declining from the top of the bar to the ground, were the block laid on the ſhare, without an equivalent bevil at its bottom, the ſide *a. b.* would decline from the perpendicular, and *a. d.* from its horizontal poſition. Again, inſtead of leaving the top of the block $13\frac{1}{2}$ inches wide from *m.* to *n.* Fig. 8. I cut a wedge from the right ſide *n. k. i. c. p. n.* $1\frac{1}{2}$ inches thick at top and tapering to nothing at bottom; becauſe I find that the tail-piece, being by this means made oblique, as *c. i.* inſtead of *k. i.* is brought more advantageouſly to the ſide of the handle. The firſt ſuperficial diagonal is conſequently brought from *m.* to *c.* and

not

not from *m.* to *k.* as in the first directions. These variations will be easy to any one after understanding the general principle. While these mould-boards have been under trial, and essays have been making of greater or less projections for the upper right edge of the block, and of different heights in proportion to the depth of the furrow, I have continued to make them of wood. But now satisfied by a sufficient experience, that for a furrow of 9 by 6 inches, the dimensions I have stated are the best, I propose to have the mould-board made of cast iron.

I am sensible that this description may be thought too lengthy and elaborate for a subject which has hardly been deemed worthy the application of science. But if the plough be in truth the most useful of the instruments known to man, its perfection cannot be an idle speculation. And in any case whatever, the combination of a *theory* which may satisfy the learned, with a *practice* intelligible to the most unlettered labourer, will be acceptable to the two most useful classes of society. Be this as it may, from the widow her mite only was expected. I have contributed according to my poverty; others will from their abundance.—None so much as yourself, who have been the animating principle of the institution from its first germ. When I contemplate the extensive good which the proceedings under your direction are calculated to produce, I cannot but deplore every possibility of their interruption. I am fixed in awe at the mighty conflict to which two great nations are advancing, and recoil with horror at the ferociousness of man. Will nations never devise a more rational umpire of differences than force? Are there no means of coercing injustice more gratifying to our nature than a waste of the blood of thousands, and of the labour of millions of our fellow-creatures? We see numerous societies of men (the aboriginals of this country) living together without the acknowledgment of either laws or magistracy. Yet they live in peace



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peace among themſelves, and acts of violence and injury are as rare in their ſocieties as in nations which keep the ſword of the law in perpetual activity. Public reproach, a refuſal of common offices, interdiction of the commerce and comforts of ſociety are found as effectual as the coarſer inſtrument of force. Nations, like theſe individuals, ſtand towards each other only in the relations of natural right. Might they not, like them, be peaceably puniſhed for violence and wrong? Wonderful has been the progreſs of human improvement in other lines. Let us hope then that that law of nature which makes a virtuous conduct produce benefit, and vice loſs, to the agent in the long run, which has ſanctioned the common principle that honeſty is the beſt policy, will in time influence the proceedings of nations as well as of individuals; and that we ſhall at length be ſenſible that war is an inſtrument entirely inefficient towards redreſſing wrong; that it multiplies inſtead of indemnifying loſſes. Had the money which has been ſpent in the preſent war been employed in making roads and conducting canals of navigation and irrigation through the country, not a hovel in the remoteſt corner of the Highlands of Scotland, or mountains of Auvergne, would have been without a boat at its door, a rill of water in every field, and a road to its market town. Had the money we have loſt by the lawleſs depredations of all the belligerent powers been employed in the ſame way, what communications would have been opened of roads and waters! Yet were we to go to war for redreſs, inſtead of redreſs, we ſhould plunge deeper into loſs, and diſable ourſelves for half a century more from attaining the ſame end. A war would coſt us more than would cut through the iſthmus of Darien; and that of Suez might have been opened with what a ſingle year has ſeen thrown away on the rock of Gibraltar. Theſe truths are palpable, and muſt in the progreſs of time have their influence on the minds and conduct of nations. An evidence that we are advancing towards a better ſtate of things may be gathered from the public patronage of your labours, which tend eminently to ameliorate the condition of man. That they may meet the ſucceſs they merit, I ſincerely pray, and that yourſelf may receive the patriot's beſt reward, the applauding voice of preſent and future times. Accept, I beſeech you, mine, with aſſurances of the ſentiments of great and ſincere reſpect and eſteem with which I have the honour to be, Dear Sir,

Your affectionate friend,

and humble ſervant,

TH. JEFFERSON.

Experiments

No. XXXIX.

Experiments upon Magnetiſm. Communicated in a Letter to THOMAS JEFFERSON, *Preſident of the Philoſophical Society, by the Rev.* JAMES MADISON, *Preſident of William and Mary College.*

April, 1798.

DEAR SIR,

Read May 4, 1798. IN the review of my philoſophical courſe for this year, I have been led to make ſome experiments upon magnetiſm, which appeared to fall within the views of the American Philoſophical Society. It is for this reaſon that I have taken the liberty of communicating them to you.

Few ſubjects in natural philoſophy are, in reality, more intereſting to mankind than magnetiſm; and yet, the inviſibility of the agent, and the conſequent difficulty of purſuing a cauſe, which ſeems to be ſubjected to none of our ſenſes, has greatly reſtricted the progreſs of knowledge in this branch of phyſics. Some phenomena, which have long attracted the attention of the philoſopher, and excited the admiration of the vulgar, have received different ſolutions, grounded upon as many different hypotheſes. No method appears to have been adopted to ſhew the fallacy of the one, or the verity of the other. They ſtill remained as hypotheſes. The following experiments, ſimple as they are, give a ſolution, which carries with it occular demonſtration; and, as the juſt explication of every fact is a real advance in philoſophy, I truſt they will not be thought unworthy of attention.

The phenomena to which I allude are thoſe, which magnets, placed upon paper, exhibit with filings of iron, when they are ſprinkled about them. Many ingenious men have ſuppoſed, that the arrangement of the filings clearly indicated the paſſage of a magnetic fluid or effluvia, in curved lines, from one pole to another of a different denomination; and hence, have ſought for the ſolution of other phenomena upon a much more extended ſcale. Others, and particularly Cavallo, in his uſeful treatiſe on magnetiſm, from the action of the magnet upon the filings, rendering each particle itſelf a magnet. The proper ſolution ſprings from the following experiments:

EXPERIMENT I.

Place an artificial magnet, of the uſual oblong form, and of ſufficient ſtrength, upon a diſh; let the ends be two or more inches diſtance from the edge or rim; cover the magnet with water about one-tenth of an inch. Then ſprinkle, or ſift the filings of iron through gauze, ſo that they may fall gently near the equator of the magnet. You will immediately ſee the filings to divide; one part moving with an accelerated velocity to the north pole; the other part, to the ſouth pole; each approaching as near to its reſpective pole, as the interpoſing fluid will permit; each turning and preſenting a diſſimilar pole to that which firſt put the particle in motion. As other particles ſucceed in their turn, the ſame effects take place; each endeavours to approach as near to its pole, as its centre, as poſſible; but the particles already arrived, preventing an approach within the limits thus previouſly occupied, the whole are neceſſarily arranged in the form of concentric circles. Particles of water in forming drops, or of mercury in forming globules, obey nearly the ſame law.

Thus doth this experiment unequivocally demonſtrate, from the motions of the floating particles, that attraction is the ſole cauſe of this phenomenon, and that this force is equally exerted by each pole. It ſhews, at the ſame

time,

time, that each of the filings, even the ſmalleſt, becomes itſelf a magnet, ſo ſoon as it arrives within a ſhort diſtance of its attracting pole. This is particularly evinced by thoſe particles which are firſt put in motion, and which occupy the neareſt ſtations. For, immediately upon their arrival at a certain diſtance, they turn round, and preſent to the magnet their oppoſite extremity.

EXPERIMENT 2.

Place two diſſimilar ends of magnets about an inch apart, in a large diſh; let them be juſt covered with water; ſift the filings between the ends. The particles of iron are immediately attracted by the neareſt pole; they move quickly in oppoſite directions, occupy the neareſt ſtation they can, become themſelves magnetic, and preſent to each other diſſimilar poles. The particles attracted by the poles of the magnets thus mutually attract each other. Between the poles of the large magnets, the filings are arranged in ſtraight lines; becauſe there, they tend directly to the attracting points; the more removed, the more curvilineal their poſition; becauſe each particle making the ſame effort to occupy the neareſt ſtation to the centre of attraction, they are all neceſſarily forced into a poſition correſponding with that effort.

EXPERIMENT 3.

Place ſimilar ends of two magnets, as the diſſimilar were ſituated in the laſt experiment, and ſift the filings between them. Here alſo, you ſee them to be acted upon by attraction as before; they move to the neareſt pole, become magnetic, and preſent to each other ſimilar poles; that is, ſuppoſing the north poles of two magnets to be oppoſed, all thoſe filings which are attracted by the north pole of one of the magnets, preſent a north pole to the north pole

pole of all thoſe attracted by the other magnet; they repel each other, of conſequence; a vacuity is obſerved between the reſpective parcels of filings; whilſt the appearance of reverted curves is exhibited, on account of the repulſion which their ſimilar poles exert upon each other.

Thus, beſides the proof which theſe experiments afford, that the attractive force of the magnets, at either pole, is the real cauſe of the phenomena which the filings exhibit; they prove alſo, in the moſt ſatisfactory manner, that the action of the magnet upon the filings, when they approach within a certain diſtance, renders them magnetic, and hence produces the effects mentioned in the two laſt experiments. But, in every inſtance, attraction firſt operates. Similar poles, whilſt they are repulſive of each other, are ſtill attractive of all other ſubſtances upon which the magnet acts. The ſame body, at the ſame time, appears to exert two oppoſite powers.

The cauſe of magnetic attraction and repulſion, as well as of all other attractions or repulſions, lies ſtill hid in the receſſes of nature; but the manner, in which theſe forces produce certain phenomena, is no longer concealed from us.

Theſe experiments may be agreeably varied by placing three or four magnets upon each other, then covering them only partially, and ſprinkling the filings on each ſide of them.

In every magnet, there is at leaſt one line, called the equator, where the attractive power vaniſhes: from this line, towards either pole, it gradually increaſes, and hence thoſe filings which are near to the ſides of the magnet, will incline towards them, forming angles, which appear to be ſuch as the reſolution of two forces, one lateral, the other polar, would neceſſarily produce.

Perhaps

Perhaps this method of making experiments, by ſubſtances floating in water, and thus ſubjected in all their motions to our examination, may lead to more important diſcoveries. The rates in which the magnetic attractive force decreaſes, at different diſtances, may, I think, be collected from noting the velocity with which the floating bodies move, at different diſtances from the poles, or the ſpaces, which they paſs over in equal times. Nothing obſtructs an accurate ſolution of this problem, but the difficulty of obtaining meaſurements of ſufficiently ſmall intervals of time. If the experiment were made upon a large ſcale, the difficulty might be removed. The remarkable acceleration which is obſervable, when the filings come near to their attracting point, ſeems to ſatisfy the eye, that the attraction increaſes in a greater ratio, than according to any law yet aſſigned.

One magnet acts upon another, at a conſiderable diſtance, either by repulſion or attraction. Will not theſe experiments lead to a rational conjecture, that in every inſtance, the action is communicated by intervening magnetic ſubſtances. It acts through atmoſpheric air. But this air may, from its conſtituent principles, and it is ſaid, does contain iron. The ſmall particles floating in the atmoſphere, may be acted upon, like thoſe floating upon water. The tenuity of the particles will only render the action more ſenſible. Each may become a magnet, and thus by the action of all the intervening affected particles, the action of one magnet may be communicated to the poles of another diſtant magnet.

I have made an experiment, in order to aſcertain, whether a magnet could exert its power in a Torricellian vacuum. A ſmall quantity of filings was poured into a glaſs tube of ſufficient length; it was then filled with mercury, and inverted in the ſame fluid; the filings floated on the ſurface of the mercury in the upper end. The reſult was, that

that the action of the magnet upon the filings, at equal distances, was sensibly less, than when the tube was full of atmospheric air. The want of a tube of sufficient diameter prevented me from making the experiment in so satisfactory a manner as I wished. It appears, however, worthy of being repeated by those who may possess the necessary means. If the magnetic power should be observed in such a vacuum, then the above conjecture will merit and receive the fate which has generally attended all reasoning in physics not founded on accurate experiment.

I am, very respectfully,

Yours, &c.

J. MADISON,
William & Mary College.

Thermometrical

Thermometrical Obſervations,

MADE AT FORT WASHINGTON,

Commencing June 1790, and ending April 1791.

BY DANIEL BRITT AND G. TURNER.

TO WHICH ARE ADDED, FOR SOME TIME,

The Riſe and Fall of the Ohio.

Communicated by G. TURNER.

Read July 14th 1797, at a Special Meeting.

N°. XL.

Thermometrical Obſervations made at Fort Waſhington, on the Ohio. N. Lat. 39°. 3′. 5″. By DANIEL BRITT.

Days.	At 3. P. M. Deg. Fah. Therm.	Winds.	JUNE, 1790. REMARKS.
1	78°	S. W.	Clear.
2	76	S.	Clear A. M. Cloudy P. M.
3	79	S. W.	Clear. Sultry P. M.
4	84	S.	Clear. Strong wind. *Greateſt heat.*
5	81	S. W.	Clear A. M. Cloudy P. M. Rain at night.
6	72	S. E.	Cloudy.
7	73	N. E.	Flying clouds.
8	71	S. W.	Ditto.
9	72	Eaſterly	Clear A. M. Cloudy P. M. Rain at night.
10	68	Ditto.	Cloudy. *The mercury loweſt now and on the 30th.*
11	73	E.	Rain with intermiſſions.
12	75	S. W.	Cloudy A. M. Rain P. M.
13	71	S.	Clear A. M. Flying clouds P. M.
14	76	S. E.	Clear.
15	81		Clear. Rain at night.
16	82	S. W.	Flying clouds. Showery.
17	80	Southerly	Clouds, with ſhowers.
18	82	S. W.	Flying clouds.
19	70	W.	Clear.
20	72	W.	Clear.
21	72	S. E.	Clear. Rain.
22	79	S.	Clear.
23	70	E.	Cloudy. Rain.
24	69	E.	Heavy Rain A. M. Rain P. M.
25	76		Clear.
26	78	S. W.	Clear.
27	82	S. E.	Clear.
28	78	E.	Clear.
29	71	S. W.	Overcaſt.
30	68	S. W.	Cloudy. Clear. *Mercury loweſt now and the 10th.*

Extremes	Mean Tem.	Enum. of winds	SUMMARY.	
84° } 68 }	75°.9′.	S. W.	Ten times—Prevailing winds, S. W.	
		S.	Four do.	
		S. E.	Four do.	
		E.	Four do.	
		W.	Two do.	Wholly clear 13 days.
		Eaſterly	Two do.	Partly do. 6 do.
		Southerly	Once.	
		N. E.	Once.	
		Omitted	Twice.	

JULY.

JULY, 1790.

3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	74°	N. W.	Clear.
2	75	N. W.	Clear.
3	79	W.	Clear.
4	80	W.	Clear.
5	79	S. E.	Overcaſt.
6	80	S. E.	Rain. Clear. Cloudy.
7	72	W.	Rain. Overcaſt. Clear.
8	69	W.	Overcaſt. *The mercury loweſt.*
9	71	E.	Clear. Overcaſt.
10	72	S. E.	Overcaſt.
11	76	S.	Cloudy.
12	85	W.	Clear.
13	85	S. E.	Clear.
14	76	W.	Rain. Cloudy.
15	78	S.	Cloudy. A gentle ſhower.
16	80	S. E.	Clear.
17	84	S.	Clear.
18	86	S. E.	Clear.
19	88	S. W.	Clear. *Greateſt heat this year.*
20	84	S.	Clear. Rain. Cloudy.
21	82	S. W.	Clear.
22	84	S. W.	Clear.
23	84	S. W.	Clear.
24	86	S. W.	Clear.
25	78	N.	Clear.
26	80	N. E.	Clear.
27	80	S. E.	Cloudy. Clear.
28	78	S. E.	Clear.
29	79	S. E.	Clear.
30	81	S.	Clear.
31	81	S. E.	Clear.

Extremes.	Mean Tem.	Enum. of winds.	SUMMARY.	
88° } 69 }	79° 17′	S. E.	Ten times—Prevailing wind.	
		W.	6—do.	
		S. W.	5—do.	
		S.	5—do.	Wholly clear—20 days.
		N. W.	2—do.	Partly do. — 5 do.
		N.	1—do.	
		N. E.	1—do.	
		E.	1—do.	

AUGUST.

AUGUST, 1790.

3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	75°	N.	Clear.
2	78	N. E.	Clear.
3	79	N. E.	Clear.
4	86	S. W.	Clear. *Greatest heat this month.*
5	84	S. W.	Clear. Flying clouds.
6	80	S.	Cloudy. Rain at night.
7	80	S.	Cloudy. Rain.
8	79	S. E.	Rain. Cloudy. Rain.
9	80	W.	Clear.
10	80	W.	Clear.
11	83	S. E.	Clear.
12	83	S.	Clear.
13	84	S. E.	Clear.
14	84	S. E.	Clear.
15	83	S. E.	Clear. Flying clouds.
16	82	S. E.	Clear.
17	80	S. W.	Cloudy. Showers.
18	74	W.	Rain. Cloudy. *Mercury lowest.*
19	77	W.	Clear.
20	75	S. E.	Cloudy.
21	77	S.	Cloudy. Clear, &c.
22	80	N. E.	Clear.
23	80	E.	Clear.
24	81	E.	Clear.
25	83	E.	Clear.
26	83	E.	Clear.
27	80	S. W.	Rain. Cloudy.
28	76	S.	Overcast.
29	78	S. W.	Rain. Cloudy.
30	80	S.	Overcast.
31	85	S. E.	Clear.

Extremes.	Mean Tem.	Enum. of winds.	SUMMARY.	
86° } 74 }	80° 9′	S. E.	8 times—Prevailing wind.	
		S.	6—do.	
		S. W.	5—do.	
		W.	4—do.	Wholly clear— 20 days.
		E.	4—do.	Partly do. — 1 do.
		N. E.	3—do.	
		N.	1—do.	

SEPTEMBER.

SEPTEMBER, 1790.

3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	86°	S. E.	Clear. Rain at night. *Greatest heat.*
2	78	W.	Clear.
3	78	W.	Clear.
4	76	W.	Clear.
5	75	W.	Clear.
6	74	W.	Clear.
7	81	S. W.	Clear.
8	83	S. W.	Clear.
9	81	W.	Clear.
10	84	S. W.	Clear.
11	82	S. W.	Clear.
12	82	S. W.	Clear.
13	82	S. W.	Clear.
14	66	N. E.	Rain.
15	66	N.	Clear.
16	67	N.	Clear.
17	68	N.	Clear.
18	64	N.	Clear.
19	65	N.	Clear.
20	65	N. E.	Clear.
21	66	N. E.	Cloudy.
22	65	N. E.	Rain. Cloudy. Rain.
23	65	N.	Clear.
24	66	W.	Clear.
25	66	W.	Clear.
26		Obſervations omitted.	
27			
28			Overcaſt—An almoſt inceſſant cold drizzling rain.
29			Do. do.
30	59	N E. & E.	Overcaſt—Small rain. *Mercury loweſt.*

Extremes.	Mean Tem.	Enum. of winds.	SUMMARY.	
86° } 59 }	72° 18′.	W.	8 times—Prevailing wind.	
		S. W.	6—do.	Wholly clear—22 days.
		N.	6—do.	Partly do. — 0 do.
		N. E.	4—do.	
		S. E.	1—do.	
		N E. & E.	1—do.	

OCTOBER.

Thermometrical Observations made at Fort Washington, on the Ohio, N. Lat. 39°. 3′. 5″. By JUDGE TURNER. *Therm. situated in the North shade.*

OCTOBER, 1790.

	3. P. M.		
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	65°	Calm.	Serene.
2	65	Calm.	Serene.
3	70	S. W.	Cloudy. Alternate sunshine. Rain and some thunder, P. M.
4	70	S. W.	Cloudy and windy. Rain last night. *Greatest heat to day and yesterday.*
5	69½	S. S. W.	Clear.
6	62½	N. N. E.	Overcast A. M. Clouds and sunshine P. M.
7	60	S. & S W.	Clear. Slight frost last night, the first.
8	58	E. N. E.	Overcast. Showery A. M. Steady rain P. M. & night.
9	50	E. N. E.	Overcast & rainy. Some rain at night. *Mercury lowest.*
10	54½	N. E.	Overcast. Clear forenoon and P. M.
11	58	E. N. E.	Clear.
12	59	E.	Overcast. Steady rain evening and night.
13	60	S. S. E.	Rainy. Showery all night.
14	54½	N. E.	Showery.
15	54	N.N.W.& W. S. W.	Showery. Fair and windy evening.
16	59½	W.	Clear.
17	59½	W.	Clear.
18	57½	W.	Clear.
19	52	N. W.	Overcast. Some sunshine P. M.
20	51	N. N. W.	Overcast.
21	53	S. S. W.	Clear. Smart frost last night.
22	53	S. S. W.	Clear. do. do.
23	57	S.	Clear.
24	56	S.	Clear. Overcast at noon. Clear evening.
25	53	W.	Rainy—Windy P. M.
26	50	W.	Overcast A. M. Clear P. M. *Mercury lowest.*
27	54	S. W.	Clear.
28	60	W.	Clear and windy. Evening cloudy.
29	63½	S. S. E.	Gentle rain.
30	67	S.	Overcast A. M. Clear P. M.

SUMMARY.

SUMMARY.

Extremes.	Mean Tem.	Enumera. of winds.		
70° 50	58° 14'.	W.	6 times—Prevailing wind.	
		W. S. W.	1—do.	
		S. W.	3—do.	
		S. S. W.	3—do.	
		S.	3—do.	
		S. S. E.	2—do.	
		N. N. W. W. S. W.	1—do.	Wholly clear—12 days. Partly do. — 8 do.
		N. W.	1—do.	
		N. N. W.	1—do.	
		N. E.	2—do.	
		N. N. E.	1—do.	
		E.	1—do.	
		E. N. E.	3—do.	
		Calm.	2—do.	

NOVEMBER.

NOVEMBER, 1790.

At 3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	62°	Calm.	Cloudy.
2	64½	Calm.	Generally ferene.
3	59	E. S. E.	Clear A. M. Overcaft P. M. Shower towards night.
4	50	W. S. W.	Windy. Overcaft. Rainy.
5	43½	W. N. W.	Cloudy. Some froft and rain laft night.
6	53	W.	Cloudy. White froft laft night.
7	59	S. S. W.	Clear.
8	63	W.	Clear.
9	57½	S.	Clear.
10	53½	W. N. W.	Cloudy.
11	50½	W. N. W.	Cloudy. Some rain at night.
12	56	S. S. E.	Overcaft.
13	59	W.	Overcaft. Slight rain.
14	59	W. N. W.	Overcaft.
15	64	S. S. W.	Cloudy and gloomy with rain. Thunder A. M.
16	54	W.	Overcaft. Rain A. M. ☞ about 2. P. M. the mercury ftood at 64°. At 3. it fell 10°. as noted, and arofe at 4. to 60°.
17	49	N. N. W.	Clear.
18	49	Omitted.	Clear, generally. Some ice formed, and fnow (the *firft* this feafon) fell this morning.
19	57½	W.	Clear and pleafant. A fmall hoar froft A. M. with ice 2 lines thick.
20	59½	W.	Clear and pleafant.
21	68	W.	Clear and remarkably pleafant. *Mercury higheft.*
22	58	W.	Cloudy A. M. Rain P. M.
23	47	N. E.	Cloudy. Moift atmofphere.
24	46½	N.	Cloudy. A little fnow at night.
25	45	N.	Cloudy. Snow towards evening.
26	36½	N. N. W.	Snow and fleet all day. *Mercury loweft.*
27	52½	W.	Clear.
28	41	W. N. W.	Clear A. M. Overcaft P. M.
29	42	W. S. W.	Clear.
30	48½	E. S. E. & E.	Overcaft till 10. A. M. Clear afterwards.

SUMMARY.

SUMMARY.

Extremes.	Mean Tem.	Enumeration of winds.		
68°	53° 19'	W.	9—times	[Prevailing wind.]
36½		W. N. W.	5—do.	
		W. S. W.	2—do.	
		N. N. W.	2—do.	
		N.	2—do.	
		N. E.	1—do.	
		S.	1—do.	Wholly clear—10 days.
		S. S. W.	2—do.	Partly do — 4 do.
		S. S. E.	1—do.	
		E. S. E.	1—do.	
		E. S. E. to E.	1—do.	
		omit. & calm.	3—do.	

DECEMBER, 1790.

3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	48½°	N.	Clear A. M. Overcaſt P. M.
2	48	N.	Heavy rains A. M. and laſt night. Rain and ſleet P. M.
3	44½	W. N. W.	Some ſnow A. M. Fair, with a few clouds, P. M.
4	44	N. N. W.	Cloudy A. M. Fair but a little cloudy P. M.
5	49¾	N. N. W. ½ N.	Clear and pleaſant.
6	50	S. S. W. ½ S.	Clear and pleaſant. Sharp froſt laſt night. *Mercury higheſt.*
7	55	N. E.	Clear and pleaſant. *Mercury higheſt.*
8	41	N.	Clear and pleaſant. Smart froſt.
9	46	N. N. W.	Clear A. M. Overcaſt P. M.
10	46	S.	Rainy morning—Next overcaſt and a moiſt atmoſphere. Drizzling rain all P. M.
11	37½	N. W.	Snow and ſleet all day. At night heavy rains. The air very moiſt, yet not cold, tho' mercury down at 37½°.
12	44	W. N. W.	Overcaſt generally. Air moiſt. Some ſnow laſt night.
13	48	N. N. W.	Cloudy—raw and diſagreeable.
14	32	N. N. W.	Clear and pleaſant A. M. Cloudy, and next overcaſt P. M.
15	31	N. N. W.	Clear and pleaſant. Sharp froſt and ice in the creeks laſt night.
16	29½	N. N. W.	Overcaſt. Very ſharp froſt.
17	26	N. N. W. ½ N.	Snow this morning and laſt night. Floating cakes of ice (for the firſt time) in the Ohio. Severe froſt laſt night.
18	29	S.	Clear and pleaſant.
19	40	S. W. by S.	Clear and remarkably pleaſant. A freſh breeze all day.
20	40½	W.	Cloudy.
21	38	W. S. W.	Clear generally. Some ſnow laſt night.
22	36½	W. S. W. ½ W.	Clear.
23	42	S. W. by W.	Clear. Increaſed floating ice in the Ohio; but in a diſſolving ſtate.
24	46	Calm.	The ice ſtill floating.

25	25½	N. N. W.	Windy morning. Some ſnow at noon and laſt night. Ice ſtill floating. The Licking R. frozen up at the mouth.
26	23	N. N. W. to N.	Clear and pleaſant. Great bodies of floating ice in Ohio.
27	23½	W. S. W.	Clear A. M. Overcaſt P. M. Much ice in the Ohio.
28	36	S. S. E. ½ E.	Clear A. M. Cloudy and windy P. M. and all night. Some ſnow and hail in the afternoon.
29	34	S. E. to N.	Clear A. M. A few flakes of ſnow P. M. Ohio ice much diminiſhed.
30	25	N. N. W.	Clear.
31	20	W. N. W.	Clear, ſerene and remarkably pleaſant. *The coldeſt day this year.*

SUMMARY.

Extremes.	Mean Tem.	Enumeration of winds.		
55° } 20 }	38° 11′.	N. N. W.	8—times	[Prevailing wind.]
		N.	3—do.	
		W. N. W.	3—do.	
		N. N. W. ½ N.	2—do.	
		S.	2—do.	
		N. N. W. to N.	1—do.	
		N. W.	1—do.	
		N. E.	1—do.	
		W.	1—do.	Wholly clear—13 days.
		W. S. W.	1—do.	Partly do. — 7 do.
		W. S. W. ½ W.	1—do.	
		S. E. to N.	1—do.	
		S. W. by W.	1—do.	
		S. W. by S.	1—do.	
		S. S. E. ½ E.	1—do.	
		S. S. W. ½ S.	1—do.	
		Calm.	1—do.	

JANUARY, 1791.

At 3. P. M.

Days.	Deg. Fahrenheit's Thermometer.	Winds.	Perpendicular rife and fall of the Ohio per 24 hours. Rifes.	Falls.	REMARKS.
	°		In.	In.	
1	26	W. S. W.			Clear and pleafant.
2	41	S. S. W. ½ S.			Do. do.
3	44½	S. S. E.			Overcaft. Some drops of rain, laft night.
4	47	W. N. W.			Gentle rain. Some, laft night. Morning foggy.
5	44½	Calm.			Foggy with fome rain.
6	56½	W.			Overcaft. Rain laft night. Ice in Ohio difappeared, except on the fhores. *Mercury higheft.*
7	40½	W.		4	Cloudy and windy. Pretty high wind laft night. At 9. A. M. (the wind then weft came round by noon to the northward) the mercury ftood at 56°.
8	32	E.			Clear and pleafant A. M. Overcaft P. M. Smart froft laft night.
9	36	N. N. W.			Rainy and windy during the evening. Much rain, fleet, & hail laft night.
10	35	N. W.	48		Cloudy & fun-fhine A. M. Overcaft P. M. Great accumulation of ice in the river, occafioned by rifing water fweeping off the cakes lodged on the fhores.
11	29	N. N. W.	36		Clear and very pleafant. The ice has difappeared.
12	39	S.	72		Clear and very pleafant. A few diffolving cakes of ice float down the river.
13	51	W. S. W.	42		Clear and very pleafant. Now vaft cakes of ice in Ohio.
14	47	Calm.	4		Clear A. M. Overcaft P. M. Great bodies of floating ice.
15	41	W. N. W. ¼ W.		18	Cloudy and damp. Heavy rains early this morning. The ice is difappearing.

16

16	39½	W. N. W.		3	Cloudy. Ohio full of ſmall cakes of ice.
17	32½	W. N. W.		36	Cloudy, with a ſlight ſnow. The ice in Ohio gone.
18	24	W.			Snow morning, and again at noon. Cloudy P. M. Briſk winds laſt night and all day. The Ohio much diſcoloured. *Mercury loweſt.*
19	33	W.		48	Tolerably clear. Windy.
20	45	S. to W.			Clear and very pleaſant.
21	47	S.		18	Clear, ſerene and uncommonly pleaſant. ☞ The thermometer roſe in the evening to 52°. Warm night.
22	36	S.		6	Clear and ſerene as yeſterday.
23	51½	S. S. W. ½ S.		18	Do do. do.
24	51	S. to S. W.		12	Overcaſt A. M. Sun-ſhine at noon. Overcaſt P. M.
25	50	W.			Black flying clouds, with ſome rain A. M. Sun-ſhine P. M. High winds and heavy rains at night.
26	52	W.		24	Clear and ſerene A. M. Pleaſant but cloudy P. M. Some froſt laſt night.
27	39¾	W. S. W.		18	Cloudy and raw A. M. Pleaſant P. M.
28	32¼	W. N. W.		6	Trifling ſnow early A. M. Clear ſerene and pleaſant P. M.
29	46¾	W.			Coudy A. M. Clear P. M.
30	32¾	N. W.	Ohio ſtand	at a —	Clear. High winds laſt night and all day.
31	33	S.	Ri-	ſing.	Clear.
			202	211	

Extremes.	Mean Tem.	Enumeration of winds.		SUMMARY.
56½°	40° 17′	W.	7 times	[Prevailing wind.]
24		W. S. W.	3—do.	
		W. N. W.	4—do.	
		S. S. W. ½ S.	2—do.	
		N. N. W.	2—do.	
		N. W.	2—do.	Wholly clear—12 days.
		N. N. W. ½ W.	1—do.	Partly do. — 6 do.
		S. to W.	1—do.	
		S. S. E.	1—do.	
		E.	1—do.	
		S. to S. W.	1—do.	
		Calm.	2—do.	

FEBRUARY.

FEBRUARY, 1791.

At 3. P. M.					
Days.	Deg. Fahren. Thermometer.	Winds.	Ohio per 24 hours. Rifes.	Falls.	REMARKS.
	°		In.	In.	
1	36½	E.			Overcaft. Slight fnow A. M. Fair but moift P. M. Slight fnow evening and night.
2	38	S. E.			Clear.
3	37	S. S. E.			Clear till 11 A. M. Overcaft, with trifling rain and fnow till 2 P. M. Overcaft again and then rain evening and all night.
4	52½	E.			Overcaft. Cloudy and rainy A. M. Ditto with rain P. M. Rain evening and night.
5	55	Calm.			Overcaft. Moift atmofphere.
6	59½	E.			Nearly calm. Foggy morning. Overcaft P. M. Rain at 2 P. M. *Mercury higheft.*
7	58	E.			Nearly calm. Thick fog on Ohio. Rainy all day and night.
8	45	E. N. E.	36		Small rains and part of night.
9	32½	W.	120		Some flight fnows. Icicles this morning.
10	28	W. N. W.	96		Some fnow A. M. Cloudy with fun-fhine P. M. Ohio much difcoloured, and great quantities of drift wood now defcending. Hard froft laft night.
11	38¼	S.	36		Clear. Smart froft laft night and early this morning.
12	45	Calm.	36		Clear and pleafant.
13	54	W. S. W.	24		Serene and pleafant.
14	54	W.			Serene and pleafant. River begins to fall.
15	49¼	variable.			Cloudy. Some drops of rain. A ftrong wind commenced P. M. and blew hard all the evening.
16	22	W. N. W.			Cloudy A. M. Clear P. M. Windy. Some fnow.

17

17	20½	N. E. to W.			Serene and pleafant. Hard froft laft night. *Mercury loweft.*
18	23½	E.			Steady fnow of fmall flakes all day, which began early this morning. It fell nearly 11 inches thick, the deepeft fnow known here.
19	30	N.			Fair—but overcaft and cloudy.
20	36	S. S. W.			Serene and pleafant.
21	42	W. N. W. ¼ W.			Serene and pleafant. Dry and frofty with very light airs.
22	37	Wefterly.		Uncertain.	Sun-fhine A. M. Overcaft and moift P. M. Very light airs and a thaw all day. At even. fnow and fleet.
23	44	W.			Clear. Thaw continues. Floating ice in Ohio.
24	50	W.			Nearly calm. Clear A. M. Overcaft P. M.
25	56¼	E. to N. N. E.			Clouds and fun-fhine alternately.
26	48	W.			Clear with light airs. High winds early A. M.
27	59	S.	36		Sun-fhine A. M. Cloudy P. M. with thunder. Lightning at night. Showery all day.
28	37	N. N. W.	72		Sun-fhine and clouds. Gufts of wind with fhowers laft night and to-day.
			456		

Ex-tremes.	Mean Tem.	Enumeration. of winds.	SUMMARY.	
59½° } 20½ }	42° 12′	W.	6 times	[Prevailing wind.]
		W. N. W.	2—do.	
		W. S. W.	1—do.	
		W. N. W. ¼ W.	1—do.	
		N. N. W.	1—do.	
		E.	5—do.	
		E. to N. N. E.	1—do.	
		E. N. E.	1—do.	Wholly clear—10 days.
		S. E.	1—do.	Partly do. — 8 do.
		S. S. E.	1—do.	
		S. S. W.	1—do.	
		S.	2—do.	
		N. E.	1—do.	
		N.	1—do.	
		Variable.	1—do.	
		Calm.	2—do.	

MARCH.

MARCH, 1791.

At 3. P. M.			Ohio per 24 hours.		REMARKS.
Days.	Deg. Fahren. Thermometer.	Winds.	Rises.	Falls.	
	°		In.	In.	
1	39	W.	84		Clear. Brisk wind last night.
2	58	W.	84		Clear and pleasant. Brisk wind A. M. and again at evening.
3	58$\frac{1}{4}$	W.	60		Clear and pleasant.
4	65$\frac{1}{2}$	Calm.	15		Serene A. M. Cloudy P. M.
5	68$\frac{1}{2}$	S. S. E.$\frac{1}{4}$ E.		5	Clear and windy A. M. Cloudy P. M. at 8 P. M. rain. Showery all night, with some thunder and pretty high wind. Very warm all night.
6	67	W. S. W.$\frac{1}{4}$ W.		24	Clear and pleasant.
7	56	W. N. W.		3	Cloudy and some sleet A. M. Clear and pleasant P. M. Brisk wind.
8	46	S. S. E.$\frac{1}{4}$ S.		84	Clear and serene.
9	46$\frac{1}{2}$	W. S. W.$\frac{1}{4}$ W.			Very rainy from 8 A. M.
10	52	S. S. E.$\frac{1}{4}$ S.	24		Very rainy A. M.
11	58$\frac{1}{2}$	W.	84		Clear, with some clouds A. M.
12	56	Calm.	12		Clear and cloudy alternately.
13	60	Calm.	48		Cloudy with some sun-shine.
14	56	N. E. by N.	6		Rainy from half past 11 A. M. Foggy morning.
15	73$\frac{1}{2}$	S.	2		Cloudy and clear alternately. High winds and rain last night. Brisk wind all day.
16	80	S.	Stands.		Cloudy in general A. M. Clear with some clouds P. M. Thunder at 2 at noon, and again at evening with lightning. *Mercury highest.*
17	68	S. E.	18		Cloudy. Heavy rains last night and this morning. Rainy P. M.
18	46	N. W.	36		Cloudy. Heavy rains and high winds last night and this morning.
19	38	N. W.		12	Cloudy in general. Brisk winds and a little snow A. M. Two lines thick of ice made last night. *Mercury lowest* [a difference of 42° in 4 days.]

20	48½	S.		24	Serene and pleaſant.
21	69	S. W.		24	Clear, pleaſant, but windy. Peach trees in bloom, and the woods covered with various flowers. The Buck-eye tree now in full foliage.
22	70	W.		3	Serene and pleaſant.
23	75	S.	5		Clear and pleaſant, but windy.
24	76	W.		3	Clouds and ſun-ſhine. High winds.
25	69	E. N. E.¼ E.		24	Clear and pleaſant.
26	70	S. E.		60	Clear A. M. Cloudy P. M.
27	68¼	S. E.		24	Cloudy. Some rain P. M.
28	52½	E.			Cloudy and rainy A. M. Damp atmoſphere.
29	60	S. E.			Serene and pleaſant.
30	63	E. S. E.		24	Serene and pleaſant.
31	62	Variable.			Serene and pleaſant.
			478	312	

SUMMARY.

Extremes	Mean Tem.	Enum. of winds		
80° } 38 }	60° 16'.	W.	6 times	[Prevailing wind.]
		N. W.	2—do.	
		S. E.	4—do.	
		S.	4—do.	
		Weſterly.	4—do.	Wholly clear—13 days.
		Southerly	3—do.	Partly do. — 8 do.
		Eaſterly.	2—do.	
		E	1—do.	
		Northerly	1—do.	
		Calm, &c.	4—do.	

APRIL, 1791.

At 3. P. M.			
Days.	Deg. Fah. Therm.	Winds.	REMARKS.
1	56°	S. E	Cloudy with ſome rain.
2	46	N. N. E.	Clear. *Mercury loweſt.*
3	50	W.	Serene and pleaſant. Smart froſt, ſome ice and wind laſt night.
4	56	N. W.	Serene and pleaſant. Smart froſt laſt night.
5	64	Variable.	Ditto. ditto. Slight froſt laſt night.
6	58	E. N. E.	Rainy A. M. Overcaſt P. M. Between 8 & 9 P. M. ſhock of an earthquake, 2 minutes.
7	64	N. E.	Clear.
8	66	Calm.	Serene and pleaſant.
9	80	Calm.	Ditto. ditto. *Greateſt heat.*
10	76	S. S. E. } Variable. }	Ditto. ditto.
11	70	Calm.	Cloudy in part A. M. Rain P. M.
12	69	S. W.	Clear and calm A. M. Cloudy P. M. a ſhort but violent guſt of wind, attended with rain, thunder and lightning.
13	74	W. S. W.	Cloudy and ſun-ſhine alternately. High winds all day, with thunder and lightning towards noon. Showery but pretty rainy at night. Laſt night heavy rains.
14	60	WNW¼W	Showery all day, and ſome ſmart rains laſt night.
			☞ Here end my obſervations at Fort Waſhington. The ſucceeding month and part of June were remarkably wet and warm; the mercury frequently riſing ſome degrees above 80°. Some of the ſhowers were like tumbling torrents of water, and were now and then accompanied with thunder and lightning. G. TURNER.

SUMMARY.

SUMMARY.

Extremes.	Mean Tem.	Enum. of winds.	
80° } 46	50° 9′	Westerly.	Most prevailing wind so far. Wholly clear—8 days. Partly do. —2 do. Note. That of the foregoing 317 days, 153 were wholly clear, and 55 partly so, and that Westerly and South-westerly are the prevailing winds.

RECAPITULATION OF FINE WEATHER.

Number of Days.	Months.	Days wholly clear.	Days partly clear.
30	June.	13	6
31	July.	20	5
31	August.	20	1
30	September.	22	0
30	October.	12	8
30	November.	10	4
31	December.	13	7
31	January.	12	6
28	February.	10	8
31	March.	13	8
14	April.	8	2
317	Total.	153	55

N°. XLI.

Calculations relating to Grist and Saw Mills, for determining the quantity of Water necessary to produce the desired effect when the Head and Fall are given in order to ascertain the dimensions of a new invented Steam Engine, intended to give motion to Water-wheels in places where there is no Fall, and but a very small Stream or Spring. *By* JOHN NANCARROW.

ELEMENTS used in the following calculations, so far as they relate to works moved by water-wheels:

1. let $h =$ mean height of the head of water in the penstock.
2. $a =$ the area of the aperture or gateway.
3. $q = 6.128 =$ the number of ale gallons in a cubic foot.
4. $s = 16$ feet, $=$ the space a heavy body falls from rest in one second.
5. $2s =$ the uniform velocity acquired by falling 16 feet from rest.
6. $2\sqrt{hs}$, or $8\sqrt{h} =$ the uniform velocity acquired by falling from rest any depth $= h$.
7. $8aq\sqrt{h}$, the number of ale gallons issuing through any aperture a in one second, and $8aqt\sqrt{h} =$ the quantity in t seconds.
8. $8a\sqrt{h} =$ the number of cubic feet flowing through a in one second, and $8at\sqrt{h} =$ the number of cubic feet in t seconds.
9. $w = 62.5$ pounds avoirdupoise, $=$ the weight of a cubic foot of water, and 10.2lbs. $=$ the weight of an ale gallon.
10. $haw =$ the weight of any column of water.
11. $\frac{\sqrt{h}}{4} = t =$ the time of falling from an height $= h$.
12. $D - \frac{d}{2}$ is the common practical rule for finding the mean height of the head of water when the aperture is vertical and rectangular where D represents the depth of

of water in the penſtock, and d the height of the gateway, and is only an approximation, though very near the truth; the genuine method derived from the parabola is as follows:

Let ABCD Fig. 1. repreſent a large ciſtern or penſtock, and MKLN an orifice made in one of its ſides.

When the upper edge of the gateway, as KL is below the ſurface of the water in the penſtock, the ſum of all the velocities or ſheets of water which flow through it, being expreſſed by the elements of the ſegment of a parabola FHIG, there will be found amongſt them a mean ordinate OT, which being multiplied by the height HP, will give a product equal to the area of this ſegment. Now, in order to determine the mean height EO, let EP$=a$, EH$=b$, HP$=c$, and the mean height EO$=x$. The ſum of all the velocities, or the area of the parabola EPG will be $\frac{2a}{3}\sqrt{a}$, and the ſum of all the velocities acquired by falling from E to H $=\frac{2b}{3}\sqrt{b}$; conſequently $\frac{2a}{3}\sqrt{a}-\frac{2b}{3}\sqrt{b}$ will give the ſum of all the velocities which flow through the orifice MKLN, which is equal to the parabolic ſegment HPIG, or to the product of the mean velocity $\sqrt{EO}$ (x) by the height HP (c), hence we have $\frac{2a}{3}\sqrt{a}-\frac{2b}{3}\sqrt{b}=c\sqrt{x}$, which equation being reduced will give $x=\frac{4}{9}\times\frac{a^3+b^3-2ab\sqrt{ab}}{cc}$.

EXAMPLE.

If the height EP (a) be $=8$ feet, EH (b) $=6$ feet, then will HP or $c=2$ feet; and by ſubſtituting theſe numbers for their reſpective values in the above equation, x will be found $=$ 6.99 feet.

By the common practical rule, (ſee article 12,) $D-\frac{d}{2}=h=x$, where $D=8$ and $d=2$; conſequently $h=\overline{7}$ feet, whence it appears that $a-\frac{c}{2}=D-\frac{d}{2}$ is ſufficiently exact for all common purpoſes.

In

In the foregoing elements (fee art. 4 and 5.) I have fuppofed the fpace which a heavy body defcribes by falling from reft in one fecond of time to be 16 inftead of $16\frac{1}{12}$ feet, and the uniform velocity acquired by fuch fall = 32 feet; whereas every author which I have read, (even on the fubject of hydraulics) makes it $32\frac{1}{6}$ feet, without allowing for the friction the water is fubjected to in its paffage through the aperture or gateway, or for the refiftence it meets with by its fudden impulfe againft the air, immediately on its leaving the penftock. It evidently follows that the uniform velocity muft be diminifhed on both thefe accounts: hence we may fafely conclude, that a uniform velocity of 32 feet in one fecond, will be found to coincide with an experimental proof, nearer than that of $32\frac{1}{6}$ feet in the fame time.

Before the dimenfions of the fteam engine can be afcertained, it is effentially neceffary to know what quantity of water it muft deliver into the penftock in a given time, in order that the power or force by which the water-wheel is moved, may be at leaft adequate to the purpofe intended. Several grift and faw-mills have been examined with this view, and fuch meafurements carefully taken as were thought neceffary for determining the powers by which they are moved. Amongft thefe we have felected John Beydler's grift and faw-mills, and a faw-mill belonging to Chriftopher Keyger, both in the county of Berks and ftate of Pennfylvania.

Calculation of the power applied to Beydler's grist-mill, either for one or two pair of stones, each being $4\frac{1}{2}$ feet in diameter, and that of the water-wheel 16 feet; the top of which is nearly on a level with the bottom of the penstock, and grinds from 50 to 60 bushels of wheat in 12 hours, with a fingle pair of stones.

The head or depth of water from its furface to the bottom of the penftock for working one pair of ftones = 22 inches.

The gateway or aperture a is 30 inches wide by $1\frac{1}{2}$ inch deep = 45 inches = 0.3125 parts of a fquare foot.

Mean height of the head, or $D - \frac{d}{2} = h$ (art. 12.) = 21¹ inches = 1.77 feet.

By art. 7th we have $8aq\sqrt{h}$ = the number of ale gallons iffuing through any aperture a in one fecond, = 8×0.3125× 6.128

$6.128 \times 1.33\ (\sqrt{h}) = 20.38$ gallons of water flowing through the gateway in one ſecond of time. The number of cubic feet which iſſue through this aperture in the ſame time $= 8a\sqrt{h}$ (art. 8.) $= 8 \times 0.3125 \times 1.33 = 3.325$. which being multiplied by 62.5 pounds, the weight of a cubic foot of water, gives 207.8 lbs. for the whole preſſure on the upper part of the wheel during the ſpace of one ſecond; but the inſtantaneous preſſure, or force of impact, where the water firſt ſtrikes the wheel, is haw (art. 10.) $= 1.77 \times 0.3125 \times 62.5 = 34.57$ pounds; alſo $8\sqrt{h} =$ the uniform velocity acquired at a the aperture in a ſecond, $= 10.64$ feet.

When this mill drives two pair of ſtones, the gate is raiſed an half inch higher; h being in this caſe $= 1.75$ and $\sqrt{h} = 1.323$ feet, by which means a becomes $= 30 \times 2$ or 60 ſquare inches, $= 0.417$ parts of a ſquare foot. The other meaſurements remaining the ſame as above, we ſhall have

$8aq\sqrt{h} = 27$ gallons per ſecond,
$8a\sqrt{h} = 4.414$ cubic feet per do.
$haw = 46.5$ pounds for the force of impact,
and $8\sqrt{h} = 10.584$ feet for the uniform velocity per ſecond.

In Emerſon's Mechanics, and Fletcher's Univerſal Meaſurer and Mechanic, the uniform velocity acquired by falling from an height $= h$, is denoted by $\sqrt{2hs}$ inſtead of $2\sqrt{hs}$, which is the true meaſure of its celerity. This circumſtance is not mentioned with the leaſt view to find fault with theſe authors, but to remove any doubts which may ariſe in the minds of ſuch as are diſpoſed to peruſe theſe calculations.

BEYDLER's SAW-MILL.

This mill has a ſmall underſhot wheel, commonly called a flutter wheel, which is no more than 3 feet in diameter. The depth of the water from its ſurface to the bottom of the penſtock is 3 feet, the gateway is 3 feet wide by 6 inches deep, $= 1.5$ ſquare foot, the mean height of the head or $D - \frac{d}{2} = 2.75$ feet $= h$, and $\sqrt{h} = 1.658$. The fall from the bottom of the penſtock to the place where the water impinges on the float or ladle-board is 10 feet, and $2.75 + 11 = 13.75$, being equal to the whole height

height of the column of water which propels this wheel. Now by art. 7. we have $8\,aq\sqrt{h} =$ the quantity of water which flows through the gateway in one ſecond, $= 8\times6.128\times1.5\times1.658 = 121.92$ gallons. Again, $haw = 2.75\times1.5\times62.5 = 257.8$ pounds, which is equal to the weight of the column preſſing againſt the aperture; alſo $8\sqrt{h} = 13.264$ feet, the uniform velocity of the water every ſecond as it iſſues through the gateway.

In order to find the force of impact on the wheel, we muſt, in the firſt place determine (what may be called) the initial weight of the water, or that with which it may be ſuppoſed to begin to preſs at its ſurface in the penſtock, viz. by dividing the momentum by the uniform velocity; but $haw = 257.8$ is the momentum at the aperture or gateway, and $13.264 =$ the uniform velocity: therefore $\frac{257.8}{13.264} = 19.406$ lbs. $=$ W, the initial weight required $= \frac{haw}{8\sqrt{h}} = \frac{aw}{8}\sqrt{h}$; conſequently $8\,W\sqrt{h}$ will expreſs the force of impact ſought, h being now $= 13.75$ and $\sqrt{h} = 3.708$; hence $8W\sqrt{h} = 8\times19.406\times3.708 = 575.66$ pounds for the conſtant impelling force on the ladle-board; but $575.66 = haw$, and $a = \frac{575.66}{hw} = \frac{575.66}{13.75\times62.5} = 0.67$ parts of a ſquare foot. To prove the truth of the above method for finding the force of impact, we need only try whether $8aq\sqrt{h}$ (where $h = 13.75$ and $a = 0.67$) will produce the ſame number of gallons as that before found, viz. 121.9 in one ſecond. In the preſent caſe $8aq\sqrt{h} = 8\times0.67\times6.128\times3.708 = 121.8$ being nearly the ſame as found above, and is a ſufficient proof that the force of impact where the water impinges on the wheel is rightly determined.

KEYGER's SAW-MILL.

This ſaw-mill is over-ſhot, the wheel 12 feet diameter, the penſtock is 6 feet in depth by 2 feet wide, and when I ſaw it at work, there were only 4 feet and 1-4th inch of water in the ciſtern, although the ſaw moved at the rate of 120 ſtrokes in a minute, whilſt it paſſed through a piece of oak at leaſt 12 inches deep,

deep, and the gateway no more than half an inch high. Hence $h = 4$, and $\sqrt{h} = 2$ feet. The aperture $a = 24 \times 0.5 = 12 = 0.0833$ parts of a ſquare foot; wherefore $8aq\sqrt{h} = 8 \times 0.0833 \times 6.128 \times 2 = 8.2$ gallons which falls on the wheel in the ſpace of one ſecond, $8a\sqrt{h} = 8 \times 0.0833 \times 2 = 1.32$ cubic feet in the ſame time, and $8\sqrt{h} = 16$ feet, the uniform velocity per ſecond, as the water leaves the aperture; alſo $haw = 4 \times 0.0833 \times 62.5 = 20.8$ pounds for the force of impact where the water firſt enters on the wheel. When this mill is ſupplied with a 6 feet head, and the gate drawn up one inch, the ſaw makes 180 ſtrokes in a minute through an oak log 18 inches deep. We have now $h = 6$, $\sqrt{h} = 2.45$ feet, and $a = 24$ inches $= 0.167$ parts of a ſquare foot. Here $8aq\sqrt{h} = 8 \times 0.167 \times 6.128 \times 2.45 = 20$ gallons per ſecond, $8a\sqrt{h} = 8 \times 0.167 \times 2.45 = 3.273$ cubic feet in the ſame time, and $8\sqrt{h} =$ the uniform velocity acquired by a fall of 6 feet; alſo $haw = 6 \times 0.167 \times 62.5 = 62.6$ for the weight of the column or force of impact on the wheel.

I have been the more particular in making theſe calculations in order to aſcertain the dimenſions of the ſteam-engine for various purpoſes; on which account we muſt again have recourſe to the parabola, and alſo to the inverted ſyphon.

To find the retarded velocity and time of aſcent of water into an exhausted receiver, through a vertical pipe or tube, by the aſſistance of the parabola. Fig. 2.

Let CBFG be an inverted ſyphon, the diameter being every where equal, accompanied with a cock T, and the firſt branch AE always kept full of water: it is certain that if all the reſt of the ſyphon be empty, and the cock be ſuddenly opened, the water will immediately ruſh into the tube of communication VX with a uniform velocity equal to that which a heavy body would acquire by falling from A to B, and will be continually diminiſhed in proportion as the ſecond branch is filled.

To ſhew in what order this retardation of the water diminiſhes its velocity at any point Q of the tube GS, where it is ſuppoſed to be aſcending towards qr, we muſt deſcribe on the lines AB, CD as an axis with the ſame parameter, two equal parabolas CPH and BKI, ſituated in oppoſite directions. Complete the parallelogram AM, and draw as many lines LR as you pleafe parallel to the horizontal line IG. Now if we take the ordinate AI, or its equal DH to exprefs the whole uniform velocity acquired by falling from C to D, it is evident that the ordinate OP will denote the velocity at the point O, acquired by a fall equal to CO, and the ordinate NK will exprefs the velocity arifing from a fall equal to NB or QS. But we fhall prove that the velocity of the afcending water in the fecond branch, when it arrives at Q, ought not to be expreffed by the ordinate which correfponds to it; but by the line LK, the difference between the entire uniform velocity LN or MB (by falling from A to B) and that of NK.

To demonftrate that the height QS or NB of the water in the tube SR, is equal to a fall which can produce the relative velocity arifing from CD, or the difference between the velocities acquired by falling from A to B, and that of the afcending water at Q; let AB and QS be confidered as two non-elaftic bodies, whofe momenta are as the altitudes AB and QS. If AB $=a$ and QS $=r$, we fhall have $a=\sqrt{a}\times\sqrt{a}$, and $r=\sqrt{r}\times\sqrt{r}$; but the difference of the momenta divided by the fum of the bodies is equal to the velocity, which let be v; therefore $\sqrt{a}\times\sqrt{a}-\sqrt{r}\times\sqrt{r}$ divided by $\sqrt{a}+\sqrt{r}=\dfrac{\overline{\sqrt{a}+\sqrt{r}}\times\overline{\sqrt{a}-\sqrt{r}}}{\sqrt{a}+\sqrt{r}}=$ $\sqrt{a}-\sqrt{r}=v=$ LK, the velocity of the water at Q, and $\sqrt{a}-v=\sqrt{r}$, which is the relative velocity produced by a fall equal to QS. As this velocity is expreffed by the ordinate NK, the difference between it and MB or LN will exprefs the retarded velocity of the water in the tube of communication DX, which is the fame as that of the furface QR at the point Q.

As it will be the fame with all the retarded velocities during the time employed in filling the tube GF, it follows that their fum will be expreffed by that of all the ordinates, or the area of the parabolic complement MIKB.

Before the obfervations of Belidor on the inverted fyphon, in his theory relating to the common fucking pump, it was cuftomary to eftimate this fum by the area of the parabola DCPH or ABKI; for the velocity at Q was expreffed by the fquare root of CO, inftead of the difference between the fquare roots of CD and QS.

The parabolic complement MIKB, being but half of the parabola ABKI, it is evident that the fum of all the retarded velocities

velocities in filling the fecond branch will be no more than half the fum of the velocities on which we have been accuftomed to count; from whence it follows that the branch FG will require twice the time to be filled as was formerly imagined. It follows alfo, that becaufe the complement MIKB, is but one-third of the parallelogram AIMB; therefore the branch BF will be three times as long in filling as it would be with the uniform velocity expreffed by MB. And laftly it follows, that the fum of the velocities of water afcending from Q to q, inftead of being expreffed by the area of the mixed quadrilateral PO op, ought to be expreffed by the area of the quadrilateral KL lk, which may be found in the following manner.

Let $AB=a$, $nB=b$, $NB=r$ and $nN=c$. Now the uniform velocities being as the ordinates, we fhall have $AI=\sqrt{a}$, $nk=\sqrt{b}$, $NK=\sqrt{r}$, and let $b-r=c$; but $\frac{2b}{3}\sqrt{b}-\frac{2r}{3}\sqrt{r}=$ the fegment $nNKk$, and $c\sqrt{a}=$ the parallelogram $LNnl$; confequently $c\sqrt{a}-\frac{2b}{3}\sqrt{b}-\frac{2r}{3}\sqrt{r}=$ the fpace $LKkl$, or the fum of all the retarded velocities during the afcent from Q to q.

To give an example in numbers, we will fuppofe $a=30$, $b=24$, $r=20$ and $c=4$, then $8\sqrt{a}=43.82$, $8\sqrt{b}=39.19$, and $8\sqrt{r}=35.78$; hence $4\times43.82-\frac{2\times24}{3}\times39.19-\frac{2\times20}{3}\times35.78=175.28-627.04-477.07=25.31$; but by the laws of accelerated motion, the fpaces defcribed are as the fquares of the times of defcription; wherefore $32 : 1''^2 :: 25.31 : 0.79$ and $\sqrt{0.79}=0.89''=$ the time required for the water to afcend from Q to $q=4$ feet.

DESCRIPTION OF THE STEAM-ENGINE. Fig. 3.

A. The receiver, which may be made either of wood or iron.
BBBBB. Wooden or caft-iron pipes for conveying the water to the receiver and from thence to the penftock.
C. The penftock or ciftern.
D. The water-wheel.

 E. The

E. The boiler, which may be either iron or copper.

F. The hot-well for ſupplying the boiler with water.

GG. Two ciſterns under the level of the water, in which the ſmall bores BB, and the condenſer are contained.

HHH. The ſurface of the water with which the ſteam-engine and water-wheel are ſupplied.

aa. The ſteam-pipe, through which the ſteam is conveyed from the boiler to the receiver.

b. The feeding-pipe for ſupplying the boiler with hot water.

ccccc. The condenſing apparatus.

dd. The pipe which conveys the hot water from the condenſer to the hot-well.

eee. Valves for admitting and excluding the water.

ff. The injection pipe, and *g* the injection cock.

h. The condenſer.

It does not appear neceſſary to ſay any thing here on the manner in which this machine performs its operations without manual aſſiſtance, as the method of opening the cocks by which the ſteam is admitted into the receiver and condenſed, has been already well deſcribed by ſeveral writers. But it will be neceſſary to remark that the receiver, penſtock, and all the pipes, muſt be previouſly filled before any water can be delivered on the wheel, and when the ſteam in the boiler has acquired a ſufficient ſtrength, the valve at *c* is opened and the ſteam immediately ruſhes from the boiler at E into the receiver A, the water deſcends through the tubes A and B, and aſcends through the valve *e* and the other pipe or tube B into the penſtock C. This part of the operation being performed and the valve *c* ſhut, that at *a* is ſuddenly opened, through which the ſteam ruſhes down the condenſing pipe *c*, and in its paſſage meets with a jet of cold water from the injection cock *g* by which it is condenſed. A vacuum being made by this means in the receiver, the water is driven up to fill it a ſecond time through the valves *ee* by the preſſure of the external air, when the ſteam-valve at *c* is again opened and the operation repeated for any length of time the machine is required to work.

There are many advantages which a ſteam-engine on this conſtruction poſſeſſes beyond any thing of the kind hitherto invented; a few of which I ſhall beg leave to enumerate.

1. It

1. It is ſubject to little or no friction.

2. It may be erected at a ſmall expence when compared with any other ſort of ſteam-engine.

3. It has every advantage which may be attributed to Bolton and Watt's engines, by condenſing out of the receiver, either in the penſtock or at the level of the water.

4. Another very great advantage is, that the water in the upper part of the pipe adjoining the receiver, acquires a heat by its being in frequent contact with the ſteam, very nearly equal to that of boiling water; hence the receiver is always kept uniformly hot as in the caſe of Bolton and Watt's engines.

5. A very ſmall ſtream of water is ſufficient to ſupply this engine, (even where there is no fall) for all the water raiſed by it is returned into the reſervoir HHH.

From the foregoing reaſons it manifeſtly appears that no kind of ſteam-engine is ſo well adapted to give rotatory motion to machinery of every kind as this. Its form is ſimple, and the materials of which it is compoſed are cheap; the power is more than equal to any other machine of the kind, becauſe there is no deduction to be made for friction, except on account of turning the cocks which is but trifling.

Its great utility is therefore evident in ſupplying water for every kind of work performed by a water-wheel, ſuch as griſt-mills, ſaw-mills, blaſt-furnaces, forges, &c.

Dimenſions of the Steam-Engine for working an overſhot wheel, accompanied with ſuch calculations as are neceſſary for aſcertaining the ſizes of its different parts, when applied to various purpoſes.

The quantity of water which this machine is intended to raiſe into the receiver in a given time, cannot be aſcertained until ſome ſtandard be fixed on for the height of the ſurface of the water in the receiver above that in the reſervoir HHH, which when known, we ſhall be enabled to calculate the diameters of the receiver and pipes with certainty.

Writers on the ſubject of hydraulics generally allow that a column of water 34 feet high is equal to the preſſure of the atmoſphere when the mercury in the barometer ſtands at 29.5 inches. Now if we admit that the water will aſcend into an exhauſted receiver

receiver to the height of 30 feet only inſtead of 34 feet, we ſhall by this means allow about 4 feet for the imperfection of the vacuum, or nearly one-eighth part of the whole power of the machine, if the ſteam in the receiver could be perfectly condenſed. Let therefore the higheſt elevation of the water in the receiver be 24 feet above the ſurface of the water in the reſervoir, and if the bottom of the receiver and the upper part of the ciſtern or penſtock be each 20 feet above the ſame level, the diameter of the water-wheel may be eaſily aſcertained when the depth of the penſtock or head of water is given.

Now as the velocity of the water is continually retarded during the time employed to fill the receiver, we muſt again have recourſe to the inverted ſyphon (Fig. 2.) in order to determine the time in which it may be filled and emptied, which when aſcertained, we ſhall be enabled to calculate the number of ſtrokes the machine may make in a minute, and conſequently the quantity of water it will deliver on any overſhot water-wheel in a given time.

The example on page 355 was purpoſely intended to ſhew the time neceſſary for filling the receiver according to the above dimenſions, where $a=30$, $b=24$, $r=20$ and $c=4$ feet; whence it appears that it may be filled in $0^{\prime}.89$ to an height of 4 feet above its bottom, or 24 feet above the level of HHH.

The common ſteam-engine invented by Newcomen and Cawley, when it works to the beſt advantage, requires the ſteam to be made about one-tenth ſtronger than the ſurrounding air; but that this receiver may be emptied with ſufficient diſpatch, it will be neceſſary to increaſe the elaſticity of the ſteam at leaſt one-fourth part beyond what is produced from the uſual heat of boiling water. Admitting therefore that a column of water 34 feet in height be in equilibrio with the preſſure of the atmoſphere, we have $\frac{34}{4}=8.5$ feet, which added to 24, the higheſt elevation of the water above the ſurface of that in the reſervoir, gives 32.5 for the ſpace AB. Fig. 2. There being now but a column of 24 feet inſtead of 30 as before, preſſing againſt a counteracting column of 20 feet, the deſcent of the water in the receiver will be conſiderably ſlower than its aſcent, during the time occupied in filling it to an height of 4 feet above its bottom;

bottom; but we have ſuppoſed the increaſed elaſticity of the ſteam to be equal to a column of water 8.5 feet high, which being added to 4 feet, the difference between the two columns, makes nB in this caſe = 12.5 feet, An = 8.5 feet, nN = 4 feet, and NB = 4.5 feet. By theſe meaſurements the parallelogram $NnlL$ will be found = 113.1368, the parabola $BnkB$ = 132.1648, and the parabola BNKB=50.9118; hence $BnkB$—BNKB= $NnkK$ = 81.253; but 113.1368—81.253 = 31.8838 = $KklL$, and $\sqrt{\frac{31.8838}{32}} = 0''.99$ or $1''$= the time required to empty the receiver, when filled with 4 feet of water; and as it appears that it may be filled in $0''.89$ or $0''.9$ of a ſecond, it is therefore evident that this machine may make 30 ſtrokes in a minute, ſuppoſing the pipes and receiver were all of the ſame diameter; but it is not neceſſary that it ſhould exceed 10 ſtrokes per minute, and conſequently the pipes which convey the water to and from the receiver, need not be more than one-third part of its area, and on no occaſion to exceed one half.

It has been ſuppoſed, in what we have ſaid concerning the ſteam-engine, that the upper part of the penſtock is on the ſame level with the bottom of the receiver, or 20 feet above the level of the water in the reſervoir, and admitting the penſtock to be 4 feet in depth, (inſtead of 22 inches, ſee page 350) there will be a ſpace equal to 16 feet left for the diameter of the wheel; but that its motion may not be interrupted by wading through the water in the reſervoir, we have here ſuppoſed the diameter to be no more than 15 feet. Now, if each revolution of the wheel be performed in the time this machine makes one ſtroke, the circumference muſt move with a velocity equal to 7.854 feet in each ſecond of time, admitting the ſteam-engine works at the rate of 10 ſtrokes in a minute.

Previous to determining the capacity of the receiver, it will not be improper to bring into one point of view, what has already been ſaid on the ſubject of Beydler's griſt-mill and Keyger's ſaw-mill.

Beydler's griſt-mill with one pair of ſtones, where $h = 1.77$ and $a = 0.3125$.

$8aq\sqrt{h} = 20.38$ in one ſecond, and $8aqt\sqrt{h} = 122.28$ gallons, t being

t being 6 feconds. $8a\sqrt{h}=3.325$ cubic feet and $8\sqrt{h}=10.64$ feet, the uniform velocity in one fecond; alfo $haw = 34.57$ lbs. the force of impact on the wheel.

For two pair of ftones, h being $= 1.75$ and $a = 0.417$. $8aq\sqrt{h}=27$ gallons in 1″ and $8aqt\sqrt{h}=162$ gallons in 6 feconds. $8a\sqrt{h}=4.4$ cubic feet, $8\sqrt{h}=10.584$ and $haw = 46.5$ pounds.

Keyger's faw-mill with a 4 feet head $= h$ and $a = 0.0833$. $8aq\sqrt{h}=8.2$ and $8aqt\sqrt{h}=49.2$ gallons in 6 feconds. $8a\sqrt{h}=1.32$ cubic feet, $8\sqrt{h}=16$ feet the uniform velocity in 1″; alfo $haw = 20.8$ lbs. the force of impact.

The fame mill with a 6 feet head $= h$ and $a = 0.167$. $8aq\sqrt{h} = 20$, $8aqt\sqrt{h} = 120$, $8a\sqrt{h} = 3.273$, $8\sqrt{h} = 19.6$ and $haw = 62.6$ pounds.

If it be intended that the receiver fhall contain 122.28 gallons for one pair of ftones, and 162 gallons for two pair, we fhall find that the former number is equal to a cylinder 4 feet high by 30 inches diameter, and that latter number is equal to one of the fame height by 3 feet diameter; but to find the area of the gateway in the penftock, adapted to the fteam-engine, which is 4 feet deep inftead of 22 inches, we muft find an area x for the aperture, which fhall difcharge as much water in a given time, (which we will fuppofe to be 6 feconds) as flows through the gateway of Beydler's mill in the fame time; making therefore H = the head of water at the fteam-engine, and h = that of the grift-mill; alfo a = the gateway as before, we have $x\sqrt{H} = a\sqrt{h}$, and $x = \frac{a\sqrt{h}}{\sqrt{H}} = 0.2078$ for one pair of ftones, and 0.2758 for two pair. Now Hxn being to haw nearly in the ratio of 3 to 2 for the difference of the forces of impact, we may fafely conclude that receivers of the above dimenfions will be amply fufficient for fupplying the water-wheel with a power as much fuperior to Beydler's mill, as the difference between the forces of impact will amount to.

With refpect to Keyger's faw-mill we fhall only remark, that as the quantity of water paffing over the wheel with a 6 feet head, is fo nearly equal to that which Beydler's mill requires for one pair of ftones, that a receiver of equal dimenfions will be found fufficiently large, the penftock of the fteam-engine being alfo 6 feet deep.

It

Fig. 1.

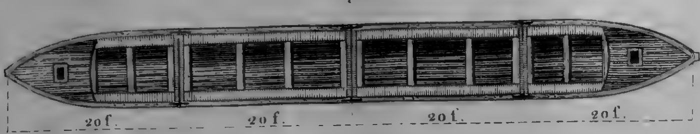


Fig. 2.

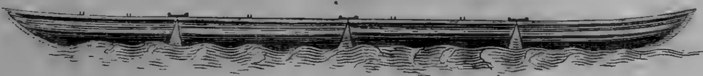


Fig. 3.

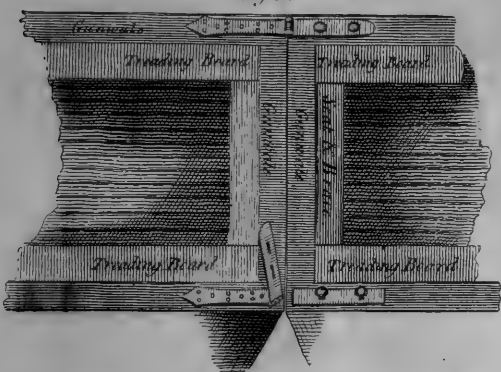
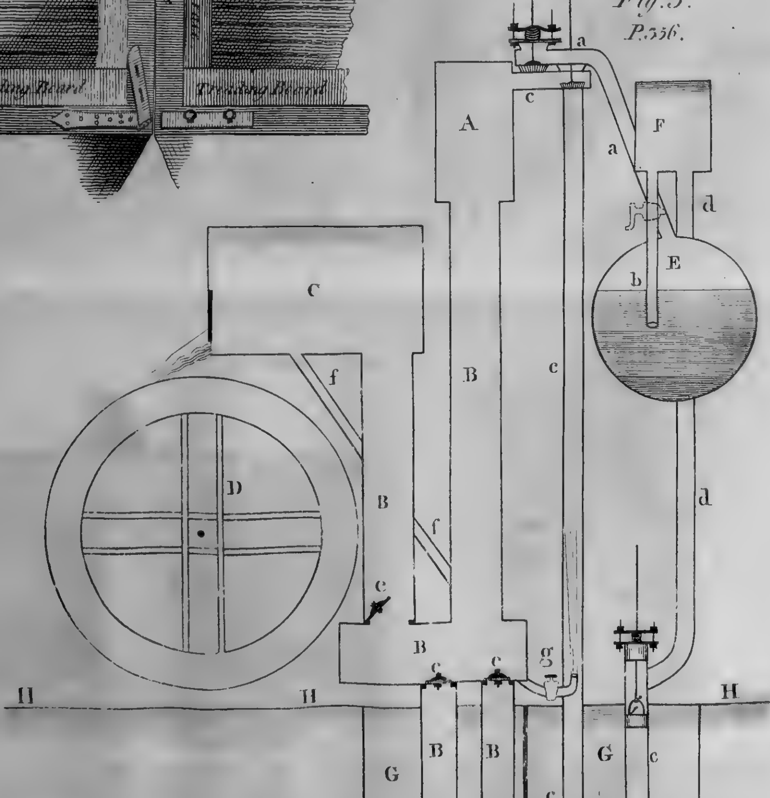


Fig. 3.
P. 356.



It will not be neceſſary that in any caſe the boiler ſhould contain more than 6 times as much as the receiver; hence we have for Beydler's mill with one pair of ſtones, and Keyger's ſaw-mill with 6 feet head, a receiver = 4 feet by 2.5 diameter = to 122.28 gallons, which multiplied by 6 = about 734 gallons for the contents of the boiler. The receiver for the ſame griſt-mill = 4 feet by 3 feet = 162 gallons, and the boiler = 972 gallons.

In order to prevent the water, whilſt the receiver is filling, from ſtriking againſt its top, it will be neceſſary that one foot at leaſt be added to its height; ſo that inſtead of being 4 feet high as we have hitherto ſuppoſed, it ſhould be at leaſt 5 feet.

No. XLII.

MEMOIR ON AMPHIBIA.*

SERPENTS.

Read, Feb. 1797.

OF the various animals which merit the attention and refearches of naturalifts, ferpents are not the leaft important, and the flight information and inaccurate ideas which we have of their manner of being, and of their habits, leave a rich and immenfe field for us to inveftigate.

Long convinced that many curious obfervations might be derived from a careful ftudy of thefe animals, I have profited by the opportunities derived from a refidence on this continent to employ myfelf on the fubject. They have been one of the objects of my refearches during a journey of about 2400 or 2500 miles made laft fummer in the fouthern parts of the United States, and among the Indians. I have even fearched for thefe reptiles in their retreats during the winter; that feafon which nature feems to have affigned to them for effecting a confiderable change, might we not fay a renewal, of feveral of their conftituent parts.

The rattlefnakes (crotalus of Linnæus) appearing to me the moft interefting, and offering the greateft number of curious phenomena (notwithftanding the dangers, too much exaggerated however, to which thofe who give themfelves up to fuch inveftigations are expofed) will form the principal objects of this memoir.

Before

* This memoir is part of a differtation on amphibia in general, which I hope to publifh after new obfervations which I propofe to make.

Before I enter into the detail of my obſervations, it is neceſſary to preſent and diſcuſs ſuccinctly, what has been ſaid and written on theſe animals, and to examine what we know concerning them.

The manner in which theſe amphibia attack the animals deſtined for their food is one of thoſe problems in natural hiſtory which are yet to be reſolved. The means they employ, as well as the real cauſes of many ſurpriſing effects, not yet well or unanimouſly ſtated, are unknown to us. According to ſome, the crotali and ſeveral other ſerpents have the faculty of enchanting and attracting birds, ſquirrels, rabbits, frogs, &c. (aves ſciuroſque ex arboribus in fauces revocant. Linn. Syſt. Nat.) According to others they inſpire them with terror, to ſuch a degree, that if we can put faith in the effects related, we ſhould be tempted to believe that they are from that moment deprived of their ſenſes, and, as it were, attacked with inſanity. According to others, in fine, theſe animals are violently affected and ſuffocated by a vapour, and fetid emanation, which the reptile diffuſes upon every thing around it.

It is ſaid that when the ſerpent wiſhes to ſeize a bird, a ſquirrel, &c. he remains motionleſs, his eyes conſtantly fixed upon his prey, and that then the unhappy victim, acted upon by a ſupernatural power, loſes all its faculties, and cannot even have recourſe to flight: it agitates itſelf, throwing out lamentable cries, goes, returns, advances, retreats, approaches, retires, comes and goes again, till at laſt exhauſted by fatigue, it voluntarily delivers itſelf up to its enemy, who delays not to devour it. Such are the effects attributed to enchantment, terror, or the ſuffocating vapour which thoſe ſerpents, it is ſaid, have the power of caſting round them, and which affects every animal which is found within its atmoſphere. Let us examine theſe three pretended cauſes, and compare them with their ſuppoſed effects.

Admitting thefe effects, attefted by fo many perfons, and by fo many refpectable authorities, effects of which I pretend not to deny the reality, but which I am likewife far from irrevocably adopting, it appears to me repugnant to reafon to attribute them to enchantment, giving to that expreffion the full latitude which it prefents to the imagination. We are no longer in that barbarous age in which men gave credit to enchantments, witchcraft, and miracles.

Reafon which ought to be the fole guide of all men, reafon, the beft gift of the author of all things, and the peculiar attribute of man, has at length affumed the upper place and driven out that general fanaticifm which formerly triumphed over unreflecting credulity. I do not pretend however to fay, that we are yet enabled to explain every thing; there are facts, (and the fubject of which I am treating is an example) whofe caufes we have not yet been able to difcover. But the men of the prefent day are fufficiently enlightened to remain in fufpenfe, and in fuch cafes to reject every idea of the fupernatural, fortilegeous, or miraculous.

If then the effects in queftion really exift, we may be allowed to believe that ferpents, deftined by nature (our common mother, always confiftent with herfelf; always equally beneficent and juft,) to fubfift on animals which have the advantage of fuperior flight and fpeed, ought to be endowed with proper arms and a power by whofe aid they may furprize and fecure their prey. But what are thefe arms? What this power? Is it one of thofe fecret operations which nature feems to envelope in impenetrable myftery? No. It is fimply a fact till now unknown, merely becaufe, 1ft, Thefe animals, whofe pretended uglinefs and danger have been fo much exaggerated, inftil into us a fpecies of repugnance which few have the courage to overcome. 2d, Becaufe few well-informed naturalifts raliſts have had it in their power to obſerve them attentive-ly, and for a length of time; and becauſe the greater num-ber of naturaliſts have been contented with reporting the facts collected in their travels.* 3d, Becauſe the oppor-tunities to make ſuch obſervations are few, and require a long and particular attention, which few men are capable or willing to afford. 4th, Laſtly, becauſe theſe reptiles, in order to exerciſe with ſecurity this imputed power, muſt ſeek retired ſpots, in which birds, ſquirrels, &c. are not ſubject to alarm, and muſt be themſelves at liberty to em-ploy the means given them by nature to provide for their ſubſiſtence.

Let us add to theſe reflections a few facts which give them additional force. It is within the knowledge of the peo-ple of Philadelphia and of the United States at large, that Mr. Peale, whoſe eſtabliſhment as well as zeal for the pro-greſs of natural hiſtory, is deſtined to produce in this country a taſte for ſo intereſting and uſeful a ſcience,† has kept

* The celebrated Cateſby to whom we are indebted for ſo many obſerva-tions and diſcoveries, more or leſs intereſting, confeſſes, ſpeaking of the boiquira (crotalus horridus. Linn.) that he never was an eye-witneſs of the pretended effects of the enchantment exerciſed by this reptile over birds, ſquirrels, &c. but that he is convinced of the facts. Might not the ſame be true of an obſervation related by him, and which upwards of twenty per-ſons have confirmed to me in my travels, that if the ſerpent is killed while thus fixing an enchanted bird or ſquirrel, the charm ceaſes, and the animal delivered from the enchantment takes its flight. This obſervation, the ſtrongeſt in favour of ſuch an opinion, and atteſted by naturaliſts of reputa-tion, appears to me to have much weight, but to require confirmation. Kalm has likewiſe pretended, that when the boiquira is taken and finds itſelf ſhut up, it refuſes all kinds of nouriſhment. The obſervations to be related in this memoir will prove what degree of confidence is to be placed in ſuch reports.

† Let me be allowed to avail myſelf of this opportunity of paying Mr. Peale the tribute which every lover of this beautiful and uſeful ſcience owes to his zeal, his courage, and his conſtancy. Without other aſſiſtance than his love for the productions of nature, and his own induſtry, he has ſucceed-ed in forming a muſeum, already very intereſting, and which will become more and more ſo daily. The Philoſophical Society by accommodating him with

kept alive a boiquira for five years and a half. He has made on this animal, many obſervations, which, if not convincing, eſtabliſh at leaſt a doubt as to all the fables which have been imagined reſpecting this reptile.

* Curious to inquire how this animal ſeizes his prey, he has confined ſeveral birds in the ſame cage with him, and the hungry reptile has made many attempts to take hold of the bird. This experiment has been repeated many times, and every time with the ſame effect. I have ſeen, myſelf, one of theſe birds in the cage, but whether the reptile was not hungry, or was ſenſible of its want of power, it remained perfectly tranquil, while the bird was perfectly at eaſe. It gave no indication which could make it believed that it was either enchanted or affrighted; and the air did not appear different, if we might judge by its behaviour, from that which it found in an ordinary cloſe cage. The bird remained two days in the ſame ſituation, without the leaſt attention paid to it by the reptile, who in the mean time eat a dead one which was preſented to him.

Another living bird was put into the cage with the ſerpent: far from being alarmed, it amuſed itſelf with pecking in the bottom and picking up a few grains which it found there: often changing place in its accuſtomed manner, and even reſting itſelf on the back of the boiquira, which made no extraordinary movements.—This experiment was made ſeveral times.

Mr.

with their building, have given a proof of the protection they afford to whatever may contribute to the progreſs of the ſciences. May this example, well calculated to fix the attention of every good government, be copied by enlightened adminiſtrators, who following the governments of Europe, and principally that of France, may be ſenſible of the value of ſuch an eſtabliſhment, and the neceſſity of encouraging it for the good of the people.

* The following experiments were all made in the ſummer, that is during the ſeaſon in which theſe reptiles take their nouriſhment.

Mr. Peale, his children, and myſelf, have often examined the reptile. We never perceived it to ſend out the ſlighteſt ſuffocating odour. It is in vain to object that the living birds thus given it were not of the kind fitted for its nouriſhment; for it has eaten the ſame birds, when preſented to it dead, and it is not uſeleſs to remark that it never refuſed one of them.

The ſame obſervation is not true of frogs, which, in the opinion of ſome perſons, paſs as the food of the boiquira; Mr. Peale often preſented to it living and dead ones. It never touched them. It never in this reſpect imitated the black ſerpent (coluber conſtrictus. Linn.) This reptile, which Mr. Peale has likewiſe preſerved alive, has eaten the flies, inſects, and frogs (the rana arborea, Linn. among others) which were preſented to it.

Theſe experiments prove: 1ſt, That the boiquira, at leaſt when it is in a ſtate of captivity, has not the power of enchanting, affrighting, or ſuffocating *birds*. 2d, That it does not nouriſh itſelf with *frogs*.

The miſtake, with reſpect to the nature of the food of this reptile, into which Linnæus and other naturaliſts after him have fallen, has been owing without doubt to there being two ſpecies of rattleſnakes; which he has confounded together.

There are within the territory of the United States two known ſpecies of crotalus. The crotalus miliaris and the crotalus horridus of Linnæus. There is however another, well diſtinguiſhed by the inhabitants of the ſouth. The miliaris is called the ground rattleſnake, and is ſo named becauſe it keeps itſelf frequently under ground. When it comes to the ſurface it is moſt pleaſed in the graſs, and is the more dangerous as it is difficult to be perceived. The ſecond is known under the name of the pine-barren rattleſnake; and ſo named, becauſe in the ſummer, that is, in the ſeaſon that it quits its retreat and ſeeks its food, this reptile

reptile is found in thofe dry and arid lands which produce pines only. The third, a non-defcript, and known by the name of the water rattlefnake, is larger than the former, is commonly confined to low grounds, and as it lives near the waters, might be prefumed to make its food of frogs. Neverthelefs I have affifted at the opening of many, (which had been killed for the fake of diminifhing their number, and extracting the greafe, of which an oil is prepared pretended to be of fuperior efficacy in rheumatic and other pains, but which in fact has no advantage over other unctuous fubftances) but I never found in their ftomachs any thing befides birds, fquirrels or rabbits. This new fpecies, which is fpoken of neither by Catefby nor Linnæus, nor by any author with whom I am acquainted, appears to have been confounded with the crotalus horridus. It differs from it notwithftanding, effentially, both by its habits and external form. The boiquira is marked acrofs the back by dark brown tranfverfal lines, a little diagonal, terminated, on each fide, by a fpot almoft round, of the fame colour. The back of the other ferpent is covered with parallelograms or lozenges of a browner colour than the reft of the body, and terminated by a yellow border. Thefe differences may be readily obferved in the annexed figures, Nos. 1 and 2.

After the difcovery of this new fpecies of ferpent, I incline to think that the term *horridus*, which is equally applicable to both, ought no longer to fubfift. I propofe then to call No. 1. crotalus boiquira, a name by which it is generally known: and No. 2. crotalus adamanteus, after the form of the marks upon its back.

Some authors have further advanced that the boiquira was very active in the water and among rocks, but very flow and inactive in the dry land. This is an error, which no doubt owes its origin to the fearfulnefs of thofe who have fallen into it. Both thefe fpecies are more tardy than

other

other ſerpents (colubres). M. Bonnaterre in the introduction to his ophiology, expreſſes himſelf in a manner calculated to perpetuate this error, with reſpect to a reptile much leſs dangerous than is ſuppoſed. "Man himſelf," ſays he, "in ſpite of the dominion he poſſeſſes over all the animals," &c.

It is perfectly well known that the boiquira never attacks man unleſs he has been touched or affrighted. We may paſs very near him without diſturbing him, or his ſhewing the leaſt diſpoſition to bite. It has been ſaid likewiſe, on as ſlight foundation, that he climbs into trees, and ſome naturaliſts have aſſerted with no greater degree of truth, that nature has provided this reptile with little bells or rattles, which make a noiſe as it moves along, to adviſe man of its approach. The rattles of theſe ſnakes make no noiſe while they creep along. When they are frightened or touched, inſtead of flying, they coil upon themſelves, remain motionleſs and ready to dart forward. Then only they move with an inconceivable velocity, the rattles which adviſe us of their vicinity, and which they cannot agitate unleſs they are in a ſtate of anger and contraction incompatible with the act of creeping.

With regard to the impreſſion of terror and alarm which ſome conſider as the ſource of the influence of ſerpents over certain animals, in addition to what I have remarked, it will be ſufficient to compare the ordinary effects of fear with thoſe which are ſaid to be exhibited by birds, ſquirrels, &c. when fixed by a boiquira, to convince ourſelves of the impoſſibility of ſuch a cauſe.

Birds and ſquirrels have other enemies beſides ſerpents. Man, dogs, cats, and many other animals ſhew them no higher favour. On the approach of theſe they fly, and no effect is diſcoverable ſimilar to that which it is pretended they exhibit on the view of a ſerpent.

Well!

Well! perhaps the partifans of the third opinion will exclaim: this power, thefe arms which are not yet difcovered, are neither terror nor enchantment, but a fetid emanation which the reptile cafts around it, and which affects the animals which experience it to that degree that they become incapable of flight. The obfervations of Mr. Peale, already related, contradict this opinion. I fhall oppofe to it another and more recent fact.

Mr. Peale and myfelf had eight living rattlefnakes confined in a box of about eighteen inches fquare. We did not open it before the end of three or four weeks, when, after having taken them out in the prefence of Dr. Deveze, a member of this fociety, one of the fons of Mr. Peale, and of two other perfons, we examined the box with attention and did not perceive the flighteft extraordinary fmell.

I have feen in my excurfions many ferpents irritated, and ready to dart upon me.* I never perceived that they emitted the flighteft odour.

It refults from what I have juft faid, that all which has been reported and written refpecting ferpents to the prefent time, is at leaft very dubious; that the ftudy of thefe animals is, as it were, yet to be commenced: and that it offers to the naturalifts who undertake it, the moft interefting and curious

* The crotalus boiquira, and adamanteus, the mokafen, which I call agkifhodon mokafen, the coluber conftrictor, getalus, ceftivus, and faurita of Linnæus; the Coach-whip fnake of Catefby—the corn fnake of the fame author—another very long one marked like the boiquira but unfurnifhed with rattles, and climbing trees—the ferpent with a copper-coloured belly of Catefby: and feveral other non-defcripts, to enumerate which would take up too much time. All thefe reptiles, upon touching them flightly with a ftick, recoil upon themfelves, raife their heads, and make a hiffing while they open their wide mouths. One day I took in my hand a black fnake, after having irritated and made it wild, it bit me on the lower joint of the fore-finger, two or three drops of blood iffued from the wound, which very much alarmed my guide and feveral perfons who were witneffes, in a few feconds the wound had dried up, and I felt no greater pain than if I had been only pricked by a pin.

curious obſervations and diſcoveries. I ſhall now proceed to detail my own obſervations, and thoſe which I have made conjointly with Mr. Peale.

FIRST OBSERVATION.—Among the information which I endeavoured to obtain in my travels with reſpect to ſerpents in general, there was one point which greatly excited my curioſity. Several perſons, and one among the reſt,* to whom I owe a debt of gratitude for civilities and marks of friendſhip, which will forever reſt engraven on my heart, had informed me that the female rattleſnake concealed its young ones in its body. That when they were alarmed by any noiſe, or by the approach of man, they took refuge in the body of their mother, into which they entered by her mouth. This fact had been already aſcertained with reſpect to the viper of Europe, but in conſequence of the unfavourable and repulſive diſpoſitions inſpired by this kind of reptile, and in order to render it ſtill more hideous, an abſurd interpretation was given to this fact. It was pretended that this ſerpent eats its little ones after having given them birth. Curious to verify this fact, related of the boiquira, I was conſtantly occupied with this idea, and began to deſpair of ever making the obſervation, when at a moment in which I thought the leaſt of it, accident furniſhed me the means. Having

 fallen

* This eſtimable perſon is General Pickens. In a lamentable ſituation, and when my life was in danger among the Indians, I owed my ſafety to the ſtrong recommendation which he gave to the Indian guide and interpreter, which he had procured for me, and to the letters which he had given me for different chiefs. His modeſty will be perhaps affected by the liberty I take of naming him without his knowledge, but he will excuſe this tranſport of my gratitude. This honeſt American, as much beloved by his fellow-citizens as by the Indians, whom he has frequently engaged and defeated in battle, knows how to confer obligations without affectation; to do good is in him a natural movement, ſo much the more to be praiſed, as among the greater part of men, it is the effect of intereſt, pride, or vanity.

fallen ſick among the Indians, I found myſelf obliged to remain a few days with one of them in the neighbourhood of Pine Log. During my convaleſcence I took a walk every morning in the neighbourhood, and one day when I was following a pretty broad path, I perceived, at a diſtance, a ſerpent lying acroſs the road in the ſun. I had a ſtick in my hand, and drew near to kill it, but what was my ſurprize, when, in the moment that I was about to give the blow, the reptile perceived me, coiled upon itſelf, and opened its large mouth, into which five ſerpents, which I had not till then obſerved, becauſe they were lying along its body, ruſhed into the gulph which I had conceived opened for myſelf. I retired to one ſide and hid myſelf behind a tree, the reptile had crawled a few paces, but hearing no further noiſe, and not perceiving me, ſtretched itſelf out afreſh. In a quarter of an hour the young ones came out again. Satisfied with this obſervation I advanced anew towards the animal, with intention to kill it and examine the interior of its ſtomach: but it did not permit me to approach ſo near as it did the firſt time, the young ones entered with ſtill greater precipitation into their retreat, and the boiquira fled into the graſs. My ſatisfaction and aſtoniſhment were ſo great that I did not think of following it.

Second Observation.—On my return to Philadelphia, I recalled to Mr. Peale's recollection the project which we had formed the preceding winter, of going into Jerſey to ſearch for the boiquira in his retreats. He conſented, and with the more readineſs, as he had juſt loſt the one which he had kept alive five years and a half. He in conſequence wrote to Bridgetown to Dr. Elmer. On receiving an anſwer we ſtarted in the month of February; the ſeaſon was already advanced, but not ſo as to render our expedition fruitleſs. Citizen Adet, miniſter of the French republic,

republic, a member of this ſociety, and zealous in the purſuit of ſcience, was to have been of our party, but was unfortunately prevented by illneſs. Arrived at Bridgetown we went forward to Morris River, in company with Dr. Elmer and Mr. White, who loaded us with kindneſs, and facilitated our reſearches by every means in their power. We were to have found at Morris River Captain Hawkins, who is perfectly acquainted with the retreats of theſe reptiles, and deſtroys them every year by hundreds.

The Captain not being yet arrived, we were conducted by an inhabitant to a place where he aſſured us he would ſhew us boiquiras. In the way he made us obſerve on the ſide of a riſing ground on the banks of Morris River, excavations which had been made three weeks before, and in which had been found 75 of theſe reptiles entwined with each other. The hole was from three to four feet deep, and of about the ſame diameter. The way to the bottom of this hole is not ſtraight.

I ſhall remark here, once for all, that boiquiras chooſe for their winter quarters two different ſituations. One on the ſouth ſide of hills, and the other in low grounds, filled with roots, and covered with a thick cotton-like moſs (ſphagnum paluſtre. Linn.). I have remarked, 1ſt, That the expoſure was not always the ſame. 2d, That the way in was tortuous. 3d, The entrance was ſmall enough to prevent the wind from penetrating with too much force. Captain Hawkins told me that he had never met with any of theſe reptiles in holes of which the entrance was larger than their bodies. 4th, In both theſe ſituations there is always found a running ſtream of ſpring water which never freezes at this depth, ſo that the boiquiras in their retreat are always near or over the water, but never in it.

Thoſe naturaliſts who have advanced that theſe reptiles feed on frogs, and ſuch like animals, will doubtleſs not fail to lay hold of this circumſtance and interpret it in fa-

vour

vour of their opinion. But facts are very convincing to the contrary. The boiquiras feek the fides of hills, and the low bottoms in which fprings are found, in order to fhelter themfelves from the cold and froft which makes them perifh. They are in thefe retreats only during the winter, that is to fay, during the time that they are torpid and do not eat. In fummer they always keep upon the heights, in the drieft and moft arid places. I made this remark during my travels, and the fact has been confirmed to me by Captain Hawkins and the inhabitants of New Jerfey, in which ftate thefe animals are in abundance, and where accidents from them are frequent, the foil being generally dry, fandy and arid.

I return to my narrative. Being arrived at the place whither our guide wifhed to conduct us, we began, all of us, to dig where he pointed out. Our refearches were fruitlefs. We hunted in three other places without fuccefs.

The next day Captain Hawkins, having arrived, conducted us four miles, into a low ground, at the foot of a fmall hill. It was covered with birch, dogwood, and other fhrubs, and with a prodigious number of large trees fallen down and rotted, whofe ftumps were yet left. It is under thefe roots that the boiquiras retire. A layer of rich black earth, formed of the remains of vegetables, and two or three inches deep, was covered by a thick bed, about 5 or 6 inches high, of the fphagnum paluftre, below this bed of earth were found, at fmall intervals, fprings which ran through a loofe miry foil into which a ftick might be thruft eafily five or fix feet. It is in the neighbourhood of thefe fprings, and above this miry foil, the reptiles are found which were the object of our inveftigation. Our firft attempt was unfuccefsful. Captain Hawkins opened however another cavity in which we found two boiquiras of moderate fize. In two other places we found

found nothing. Captain Hawkins now conducted us about half a mile further to a low ground nearly ſimilar, leſs covered with wood, but conſiderably loaded with ſphagnum paluſtre, without which we ſhould have ſunk infallibly into the mud, ſo ſoft was the ſoil. The firſt ſearch produced nothing; but the ſecond made us ample amends for the pains we had given ourſelves till that time. In the ſpace of two hours, and in a ſpot of ground about 12 feet long and 8 or 9 wide, we took eight boiquiras of various ſizes, which had each from two to nine rattles.

I had perſuaded myſelf, after the different reports which I had heard, that I ſhould find in the ſame cavities, and mingled with the boiquiras, many other ſpecies of ſerpents and even frogs. Having met with none, I inquired of Captain Hawkins if he had ever made the ſame obſervation. He anſwered it was not rare to find black ſnakes (coluber conſtrictor) mingled with the boiquiras; but he had never met with others, except once that he found in the ſame hole a young garter-ſnake (coluber ſaurita): as to frogs or toads he had ſometimes met with them, but in ſmall numbers, and very rarely.

We aſked him whether he was acquainted with the retreat of other ſerpents, he anſwered in the negative, but ſuppoſed they paſſed their winters in holes at the foot of trees, on the heights. It would not be leſs important to diſcover and inveſtigate the retreat of other reptiles: the true way to do it would be, it appears to me, to obſerve what holes are in the vicinity of the exuviæ, which theſe animals caſt off in the ſpring and autumn, and to ſeek them in ſuch places during the winter.

Third Observation.—The cold was very moderate when we made this ſearch, for about 10 o'clock the thermometer ſtood at 30° or 31° of Fahrenheit's ſcale: at noon we plunged it into the mud above which lay the boiquiras, where we left it ten minutes: it had riſen to

43°

43° when we took it out. At the time when we found ſeven ſerpents together in the ſame place, the ſun had great power, the thermometer in the ſhade being above 40°. The ſerpents began to be ſenſible in this mild temperature: as we put them into the box which we had brought for this purpoſe they moved their rattles; but we did not perceive them to make any efforts to dart forward or bite.

After our return to Philadelphia, our boiquiras remained above three weeks in the ſame box, which, as I have already related, did not emit the ſlighteſt odour when we took them out of it.

Fourth Observation.—At the end of this time we ſhifted them. I choſe out one with rattles, which had been wounded by the blow of a ſtake in digging it out, and therefore could not live long, with the intention to make a few obſervations on the teeth of theſe reptiles. The ſeaſon began to be very mild, the reptiles began to agitate their rattles; but unwilling to loſe ſo favourable an opportunity, I ſeized the boiquira with great caution by my left hand, and holding it very near its head, ſo that by forcing its jaws forward I made its mouth open. I attempted with ſciſſars in my right hand to diſſect out the fleſhy membrane or ſheath which contains one of the fangs. I accidentally burſt the bladder which held the venom, and two or three drops flowed upon my fingers. This liquor was of a clear and tranſparent yellow colour. After the operation, I took a ſmall pair of flat pincers and drew the fang. At the moment I extracted it, five or ſix drops of poiſon came out with force, and flew to the diſtance of about two paces. I proceeded in the ſame manner to get the other fang, but made no venom fly out; and, leſs happy than in my firſt attempt, I brought away with it a portion of the jaw.

My

My intention was, 1ſt, To examine the teeth and fangs of theſe reptiles: 2d, To obſerve whether they would be reproduced, and in what ſpace of time: but the animal dying of its wounds, or rather of the cold which came on two days afterwards, we ſhall not have it in our power to make this laſt obſervation.

FIFTH OBSERVATION.—I put into a ſmall bottle, with water, the teeth of the boiquira, and carried them to Dr. Graſſi, of our ſociety, who, prevented by ſome patients under his care, had not been able to aſſiſt at our experiment, and we examined them together. I opened with attention the fleſhy membrane which I had removed with the tooth, and we found eight teeth attached by a little fibre to a common membrane, as repreſented in Fig. 5. *Theſe teeth* are deſtined to replace the old ones, which, according to all appearance, fall at leaſt once every year.*

SIXTH OBSERVATION.—Having remarked the prodigious quantity of young teeth in this fleſhy membrane, I was curious to learn what is their arrangement while the animal is alive. Mr. Peale had been ſo kind as to prepare for me a young boiquira, one of thoſe which we had taken, and which had died that day. I choſe it for the ſubject of my experiment, it was almoſt dry, I looſened lightly, with a penknife, the fleſhy and dried ſheath which covered one of the fangs, and perceived three teeth of different ſizes, placed one above the other in the manner deſcribed in Fig. 3. Not perceiving any more, I concluded that the ſmall ones had been either removed with the fleſhy part, or were ſo concealed as to render it impoſſible to diſcover them.

* This obſervation is not new. I have ſince found, in conſequence of my reſearches, that this multiplicity of teeth had been already remarked in the European viper (coluber natri Linn.); and that John Bartram mentions the ſame fact in a Memoir on the Boiquira, printed in the Philoſophical Tranſactions. Volume XLI. No. 456.

Seventh Observation.—The above obſervations led me to another fact which I was not looking for, which ſeems to me to explain the prodigious quantity of venomous matter of which I ſpoke in my firſt remarks. Beneath each fang, and towards the middle of the lower jaw, I find a bladder which has a communication with the root of the tooth. This bladder appears to be a reſervoir of the poiſon, whence it is tranſmitted to the lower part of the tooth, in proportion as the animal pours it forth in the act of biting any object. Vid. Fig. 4.

Eighth Observation.—I do not offer this laſt obſervation as a new one. I am ſenſible that all the naturaliſts who have deſcribed the fangs of rattleſnakes and vipers, (for they have both the ſame conformation) have been perfectly acquainted with this ſpecies of tooth; but the deſcription they give of it is ſo inaccurate, that I think myſelf obliged to rectify them. All the deſcriptions lead one to believe, that theſe teeth are hollow at the baſe, in their upper part, and at the extremity. They are in fact pierced at their baſe, and this opening communicates with, or rather is included in the bladder which contains the poiſon; but the hole which correſponds to this is always one or two lines, according to the ſize of the tooth below the point. It is as it were cut obliquely. The interior part of the tooth forms a ſpecies of channel which is prolonged on the outſide from the ſecond opening till near the extremity, as may be ſeen in Fig. 6. which repreſents a tooth through which a briſtle has been paſſed.

I ſhall allow myſelf no reflections on theſe obſervations; but I think I have ſufficiently demonſtrated that we have almoſt every thing yet to learn relating to theſe extraordinary reptiles. Time, with repeated and multiplied obſervations, can alone afford us the information requiſite to form a ſolid judgment on this ſubject: and I am perſuaded we ſhall arrive at the proof, that the pretended effects of enchantment,

enchantment, terror, or a ſuffocating emanation, the produce of that unreflecting horror which theſe reptiles infuſe into the greater part of mankind, are very natural phenomena, and of eaſy explication, as ſoon as obſervers and naturaliſts have learnt to ſhake off their prejudices, and will be bold enough, without raſhneſs, to ſeek theſe animals in their retreats, at all ſeaſons of the year, in order to obſerve them with coolneſs, and without prepoſſeſſion.

We, Mr. Peale and myſelf, propoſe to make experiments upon the poiſon of the boiquira, and we ſhall ſubmit them to the ſociety when the facts and experiments have been ſufficiently repeated and authenticated to eſtabliſh ſome certain truths. I ſhall conclude this memoir by a few reflections on the ſyſtematical diſtribution of ſerpents.

Linnæus was of opinion that the teeth of ſerpents did not afford characters ſufficiently marked to be the foundation of a ſyſtematical arrangement. He made uſe for this purpoſe of the plates or ſcales which cover their belly and the under part of their tail. M. de la Cepede, a ſucceſſor worthy of Buffon, on account of his eloquence and his clearneſs, and ſtill more worthy of eulogium on account of the reſpect which he pays to the moſt celebrated of naturaliſts, the immortal Linnæus, has followed the ſame plan.

M. de la Cepede diſtributes ſerpents into eight genera; namely, *Couleuvres* (coluber) whoſe characteriſtics are large ſcales under the body, and two rows of ſmall ſcales under the tail. *Boa* (boa) which have large ſcales under the body and tail likewiſe. The rattleſnakes *Boiquira* (crotalus) which have large ſcales under the body, and the tail in like manner, but are terminated by rattles, articulated the one into the other and giving out a noiſe.

The *Anguis* (anguis) which are wholly covered with imbricated ſcales.

The *Amphiſbenes* (amphiſbænæ), whoſe body and tail are covered with circular ſcaly rings.

The *Cæciles* (cæciliæ), the ſcales of whoſe body are in folds.

The *Langaha* (langaha), which have large ſcales under the belly, annular ſcales near the anus, and very ſmall ſcales under the tail.

Laſtly, the *Acrochordes* (acrochordes), whoſe belly and tail are furniſhed with little tubercles.

After this diſtribution, it appears that the viper, atropos, ammodytes, and ſeveral which have fangs, and are poiſonous, are confounded with the Coluburs, properly ſo called, which are not ſupplied with this ſpecies of teeth, and which are all harmleſs. It ſeems therefore natural to make a diviſion of this genus already too numerous.

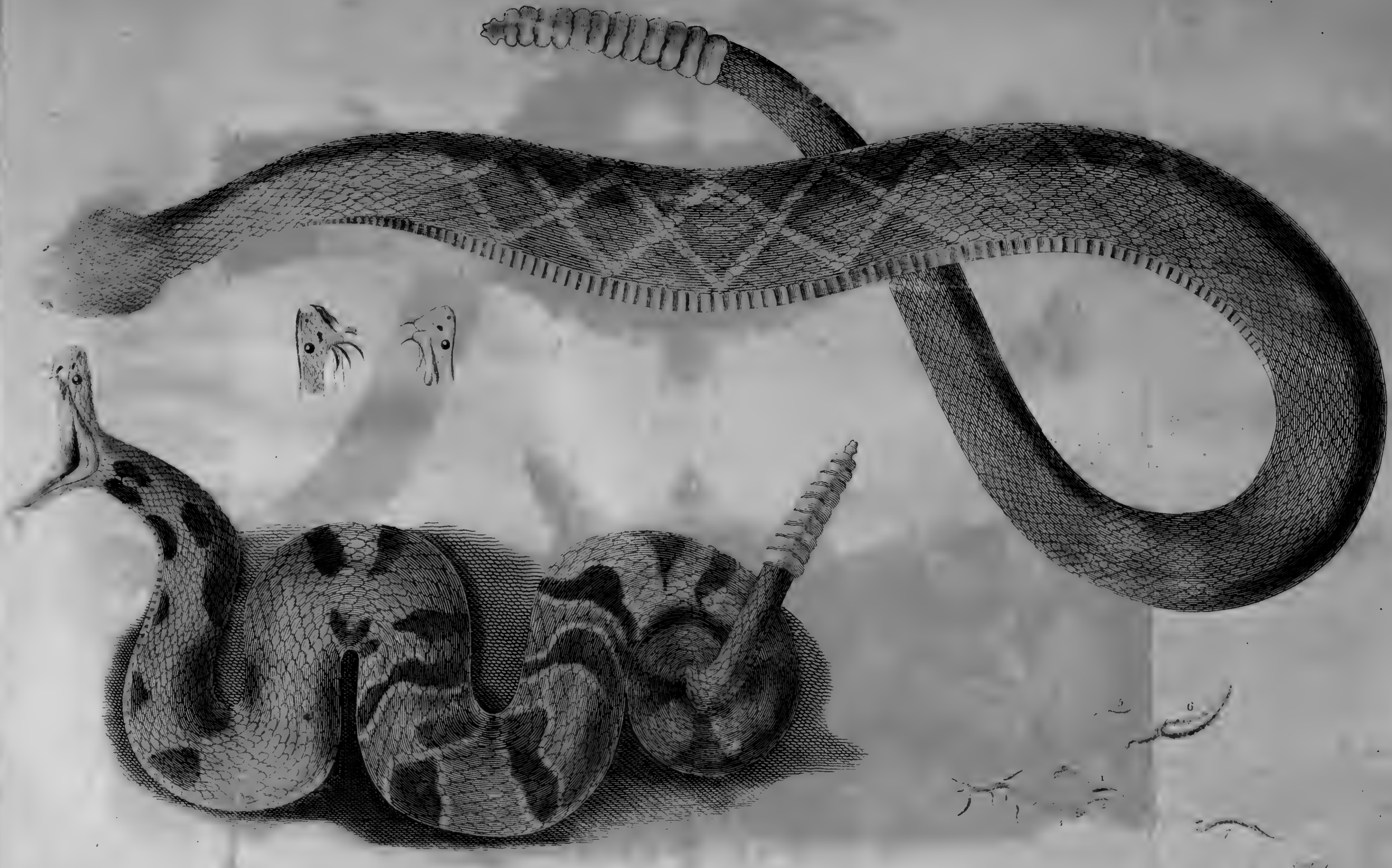
The genus boa offers another confuſion which might be avoided. The greater part of ſerpents of this ſpecies are without teeth. There is moreover in America a non-deſcript ſerpent (the mokaſon) which according to the ſcales under its belly and tail, ought to be arranged among the boas. This ſpecies however have not only teeth, but the extremities of their jaws are furniſhed with fangs like the boiquira.

For theſe reaſons I think * the genus coluber ought to be divided into

Vipers (Vipera), whoſe characters would be large plates or ſcales under the belly. Two rows of imbricated ſcales under the tail. The extremity of the upper jaw on each ſide furniſhed with a hollow fang or canine tooth. Venomous.

(Coluber)

* There is another conſideration in favour of this change. It is that all fanged ſerpents, at leaſt all which I have had occaſion to ſee, appear to me certainly viviparous: perhaps the colubres, properly ſo cited, are all oviparous. This is another fact relating to theſe reptiles very important to aſcertain.



(Coluber). Large ſcales under the belly. Two rows of imbricated ſcales under the tail. All the teeth alike. No fang or canine tooth. Harmleſs.

(Boa). Large ſcales under the belly and tail. The tail without rattles. No teeth.

Cenchris. Large ſcales under the belly and tail. The tail without rattles. Small equal teeth.

Agkiſtrodon. Large ſcales under the belly and tail. No rattles. The extremity of the upper jaw furniſhed with two hollow fangs or canine teeth. Venomous.

In this laſt diviſion ſhould be arranged the mokaſon.

I ſay nothing with reſpect to other genera, in which I have no alteration to propoſe.

An

No. XLIII.

An Appendix to the two Articles in this Volume, (p. 1.) by Dr. Friestley, *in a Letter to* B. S. Barton, M. D.

Dear Sir,

Read, Nov. 23, 1798. SINCE so much time has elapsed since the printing of the articles which had the honour of being inserted in the *Transactions* of your Philosophical Society, I beg leave to add a few more observations before they are published. The experiments which I have made since that time have confirmed all the *facts* reported in them, but not all the *conclusions* which I then drew from them.—1. Because the diminution of a mixture of atmospherical and nitrous air proceeded, in a course of time, much farther than it does presently after they are put together, I concluded that atmospherical air contains a much greater proportion of pure air than had been supposed, on the idea that the latter part of the diminution was owing to the same cause with the former. But I have since found that any kind of air, confined by water, the surface of which is exposed to the common air, will in time be wholly absorbed by it, though in those circumstances it might be supposed that the water in which it stood would be fully saturated with air, and therefore would not be disposed to take any more, especially phlogisticated air, which it never takes in preference to the dephlogisticated. To what this unexpected circumstance is owing, I have not yet satisfied myself, though I have made some progress in the investigation.—Notwithstanding this, I have no doubt but that part of the phlogisticated air that is found after some of the processes for ascertaining the purity of air, is formed at the time, by the phlogiston emitted from the substance that is used for this purpose uniting with the purer er part of the air. Several of my late experiments confirm this. Indeed, the different degrees of the diminution of atmoſpherical air in different proceſſes, and in the different methods of conducting the ſame proceſs, is a full proof of this.

2. I find *ivory black* a more convenient ſubſtance for the experiments recited in my paper than the *black bones* I then made uſe of; firſt giving it as much heat as I can in a ſmith's forge, without any acceſs of air. It becomes white not only when heated in atmoſpherical air, but in phlogiſticated, or inflammable air, the quantity of which is thereby increaſed, by an addition of inflammable air. And that this addition of inflammable air comes from the *bones*, and from the principle that conſtitutes their *blackneſs*, and not from any decompoſition of the *water* over which the proceſs is made, is evident from that water containing no acidity, and its yielding air leſs pure than before; whereas, had the water been decompoſed, ſince it is ſaid that 85 parts in 100 are oxygen, it muſt have been found in the form either of an *acid*, or of *pure air*. That it is not contained in the bones, is clear from their receiving no additional weight.

3. The ſame thing appears to me to be proved by the heating of *zinc*. When this is done in atmoſpherical air, *flowers of zinc* are produced, and the air is diminiſhed. After this the air is increaſed by the addition of inflammable air; and inſtead of flowers of zinc, a *black powder* is ſublimed. If the water be decompoſed in this proceſs, where is the oxygen that muſt be diſengaged at the ſame time with the hydrogen? It is not in the water, or the black powder; for this appears to be nothing more than zinc ſublimed. It becomes white when it is heated in common air, and diminiſhes it.

4. Both iron and zinc, eſpecially the latter, give out much inflammable air in pure water; and yet that water acquires

acquires no acidity, and gives out air more impure after than before the procefs, nor is there any oxygen in what remains of the metals. Where, then, is the oxygen into which it is faid that the water is refolved, at the fame time that it gives out hydrogen, or inflammable air?

5. I have had abundant confirmation of the experiments that I made with *needles*. I made ufe of *fteel* on the fuppofition that, abounding with phlogifton, it would part with more than it would gain in proportion to other fubftances; and that the phlogifton it contained uniting with the pure air would make more phlogifticated air. I lately heated 100 grains of the needles in 7.3 oz. meafures of atmofpherical air, over mercury, till it was reduced to 6.5 oz. meafures, without any fenfible quantity of fixed or inflammable air in it, being wholly phlogifticated; a diminution fo much lefs than ufual, that much phlogifticated air muft have been formed in the procefs. As the needles had not gained or loft any fenfible weight, fomething muft have been thrown off from them, though it could not be collected; and this could only have been from fomething going out of them, and not by any thing entering into them.

6. It is faid that when *red precipitate*, which is a calx of mercury, is heated in inflammable air, the pure air expelled from it uniting with the inflammable air, forms *water*. But in my late experiments I have had the cleareft proof that it does not form either water or any other fubftance, but remains mixed with the remainder of the inflammable air, while it imbibes fome of the inflammable air, and is revived by it. This appears from examining the air that remains, and which is found to contain a portion of pure air, and efpecially from the *explofion* of the two kinds of air, which has more than once happened to me, and is not a little dangerous. I find by computation, that fo much inflammable air is abforbed in the revival of red precipitate, that an ounce of mercury will ab-

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ſorb not leſs than 362 ounce meaſures of it, or the phlogiſton contained in it. An ounce of lead, I have ſhewn, takes 108 ounce meaſures of this air, an ounce of biſmuth 185, and an ounce of tin 377 ounce meaſures of inflammable air; ſo that mercury contains more phlogiſton than two of thoſe metals; and notwithſtanding this it may be revived in a glaſs retort, without any inflammable air at all. Here is a great difficulty, no doubt; but it may be ſolved by ſuppoſing that this ſubſtance is capable of attracting phlogiſton through the hot glaſs. And if *light*, and *heat*, both acknowledged *ſubſtances*, can penetrate glaſs, why may not *phlogiſton?* This muſt either be ſuppoſed, or that an ounce of mercury may either contain all the phlogiſton in 362 ounce meaſures of inflammable air, or none at all, and yet be the ſame thing, having all the ſame chemical properties. Let philoſophers conſider this caſe with impartiality, and form the beſt theory that they can to account for the facts. Though ſo much ſtreſs has been laid on the experiment with this calx of mercury, as a foundation for the new theory, it is by no means concluſive in its favour.

Had the publication of your Tranſactions been more frequent, I ſhould with much pleaſure have ſubmitted to the ſociety a full account of theſe and other experiments, which appear to me to prove, that metals are compound ſubſtances, and that water has not yet been decompoſed by any proceſs that we are acquainted with. Still, however, I would not be very poſitive, as the contrary is maintained by almoſt all the chemiſts of the age, and therefore their hypotheſis requires to be conſidered with the greateſt attention. This I ſhall continue to give to it; and certainly it is high time to decide this queſtion; ſince a great part of the ſyſtem of chemiſtry depends upon it, and a falſe theory may retard the progreſs of this important branch of ſcience.

Wiſhing the continuance of your ſucceſs in the ſeveral valuable inquiries in which you are engaged, and which has already gained you much deſerved reputation, I am,

Dear Sir,

Yours ſincerely,

J. PRIESTLEY.

Northumberland, Aug. 8, 1798.

In all my computations of the quantity of pure air contained in a portion of atmoſpherical air, I have of late years followed the example of others in *agitating* the mixture of nitrous air with it. But I have lately obſerved that this agitation only promotes the abſorption of part of the nitrous air by the water, and not the union of the two kinds of air, as has generally been ſuppoſed. Conſequently, my original method of mixing them *without agitation* is preferable to it, and this gives the proportion of pure air in atmoſpherical air more agreeable to the truth, viz. about 27 parts in 100; whereas, if the computation be made after the agitation of equal quantities of the two kinds of air, the proportion will be about 33 in 100. After agitation, one meaſure of atmoſpherical air and one of nitrous will generally occupy the ſpace of 1.01 or 1.02 meaſures; when without agitation, it will be about 1.25; and this alſo the reſult of firing together an equal quantity of inflammable and atmoſpherical air.

J. PRIESTLEY.

An

No. LXIV.

An Inquiry into the comparative effects of the Opium Officinarum, extracted from the Papaver Somniferum or White Poppy of Linnæus; and of that procured from the Lactuca Sativa, or common cultivated Lettuce of the same author. By JOHN REDMAN COXE, M. D. An Honorary Member of the Philadelphia Medical Society; and a Senior Member of the Chemical Society of Philadelphia.

GENTLEMEN,

Read, Nov. 24, 1797. IN the boundlefs fields of inquiry which the book of nature opens to our view in the extenfive regions of America, much remains to be inveftigated. Our forefts, our fields and rivers, our mountains, and the bowels of the earth, alike invite attention from the philofophic mind. Too long has a fupine inactivity prevented our benefitting by the bounty of nature. She is not coy; yet fhe requires purfuit from thofe who wifh to fecure her: thofe alone who feek her, will fhe meet with a fmile, and conduct them to the temple of honour and fortune. Proteus-like fhe affumes every form, and thus fuits herfelf to the moft fantaftic imaginations.

The rugged afpect of the entrance to the various avenues of knowledge has deterred many from its purfuit, who if they had made the leaft advance, would have perceived a fpeedy termination to the labyrinth before them, and a luxuriant profpect unfolding to their view, and growing more delightful in proportion as they proceeded.

Among the various objects which nature holds up to our view, none are more deferving of inveftigation than the *vegetable* kingdom.—Here we difcover, plants fitted to nourifh and to preferve life; whilft others ferve, by their grateful fruits and odours to gratify the fenfes of tafte and

and ſmell; or by their brilliant colours, the eye of man. By the noble diſcovery of the art of dying, many of theſe colours have become tributary to taſte, by their tranſmiſſion to, and fixation in, other bodies; nature is thus improved upon, by rendering permanent and fixed theſe her fugacious and tranſient ornaments. In medicine, many of the moſt valuable articles of the *Materia Medica* are derived from this ſource; witneſs the ipecacuanha, jalap, rheubarb, gamboge, bark, and opium, with many others which might be mentioned, of leſs note.

Wherever we look, we find nature tributary to the labours of man. Her luxuriance is increaſed; ſhe ſeems anxious to remunerate our fatigue, and to diminiſh as far as is in her power the curſe inflicted upon the human race, in the perſons of our firſt parents, of "eating their bread with the ſweat of their brow."

Though the bounty of nature is thus variouſly extended throughout the regions of the earth, it is not the leſs our duty and intereſt, to endeavour to diſcover ſuch articles in our own country, as are ſimilar or analagous to thoſe which we obtain by importation from diſtant places; or at leaſt to draw from other countries thoſe riches which will prove equally productive, when naturaliz'd to our ſoil and climate. In the immenſe extent of the United States, may be found almoſt every climate from the torrid to the frigid zone. Let us not then deſpair of ultimately poſſeſſing among ourſelves, all thoſe invaluable ſources of health and nutrition which are drawn from the vegetable creation in every part of the globe.

The *potatoe* is not a native of our climate, nor of the European countries in which it is cultivated; yet it is one of the moſt uſeful of the vegetable tribe, and grows among us as luxuriantly as in its native ſoil of South America. The rheubarb, though not natural to the clime of Great Britain, by cultivation, has there become tributary to the

wants

wants of man. Let us not then longer than is neceſſary be dependant upon foreign countries for the production of ſuch ſubſtances as our own will afford us: let us ſeek in our extenſive regions thoſe treaſures of the vegetable world, which now droop unnoticed, "and waſte their ſweetneſs in the deſert air;" and which by cultivation may become ſuch articles of commerce as amply to repay any labour expended upon them. We have too long laviſhed our treaſures upon foreign productions; let us now in turn render foreign countries tributary to us.

Having ſaid thus much, I ſhall now proceed to treat of one of the moſt valuable articles of the Materia Medica, in a curſory way, as an introduction to the ſubject of the following paper; and which I truſt from its importance will be found worthy of the attention and particular notice of your reſpectable body.

The ſubſtance I propoſe to conſider, is OPIUM; emphatically ſtyled by ſome authors, "Magnum Dei Donum," and in the claſs of ſtimulants regarded as the principal.

The plant which has *hitherto* yielded for the ſhop this invaluable drug is the *papaver ſomniferum* or *white poppy*; in the claſs polyandria and order monoginia of Linnæus. It is an annual plant; from the heads or capſules of which, the opium is obtained in Perſia, Arabia, and other warm regions of Aſia. Both the ſmell and taſte reſide in a milky juice, which is moſt copious in the cortical part of the capſules; though the leaves and ſtalks poſſeſs it in a leſs degree. This milky juice in a concrete ſtate, forms the officinal opium. *Kæmpfer* and others have long ago deſcribed the manner in which it is collected: but the moſt circumſtantial detail of the culture of the poppy, and the method employed to procure the opium from it, is that

given

given by *Mr. Kerr*, as practiſed in the province of *Bahar**.

The pureſt kind of opium is chiefly retained for the uſe of the inhabitants of thoſe countries in which it is prepared; who being debarred by their religion, from wine or ardent ſpirits; accuſtom themſelves to a ſtill more pernicious luxury, by raiſing their enfeebled ideas with the ſtimulus of opium.

The quantity taken by ſome in the ſpace of twenty-four hours is truly ſurpriſing. It is true, that the uſe of it in the commencement is very moderate; but like dram-drinking becomes more neceſſary each day, to the exiſtence of thoſe who are accuſtomed to its influence.

According to Mr. Baumé *opium* conſiſts of an *extractive* matter ſoluble in water; a *reſin*; a volatile concrete oil; and a peculiar ſalt; exiſting in the following proportions.

4 ℔ of common *opium* yielded	℔.	℥.	ʒ.
Of *Inſoluble* matter, - -	1	1	0
Extractive matter, - -	1	15	0
Reſin, - - - -	0	12	0
Oil, - - - -	0	3	7
Saline matter, - - -	0	0	1
Equal to 64 ℥. or - - ℔.	4	0	0

In the hiſtory above referred to, of the cultivation of the poppy, previouſly to obtaining from it this invaluable drug, may be remarked the extreme labour and attention requiſite to its production. If then this time and labour can be ſaved, it muſt certainly prove beneficial to mankind, by diminiſhing the price of this uſeful remedy. Nor is

* See Woodville's Medical Botany. Vol. III. p. 505.

is there any danger that this diminiſhed price ſhould tend to increaſe the number of thoſe unfortunate wretches, who blindly ſeek to bury their faults or their misfortunes with them in the grave, by the impious and cowardly act of ſuicide. The avenues of death are too numerous, and the price of this balm to human miſery (when properly applied) is much too inconſiderable, to deter from his purpoſe a perſon, intent on terminating his exiſtence.

The *lactuca ſativa*, or common cultivated garden lettuce, is ranked by that great naturaliſt Linnæus in his claſs *ſyngeneſia*, order *polygamia equalis*, and is thus deſcribed.

" Lactuca. Receptacle naked. Calyx imbricated, cylindric, with membranous margins. Pappus ſimple, ſtiped. Seeds poliſhed."

The genus Lactuca compriſes according to Linnæus *ſeven* ſpecies; of which this is the ſecond, and is deſcribed thus, " *lactuca ſativa*, with leaves rounded on the ſtem hearted, ſtem corymbed."*

Since the time of this great man ſeveral other ſpecies have been enumerated; it is not however my intention to ſpeak of any other than the one above mentioned.

The *lettuce* had long been known to poſſeſs *narcotic* properties. None however had extracted from it a ſubſtance poſſeſſing all the properties of opium in the fulleſt degree: it was chiefly from tradition that its effects were known, and by obſerving that people were rendered ſleepy by eating old lettuce. It is the more remarkable, becauſe, as we ſhall ſee preſently, ſome have arrived at the very threſhold of the diſcovery, but have ſtopped from the purſuit.

As far back as the year 1792, and long before I had peruſed any author, upon the ſubject of lettuce, it occurred

* Syſtem of Vegetables of Linnæus, tranſlated by a Botanical Society of Litchfield. London 1783.

curred to me to try ſome few experiments, to determine the quality and nature of that milky juice which exudes from this plant in copious ſtreams when wounded; and this I was induced to do, from the well known effect of the plant in cauſing ſleepineſs when old; as well as from its peculiar ſmell and taſte.

Theſe experiments, at that time few in number, convinced me of the truth of the analogy which I had drawn between the *common officinal* opium, and the milky juice of this plant; for with a ſmall quantity of extract obtained by inſpiſſating this juice, I found ſimilar effects induced upon myſelf when taken internally in the ſame doſes with *opium* of the poppy. The moſt pleaſing ſleep was brought on by *one grain* of the extract, or by *fifteen* drops of the tincture made with proof ſpirit. By ſimilar experiments ſince tried upon myſelf in England, I found the ſame effects; and a repetition of them within a few months paſt, proves them ſtrictly the ſame. I have occaſionally removed in myſelf a ſlight cholic, with twelve or fifteen drops of the tincture: and a ſeries of comparative experiments upon frogs, &c. which I ſhall here detail, aſſure me by their uniformity of the *identity* of the *opium* extracted from the poppy and of that procured from the lettuce.

Before I proceed to relate the experiments I have made upon this ſubject, I muſt be permitted to ſhew, by quotations from ſeveral authors, how nearly they had reached the diſcovery of this fact. JONES, a celebrated author, who publiſhed in 1701 his " *Myſteries of Opium revealed*," in ſpeaking " of the election (or choice) of opium," ſays; " 3. It was mixed with juice of *lactuca ſylveſtris*, or *wild endive leaved lettuce*."

" This made it of a duller *colour*, and not to ſmell ſo perfectly and rankly of the *poppy*." He ſoon after, adds, " but *lactuca ſylveſtris* being of the nature of *opium*, made the loſs of its *virtue* leſs diſcernible." See p. 13.

Dr.

Dr. CHARLES ALSTON, in the 5th vol. of the Edinburgh Medical Eſſays and Obſervations, p. 105. in his diſſertation on *opium* after mentioning ſeveral articles with which it is reported to be adulterated, adds, " I know not the *glaucium* of the ancients, nor did I ever ſee any *opium* that I had reaſon to ſuſpect as adulterated with *gum* or *ſuet*; but the *wild lettuce*, that is, the *lactuca ſylveſtris, odore viroſo*, C. B. Pin. 123. abounds *more than any poppy* I know, with a milk of the ſame taſte and ſmell; perhaps therefore this, if it can be more eaſily collected, may ſtill in ſome places be mixed with *opium*, and the medicine be nothing the worſe for it, the milk of even the *common lettuce* being anodyne and ſomniferous, as well as that of the poppies."

See alſo his 57th. lecture in the 2d vol. of his Materia Medica, p. 153. et ſeq.

HILL in his *Britiſh Herbal*, p. 436, under the head of *lactuca*, has the following: " Diviſion I. 1. *Great wild lettuce. Lactuca ſylveſtris major opii odore.*"

" The root is long, thick and whitiſh; and when cut, it yields abundantly a yellow juice, of a very unpleaſant ſmell, reſembling that of opium; and of a bitter, nauſeous taſte." " C. Bauhine calls it, *lactuca ſylveſtris odore viroſo*. Others, *lactuca ſylveſtris major odore opii.*"

" This is one of thoſe Engliſh plants which deſerves to be more known in medicine. It has been called poiſonous, and men have from that been frighted from its uſe; but it is a very *gentle* and a *ſafe opiate*. The beſt way of giving it is in a ſyrup made from a decoction of the freſh leaves and ſtalk. This way it greatly exceeds the common diacodium, and may be given to tender conſtitutions with more ſafety. This I write from experience."

DALE has deſcribed the *lettuce* alſo in his Pharmacologia, p. 80. In this the different characteriſtic names of various authors are brought together. *J. Bauhine* calls it " *lactuca ſylveſtris lato folio, ſucco viroſo.*" I. B. ii. 1002. DIOS-

CORIDES has ſaid, according to *Dale*, that it mitigates pain.

DALE has alſo made a ſecond ſpecies or variety of the *lactuca ſylveſtris*, under the diſtinguiſhing mark of, " *L. ſylv. coſta ſpinoſa*, or *jagged leav'd wild lettuce*." I ſhall here quote his own words.

" Lactuca ſylveſtris ſativæ ſimilis eſt (ut ſcribit Dioſcorides) ſed longior caulis, et folia gracioliora, et aſperiora; maro guſtu eſt. Quæ de viribus lactucæ ſylveſtris veteres prodiderunt, quod ſcilicet ſemen ejus non minus quam ſativæ libidinum imaginationes in ſomno amolitur, et venerem arcet; cui huic plantæ conveniant, dubitat *D. Raius*. Narcoticam eam eſſe et ſoporiferam, adeoque (ut rectè obſervat) *viribus papaveri ſimilem*, ut *Dioſcorides* et *Plinius* tradunt, *opii vehemens et viroſus odor abunde convincit*," et ſeq.

Theſe quotations will ſuffice to prove, that however analagous their authors might ſuſpect the *officinal opium* and the juice of the lettuce to be; they had not put it to the teſt of experiment. I now proceed to ſtate thoſe which I have made.

LETTUCE OPIUM.	COMMON OPIUM.
EXPERIMENT I.	*EXPERIMENT* 2.
July 1ſt. 1797. To one ounce of rain water, I added 5 grs. of the *opium* of the lettuce in the vial marked, A.	The ſame day I added a ſimilar quantity of rain water to 5 grs. of the *opium* of the poppy, in the vial marked, B.

I frequently agitated both vials, and on the 21ſt of the month, I found by filtration, only one grain and a half, left on the filtre of the vial A. whilſt 2 grains were left on that of B.

This difference of half a grain I at firſt aſcribed to the common opium being much more dry than that of the lettuce which was freſhly made; and hence, in an equal weight not containing ſo great a proportion of fixed matter.

The colour of the ſolution A. was however much deeper than that of B. and ſucceeding experiments convinced me that the quantity of *extractive* matter in the *lettuce opium*, is conſiderably greater than in the common. By the aid of my ingenious and worthy friend Dr. *Cooper*, of this city, I obtained a larger quantity of the *lettuce* opium, with which I was enabled to make the following.

LETTUCE OPIUM.

Experiment 3.

Auguſt 14th. I put 20 grains of *this opium* thoroughly dried, into *two* ounces of filtered rain water, in the vial A. and after repeated agitations, I filtered it on the 18th. When dried, there were left on the filtre, 10 grains, ſo that *one half*, was pretty accurately held in ſolution. The filtered ſolution was of a dark brown colour, poſſeſſing much of the taſte and ſmell of *opium*.

COMMON OPIUM.

Experiment 4.

Auguſt 19th. I put 20 grains of *common* opium into a ſimilar quantity of rain water in the vial B. On the 24th, I filtered it after frequent agitations. There remained on the filtre, when dried, grains 11, which conſequently leaves but 9 twentieths, diſſolved by the water. The filtered ſolution was not nearly of ſo deep a colour, as that of A.

To both of theſe ſolutions I added about a drachm of alkohol, to prevent putrefaction.

LETTUCE OPIUM.

Experiment 5.

Auguſt 19th. I put the 10 grains remaining on the filtre of A. (experiment 3.) into an half ounce of alkohol. I filtered it on the 29th, and found 7 grains left on the filtre, which when waſhed and dried, was devoid of taſte or ſmell. The quantity of *reſinous* matter then was 3 grains, or rather more than one 7th of the whole maſs.

COMMON OPIUM.

Experiment 6.

Auguſt 26th. I put the 11 grains remaining on the filtre of B. (experiment 4.) into the ſame quantity of alkohol. I filtered it on the 29th, and found 8 grains left on the filtre; devoid of taſte and ſmell, when waſhed and dried. Here then the proportions agree.

The colour of *this* ſolution in alkohol was much deeper than that of experiment 5.

Neither of the above ſolutions poſſeſſed to any conſiderable degree the peculiar ſmell or taſte of *opium*; probably from the large proportion of alkohol. The *reſin* was precipitated from the ſolutions in alkohol, by the addition of water. That of the lettuce appeared to me *whiter* than the other; but not ſo copious: the opium taſte, &c. was more evident in the water. Its reſin was more evident by ſtanding ſome days.

LETTUCE OPIUM.

Experiment 7.

Auguſt 19th. I put 20 grains of the *lettuce* opium into the vial A. and added to it one ounce of a mixture of equal parts of alkohol and rain water. On the 29th, after repeated agitation I filtered it and found that $12\frac{1}{2}$ grains had been taken up, as $7\frac{1}{2}$ remained on the filtre after waſhing and drying. The ſolution eminently poſſeſſed the ſmell and taſte of laudanum; and was of an higher colour than that of the following experiment.

COMMON OPIUM.

Experiment 8.

The ſame day, I put a ſimilar quantity of *common* opium into the vial B. and added the ſame quantity of the mixture of the alkohol and water. By filtration on the 29th, *ſeven* grains were left upon the filtre; or 13 grains were ſuſpended in the ſolution.

This difference I regard as proceeding from a ſmall allowance not being made in the weight of the *opium* of the *lettuce*, which had not dried thoroughly; and hence not containing as much *ſolid* matter in the whole maſs.

The maſs left on the filtre A. was of a more *gummy* feel than that of B. and not of ſo high a colour; the ſmell or taſte of opium was not very evident in either of them.

With the ſolutions of experiments 3 and 4, I proceeded now to make the following.

LETTUCE OPIUM.

Experiment 9.

To a ſolution of ſugar of lead, I added 30 drops of the aqueous ſolution of the *opium lactucæ*; a copious brown coloured precipitate inſtantly formed. The *opium* ſmell was evident.

COMMON OPIUM.

Experiment 10.

A ſimilar effect took place with the acetite of lead, and the aqueous ſolution of common opium. The precipitate was not as dark as the former; the *opium* ſmell was evident.

Experiment

LETTUCE OPIUM.

Experiment. 11.

To a ſolution of ſulphate of iron (green vitriol) I added 20 drops of the aqueous ſolution. A browniſh coloured precipitate was formed; but not very copious. The ſupernatant liquor upon the ſubſidence of the precipitate was of a *dirty green*; as was alſo the precipitate itſelf upon ſtanding. The *opium* ſmell was retained.

Experiment 13.

To a ſolution of *hepar arſenicum* (made with orpiment and quicklime) I added 20 drops of the above aqueous ſolution; a *brown* and pretty copious precipitate was here formed. The ſupernatant liquor appeared clear. The hepatic ſmell ſeemed to be augmented by the union of the two ſolutions.

Experiment 15.

To one drachm of *lime-water*, I added 20 drops; a browniſh precipitate was formed. The opium ſmell remained.

Experiment 17.

I added 20 drops, to 15 drops of *nitrate of ſilver*, diluted with rain water; a light coloured cloud gradually formed itſelf after ſtanding ſome time.

Experiment 19.

I added 20 drops to a ſolution of *carbonate of ammonia*; a browniſh coloured precipitate took place. The peculiar ſmell of both ſolutions, was evident.

COMMON OPIUM.

Experiment. 12.

The precipitate here was of a darker colour, but in the ſame proportion apparently. The ſolution was itſelf of a *brown* colour, and retained the *opium* ſmell.

Experiment 14.

The precipitate here, was of a light *green* colour, and very ſmall in quantity, until after ſtanding a conſiderable time, when the cloud began to ſubſide of a browniſh or dirty green colour. The hepatic ſmell ſeemed increaſed.

Experiment 16.

In this experiment, a browniſh precipitate was likewiſe formed, though leſs abundant; the opium ſmell remained.

Experiment 18.

The ſame effect, but in a leſs degree, took place in this experiment.

Experiment 20.

In this experiment the ſame effects took place.

LETTUCE

LETTUCE OPIUM.	COMMON OPIUM.
Experiment 21.	*Experiment* 22.
I added 20 drops to a diluted ſolution of *nitrat of copper*. The green colour of the latter, predominated; but a very lightiſh brown coloured precipitate gradually ſubſided.	In this experiment the brown colour of the ſolution of opium predominated, and a beautiful clear ſolution remained which did not become cloudy after ſtanding 10 minutes.
Experiment 23.	*Experiment* 24.
A copious lightiſh brown coloured precipitate was produced by adding 20 drops, to a diluted ſolution of *nitrat of mercury*.	A ſimilar effect took place in this experiment.
Experiment 25.	*Experiment* 26.
Alkohol, diluted with water, produced no effect when added to the above ſolution.	This experiment proved the ſame.

As in the above related experiments, the general effects of the *two ſpecies* of opium were pretty nearly ſimilar, with *chemical* teſts; I thought a ſet of *comparative* experiments made upon frogs, would be proper to illuſtrate ſtill farther this identity. I therefore ſubmitted ſeveral to the action of the *opium* in the manner following.

Experiment 27.

July 1ſt. In a vial (C.) I put 8 grains of the *opium lactucæ*, and added by meaſure half an ounce of good brandy. On the 14th of Auguſt, I filtered it, and found 2 grains left upon the filtre. This I put into the ſame vial C. and added half an ounce of filtered rain water. The maſs was of a *gummy* nature, poſſeſſing nothing of the peculiar *taſte* of opium, and but little of the *ſmell*. On the 16th, after filtering it, I ſtill found 2 grains remaining. The water had acquired

acquired an evident bitter tafte, and a flight fmell; which was doubtlefs owing to my neglect of *wafhing* the mafs previoufly to the addition of the water.

Experiment 28.

Auguft 20th. At 12 o'clock, I injected a portion of this *aqueous* folution (experiment 27.) between the fkin and mufcles of the *right* lower extremity, of a fine lively bul-frog. At the fame time I alfo injected between the muf-cles and the fkin of the *left* lower extremity, a mixture of *one* part of alkohol, and *two* of water. He did not appear fenfible of pain at the introduction of either, but leaped about in the receiver, in which I confined him, with great vigor. At 10 minutes after 12, he appeared equally vigorous; as he did at the expiration of 15. I now injected fome more of the fame folution under the fkin of the *right* extremity, but found no alteration evinced at the end of 10 minutes. I therefore introduced a third portion, at the diftance of 25 minutes from the firft; but without any alteration, excepting a flight convulfion, in drawing the leg to the body, and which probably was occafioned by the irritation of the inftrument ufed in injecting the folution. Finding no effect produced by the folution upon the muf-cles of the extremities, I injected a portion into the *ftomach* at 30 minutes after 12. At 35 minutes after 12, the *right* leg was moved with confiderable difficulty; and generally remained in an *extended* pofition, unlefs ftruck or otherwife irritated; when it was drawn forwards pretty forcibly. The frog could ufe it very well in the action of jumping; and he did not feem affected by what was taken into the ftomach, except that refpiration appeared to be increafed at the end of 10 minutes.

A portion injected into the *rectum*, produced no effect; and his legs had regained their perfect ufe.

At

At 10 minutes before one o'clock, I introduced between the ſkin and muſcles of the *left* leg (which had had the alkohol and water injected into it at the commencement of the experiment) a portion of the *aqueous ſolution* of the *opium lactucæ*, of the vial A. (experiment 3.) At this time the frog was very lively. Much of the ſolution was diſcharged by the motion of the leg in placing him under a receiver: in 5 minutes he moved about briſkly; in 10 his *left* leg began to drag. At this period of the experiment, ſomething occurred to carry me away, and I put the frog into the water.

Experiment 29.

Auguſt 21ſt. The frog, the ſubject of the preceding experiment, had perfectly regained his livelineſs and animation. At 30 minutes after one o'clock, I injected a few drops of the *aqueous* ſolution A. (experiment 3.) into his ſtomach. In 3 minutes, conſiderable contractions of his abdomen appeared, and continued at intervals; with an opening of the fauces, as if to obviate the difficulty of reſpiration. By agitating him, he was made to move with conſiderable briſkneſs. At 15 minutes before 2, he appeared very lively. I injected a ſecond portion into his ſtomach, and a third, at 20 minutes after 2, as he ſtill continued very lively. Neither of theſe appeared to affect him. At 25 minutes after 3, I injected a portion, through a ſmall inciſion, into the abdomen; a conſiderable part of it was rejected; but his lower limbs were paralized to a certain degree in 5 minutes. He could not jump, but drew his legs after him with much difficulty. In 12 minutes he could jump ſlightly. At 45 minutes after 3, I injected more, and retained it by keeping him upon his back. At 4 o'clock he jumped pretty well. At 30 minutes paſt 4, he continued lively. I now put him into the water, but found him *dead* the following day.

His

His death arose, in all probability from *inflammation*, induced in the abdomen, by the incision made into it for the introduction of the solution; at least it must have had some influence.

Experiment 30.

Neither the *aqueous solution* A. (experiment 3.) nor *alkohol* and *water*, appeared to affect this frog when dropped upon the naked eye.

Experiment 31.

Several drops of the *aqueous solution* A. (experiment 3.) were dropped into my *right* eye. It gave me some degree of pain, which was not of long duration. I felt no other inconvenience from it; but a slight inflammation for some hours was evident in it.

Experiment 32.

After separating by inflation the skin and muscles of the *right* inferior extremity of a fine active frog; I injected, at 20 minutes before 4, several drops of the *aqueous solution* of *opium lactucæ*, A. (experiment 3.) In 5 minutes little effect was induced. In 10 he experienced some difficulty in moving it, and it was accompanied with a dragging motion. In 15 minutes still greater difficulty. Upon extending the leg it was retained in that position; whilst the *left* was quickly drawn up to the body; yet when the *right* leg was irritated, it was exerted with considerable facility.

In 20 minutes the sense of feeling seemed to be in some degree impeded; for it did not appear to evince by any contractions, that it felt pain from a pointed instrument in this leg, though in the *left*, it was very evident.

At 5 minutes paſt 4, I introduced ſome of the ſame ſolution beneath the ſkin of the *left* leg. At 10 minutes paſt 4 there was conſiderable difficulty in moving this leg; and his motion ſeemed now to be performed by puſhing himſelf on with his fore-legs. If he wiſhed to jump, he was compelled to puſh his body back upon his hind legs, inſtead of drawing them up to his body. The jump was only the length of his hind legs, which then remained extended as before. A ſilver probe introduced into the opening made to inject the ſolution, produced convulſions in both legs, by the aid of zinc. At 15 minutes after 4, both his legs appeared perfectly paralytic. At 20 minutes after 4, I injected ſome of the ſame ſolution under the ſkin of the abdomen, which ſeemed in ſome degree, after a ſhort time to paralize his fore-legs.

The ſolution applied to the naked eye of the frog did not ſeem to affect it in the leaſt, as it did not cauſe it to cover it with the lids.

At 30 minutes after 4, I injected ſome drops into the ſtomach, which ſeemed at firſt to convulſe it conſiderably; It appeared to ſtrive to vomit, opening its mouth to the utmoſt extent, and making repeated convulſive motions of the œſophagus. It could not now move its lower limbs, though they were occaſionally convulſed; and violent convulſions were induced by zinc and ſilver.

At 20 minutes before 5, it ſeemed to have expired, but by introducing a few more drops into the ſtomach, a ſlight convulſion was induced in about a minute. At 15 minutes before 5, it was completely dead.

Ten minutes before 5, I opened the thorax and abdomen. The heart beat 80 pretty vigorous pulſations in a minute. After removing the pericardium, I put a drop of the ſolution upon the heart, which did not appear to diminiſh its frequency. I now removed it from the thorax, and put it into ſome drops of the ſolution, which ſeemed ſoon to check it, it, for at 5 o'clock it beat only 50 weak pulſations in a minute, and at 10 minutes paſt 5, only 18, and chiefly of the auricle. A pointed inſtrument ſcarcely increaſed its vigour.

The ſtomach was corrugated, and contained the ſolution mixed with a ſlimy matter.

Experiment 33.

At 15 minutes before 2, P. M. I injected a few drops of the aqueous ſolution A. into the abdomen of a lively frog, the greateſt part of which eſcaped. Though the frog was *ſtifly* contracted before the introduction of the ſolution; yet the abdominal muſcles relaxed and elongated themſelves the inſtant it was introduced.

At the expiration of 5 minutes no effect was produced. At 10 minutes being equally lively, I introduced another portion and retained it there for ſome time.

In 10 minutes he *lay* upon his abdomen, not as uſual reſting upon his legs. Irritation with a pointed inſtrument, did not now cauſe his extremities to contract; they appeared perfectly paralized. When placed upon his back, he lay without motion. His eyes were ſenſible to irritation.

In 20 minutes he began ſlowly to move his lower, and ſoon after his upper extremities, and gradually elevated himſelf upon them as uſual. Contractions were produced by zinc and a ſilver probe paſſed into the abdomen.

In 5 minutes from this time he appeared to be nearly dead, and was completely ſo in two or three minutes longer. At 3 o'clock his limbs were nearly ſtiff.

On opening the thorax the heart was beating 60 vigorous pulſations in a minute. I removed it from the body, and in 15 minutes it pulſated only 32, and chiefly of the *auricle*. In 30 minutes after 3, it beat only 10 times. At 45 minutes, it was excited to a few weak pulſations by a pointed inſtrument.

The length of time in which contractions may be induced by metallic ſubſtances, in the frog, is much diminiſhed by the application of opium. In 20 minutes after the death of this frog I could not produce any; now they may be induced at the expiration of 48, 72 and even a greater number of hours, in a frog killed by cutting off, or cruſhing the head; as the experiments of Dr. Fowler on animal electricity evince.

EXPERIMENT 34.

Auguſt 22d. At 10 minutes after 3 o'clock, I expoſed to view the *brain* of a frog, and put a few drops of the aqueous ſolution A. upon it. By a want of attention to the motions of the frog, the greateſt portion of it was ſpeedily loſt. In 5 minutes he was very lively. In 10 minutes the ſame. At 30 minutes after 3, I introduced a ſecond portion with greater care, which almoſt inſtantly ſeemed to affect him; for inſtead of ſupporting himſelf as uſual upon his legs, he lay upon his abdomen. In 5 minutes his *left* leg ſeemed paralized, and he tumbled about with a ſort of convulſive motion. In 10 minutes he was more affected. A pointed inſtrument ſcarcely cauſing him to move; and his motions were chiefly confined to his *upper* extremities.

In 20 minutes he appeared to be quite dead. On opening the thorax I found the heart pulſating vigorouſly 56 times in a minute. In 15 minutes from this time it beat 48. In this frog, the contractions produced by zinc and ſilver were by no means ſo vigorous, as in thoſe killed without the application of opium.

EXPERIMENT 35.

By way of a comparative experiment, on the 26th of Auguſt, I injected ſome drops of the aqueous ſolution of *common*

mon opium, B. (experiment 4.) beneath the ſkin of the *right* inferior extremity of a lively frog, at 15 minutes before 1 o'clock. At 1, he was quite lively. By means of a probe, I now detached the ligamentary union of the ſkin at the knee, and paſſed a ſecond portion of the ſolution down to the ankle joint. In 20 minutes he was as lively as ever. I now injected a third portion and retained it ſome time. A *prolapſus ani* occurred during the introduction of the ſolution by the ſole exertion of the animal, as no force was employed. In 5 minutes his leg began to drag, and in 10 minutes he could not move it. The *left* was uſed with violence when irritated.

At 20 minutes before 2, I injected a portion into the ſtomach, which almoſt inſtantly convulſed him, in a manner reſembling the contractions produced by zinc and ſilver. His *irritability* was ſo highly increaſed from the effects of the opium, that the ſlighteſt touch produced convulſions in all his extremities. After ſome minutes more had elapſed, a ſudden noiſe or even blowing upon him, would produce them, and they became more frequent by degrees. At 2 o'clock they were leſs conſiderable, and at 10 minutes paſt 2, he appeared dead, as irritation produced no contractions.

On opening the thorax, the heart beat 48 vigorous pulſations in a minute. The ſtomach was filled with a ſlimy maſs, poſſeſſing the ſmell of opium, and it appeared to have contracted upon itſelf about the middle. The veſſels on its ſurface were diſtended with blood. Contractions induced by zinc and ſilver, were very inconſiderable; being confined chiefly to the toes of the *right* leg, even when the ſilver was placed in contact with the large ſciatic nerves; and in the *left* leg, ſimilar contractions extended no farther than the foot. At 3 o'clock *neither* would contract. The *auricle* was ſtill pulſating 27 times in a minute.

Having ſhewn by the preceding experiments that there exiſts a great ſimilarity between the effects of the aqueous ſolutions

ſolutions of *common*, and of the *lettuce* opium; I next proceeded to ſome few experiments with the *ſpirituous tinctures*, A. and B. deſcribed in experiments 7 and 8.

EXPERIMENT 36.

September 2d. At one o'clock, P. M. I injected between the ſkin and muſcles of the *right* inferior extremity of a lively frog, a few drops of the ſpirituous tincture of *opium lactucæ*, A. (experiment 7,) and at the ſame time, I introduced beneath the ſkin of the *left* leg ſome of the ſpirituous tincture of *common opium*, B. (experiment 8.) At the moment of introduction both tinctures gave pain, and by the efforts which the frog made to eſcape, a conſiderable portion of the tinctures was loſt. In 5 minutes he hopped with great difficulty; or rather, his motions ſeemed to be performed by quickly and repeatedly puſhing himſelf on by his lower extremities. Conſiderable inflammation was ſpeedily induced in both thighs, and blood was even effuſed.

In 10 minutes his motions were more difficult; and his jumps more circumſcribed. Both legs ſeemed equally affected.

A drop of the tincture put upon his eye appeared to give pain, as he immediately cloſed it.

In 20 minutes I injected a few more drops below the ſkin of the inferior extremities. In a few minutes, both legs appeared immoveable. In 5 minutes from this time, the inflammation was ſeen extending itſelf with conſiderable ſpeed, down the legs; as many ſmall veſſels before inviſible to the naked eye, were now diſtended with red blood. His legs lay motionleſs in any poſition they were placed; and no irritation, except that produced by zinc and ſilver, cauſed them to move. Theſe metals cauſed ſtrong convulſions in both.

For

For nearly 20 minutes he ſeemed to be dead. After which a very ſlight touch convulſed him; and by this time the inflammation had extended to his toes.

Some buſineſs carried me away at this period. I did not return till nearly 3 o'clock, when I found the frog laying as I left him; but the irritability of his ſyſtem was ſo highly increaſed, that a very ſlight touch cauſed ſtrong convulſive motions. I now placed him in a tumbler of water, and at 20 minutes after 3, the merely making a noiſe, as in the motion of a chair along the floor, and even only touching the glaſs in which I had placed him, cauſed ſuch ſtrong convulſions, as nearly to project him from it. By degrees this effect ceaſed; and by 4 o'clock he was quite dead.

On opening the thorax the heart was beating 45 moderate pulſations in a minute. The *right* leg exhibited ſtronger marks of inflammation than the *left*; as the veſſels were more turgid, and one or two conſiderable effuſions had taken place into the ſubſtance of the muſcles,

EXPERIMENT 37.

Fifteen minutes before two o'clock, I introduced a few drops of the ſpirituous tincture, A. (experiment 7.) into the ſtomach of a fine lively bulfrog. A violent and inſtantaneous projection of the tongue ſhewed a diſpoſition to vomit it up. He jumped about under the receiver with great vigor. In 10 minutes he began to breathe more quickly, and his jumps appeared more languid. When placed upon the ground, he could not jump above twice or thrice his length. About 3 o'clock he appeared to have recovered conſiderably from the effects of the tincture.

At 10 minutes paſt 3, I injected a few drops below the ſkin of the *right* lower leg, which cauſed conſiderable pain. A portion of it was loſt, and a ſlight effuſion of blood took place, which probably waſhed away another portion of the

tincture

tincture. At 20 minutes paſt 3, he moved with conſiderable agility, and ſeemed very briſk. In 30 minutes he appeared quite well. I now injected a ſecond portion under the ſkin of the leg. At 35 minutes after 3, he moved it very briſkly. At 40 minutes after, the *right* leg began to drag, although he could draw it to his body; and he lay with his head upon the table, inſtead of ſupporting himſelf upon his legs as uſual. I now put him into ſome water, which revived him conſiderably; ſo that at 4 o'clock he moved his legs with eaſe, and by 20 minutes after, he uſed them vigorouſly; though ſtill he was unable to leap to any diſtance.

At 20 minutes before 5, I introduced ſome more of the tincture into the ſtomach. In 10 minutes, he appeared very languid. Five minutes before 5, his legs remained motionleſs in any poſition in which they were placed, and were inſenſible to irritation. At length he gradually began to mend, and at 6 o'clock could move his limbs with great eaſe. I now put him into the water, and the next day found him quite lively.

EXPERIMENT 38.

September 5th. At one o'clock, P. M. I laid bare the brain of the frog, the ſubject of the preceding experiment. He had perfectly recovered from the effects of that experiment, and was extremely lively. I injected ſome drops of the ſpirituous tincture A. down the ſpinal canal, which ſeemed inſtantly to affect him, as his fore legs were conſiderably paralized. He appeared ſomewhat recovered in 10 minutes, but breathed quick. The greateſt part of the tincture was diſcharged and waſhed away by ſome blood which oozed from the wound. At 15 minutes after one, with more care, I introduced a ſecond portion, which paſſed to all appearance, lower than the firſt. In a moment the *whole* muſcular

muſcular fabric, became motionleſs and relaxed. The eyes cloſed; reſpiration ceaſed; and a ſlight pulſation of the heart, evinced by the motion of the thorax, alone rendered it probable that any vitality remained. No contractions of the extremities followed the application of a pointed inſtrument; but zinc and ſilver cauſed ſtrong convulſive motions of the limbs. If the brain was touched with a ſilver probe, and brought into contact with the zinc on which the frog was placed, ſtrong contractions of the body and limbs ſucceeded. When the probe was introduced to ſome diſtance down the ſpinal canal, the frog moved. At 25 minutes paſt one, he opened his eyes; and drew up ſoon after, his extended *lower* extremities to his body. At times the muſcles of his *upper* limbs appeared ſtrongly contracted, and they generally remained in the ſame poſition unleſs irritated.

At half paſt one, he ſuddenly became moſt violently convulſed; writhing his body and limbs, in every poſſible direction; and he even threw himſelf with conſiderable force from the table on which he was placed, although *at leaſt ſix inches* from its edge. During the period of theſe violent convulſions he uttered a croaking noiſe. The convulſions were induced by the ſlighteſt noiſe, extending even to the toes; and they were more evident in proportion to the *ſuddenneſs* of the cauſe producing them. In 5 minutes this effect diminiſhed conſiderably, and his limbs when extended were ſlowly drawn up again to the body.

At 20 minutes before 2, I left him feebly ſupporting himſelf upon his legs; and did not return till about 5 minutes before 3, when I found him under the receiver, and lying upon his back, as if from a renewal of the preceding convulſions. His eyes were open, and he moved ſlightly when touched. A probe paſſed down the ſpine cauſed his extremities to move. In 10 minutes from this time, very little effect was produced by paſſing the probe down the ſpina canal; and in a minute or two, he appeared completely dead.

Convulſions produced by zinc and ſilver were ſtill ſtrong. The heart on expoſing it to view was pulſating moderately 42 times in a minute.

Experiment 39.

At 30 minutes paſt 4, I injected a few drops of the above tincture A. into the ſtomach of a young frog; which cauſed it to *gag*, and a conſiderable portion was rejected. In 5 minutes he remained under the receiver pretty quiet; though before this he had been ſtriving violently to eſcape. When touched he did not jump, but lay in the poſition in which he was placed. Ten minutes before 5 o'clock, his reſpiration was quick, being 66 times in a minute. His hind legs were moved with difficulty, and he lay with his head upon the table.

At 5 minutes paſt 5 he began to move about, and ſeemed to have recovered conſiderably. Buſineſs now calling me away, I put him into the water. At 10 o'clock he was very active and vigorous, and continued ſo till I threw him out ſome days after.

The following very intereſting experiments, were made at my requeſt at the Pennſylvania Hoſpital by my very ingenious and worthy friend Dr. Samuel Cooper, to whoſe kindneſs I am much indebted.

" Jeremiah Smith, 34 years old; pulſe beating 96 ſtrokes in a minute, took 30 drops of the lettuce laudanum, (A. experiment 7.)

In min.	2	5	10	15	20	25	30	35	40	45	55	60	70	80
Pulſe beat	96	96	94	95	98	98	100	101	100	101	101	103	102	102

His face was now evidently fluſhed, and his ſkin was warmer. He ſaid that he felt very agreeable. His pulſe ſeemed increaſed in force as well as frequency."

" Upon

" Upon taking 30 drops of the ſame preparation, I felt as if I had ſwallowed a glaſs or two of wine, or a ſmall quantity of opium.

" It was given in the following diſeaſes, viz. heart-burn, chronic rheumatiſm; the pain of which occurred in the night; Diarrhœa; and in a pectoral complaint attended with a periodical cough. It ſeemed to deſtroy the diſagreeable ſenſation of heart-burn, and hindered the occurrence of the pain of rheumatiſm. It checked the frequent ſtools accompanying diarrhœa, and occaſioned the evacuation of much flatus. It allayed the cough attending the pectoral complaint. In all theſe caſes it ſeemed to be preciſely analogous in its operation to the tincture of opium; and like opium it increaſes the frequency and energy of the pulſe."

If any perſon reads the foregoing experiments with attention, he cannot heſitate in allowing the moſt perfect identity to the two ſpecies of opium. The experiments of *Whytt*, of *Alſton*, and of others, ſtrengthen in the higheſt degree the evidence of the fact.

The milky juice from which the opium is prepared, exiſts in the ſtalk and in the leaves of the plant. It is not indiſcriminately depoſited throughout, but is placed in appropriate veſſels running longitudinally in the woody or fibrous part of the ſtalk. The internal or medullary part of the plant is ſoft; and perfectly bland to the taſte, abounding in a tranſparent mucilaginous juice; which has not the ſmalleſt analogy to the above-mentioned one.

The beſt time for collecting the juice, is when the plants are beginning to ſeed. If we take it before this, it has not ſufficiently acquired its medical properties; and if at a later period, the quantity is by no means ſo conſiderable.

It is beſt procured in the manner deſcribed for collecting it from the poppy, viz. by inciſions; with this difference, that in the poppy they are *longitudinal*, but in this muſt be circular. A very moderate depth ſuffices. It exudes freely in milky drops, which may be either immediately collected; or ſuffered to dry on the ſtalk, and then ſcraped off and depoſited in proper veſſels. If we obtain it by preſſure from the plant, and then inſpiſſate; the other juices ſeem to alter it conſiderably: the colouring matter of the vegetable is taken up, and the ſmell of the opium no longer exiſts; at leaſt this was the caſe with 30 grains of an extract procured thus, from 10 drachms of the plant, by Dr. Cooper. It poſſeſſed none of the peculiar ſmell or taſte of opium, and when I put it into a mixture of equal parts of alkohol and water, it readily yielded the green colouring principle, but nothing further. Probably more attention to the ſubject will lead to a method of ſeparating the opium from the other principles united to it. Expoſure to the ſun and air, may poſſibly produce this effect: the ſmell of the juice when firſt extracted by preſſure is ſtrong of opium. The extract above alluded to was inſpiſſated in a ſand bath, the heat of which may have been too conſiderable for it.

Having ſaid thus much upon the juice of the common lettuce, I muſt obſerve that all the ſpecies contain it in a larger or ſmaller proportion. The *lactuca ſylveſtris*, or *viroſa* of Linnæus, contains it moſt abundantly. That from which I obtained my opium, was, I obſerved before, the *lactuca ſativa*; it abounds in juice, and will ſerve the double purpoſe of cultivating for the table as well as for the ſhop.

I cannot avoid contraſting the ſuperior advantages of the opium extracted from the *lettuce*, above that procured from the poppy.

Some judgment may be formed of the labour and expence attendant upon the cultivation of one acre of the poppy, by the account given by Mr. Kerr. He ſays "an acre yields in the Eaſt Indies, 60 lbs. of opium, which, at 9 ſhillings ſterling, (2 dollars) per pound, is £.27 an acre." Now,

at

at a moderate computation, it may be presumed that *one half* of this sum is employed in the necessary expences of ploughing, manuring, sowing, watering, and collecting, &c. &c. Say then that £.13. 10, are clear gain, (which must be allowed to be a large proportion.) Now the *poppy* cannot be employed as an article of diet; whereas the *lettuce*, which grows here in the most luxuriant manner, will amply repay the labour and expence (which at most is trifling) attending its cultivation, by the sale of the supernumerary plants taken up at an early period for diet, long before the developement of the opium principle. Here then the very labour employed has the double advantage of thinning the plants, thereby rendering the remainder more perfect; whilst it collects for the market such as have arrived to sufficient maturity for the table.

The sale of these supernumerary plants would, I conceive, *at least* repay the labour, &c. attending their cultivation: and if the rest yielded per acre *only* 60 lbs. of opium, double the profit would arise from its cultivation, above that of the poppy. The great abundance of the juice however, and the luxuriance of the plant, render it highly probable, that *double* that quantity, *if not more,* might be procured from the acre of ground.

The price of this valuable article of the Materia Medica, leads me to hope that farmers and others will attend to the cultivation of the lettuce, in order to obviate one source of the annual expenditure of money from the United States; and as Dr. *Crumpe* observes in his valuable treatise upon opium, "If any overplus remained after our own demands, a ready market would be found for it in the East Indies, where its consumption is very considerable, and price generally high."

The medical virtues of opium would appear from the experiments of authors, to reside more particularly in the *extractive* principle. If this be certainly the case, the *opium* of of the *lettuce*, would prove far more valuable to the Materia Medica, than the *common* opium: for by the comparative experiments 3d and 4th, *ten* grains of *extractive* matter were taken up from one *ſcruple* of the *lettuce* opium; whilſt only nine grains of the common opium were taken up from the ſame quantity. This in a pound weight, will give a very decided advantage of *ſix* drachms, *one* ſcruple, *four* grains, to the *lettuce* opium, above the common; for in one lb. of the lettuce opium, *one half*, or 8 oz. are extractive matter, whilſt of the *common* opium only 7 oz. 1 ſc. 1 dr. 16 grs. are extract.

Common Opium.

If 20 grs. : 9 :: 7680 grs. the No. in 1 lb.

9

2|0 | 6912|0

6|0 | 345|6

8 | 57 · 36

oz. 7. 1. 1. 16.

As I conceive the foregoing facts may prove ſerviceable to my fellow citizens, I have taken the liberty of drawing them up in the form of a paper, addreſſed to your reſpectable Body, as the ſureſt mode of obtaining their promulgation.

I have the honor to be,

With the greateſt reſpect,

Your obedient humble ſervant,

JOHN REDMAN COXE.

No. LXV.

Experiments and obſervations, on the atmoſphere of marſhes: *By* ADAM SEYBERT, M. D.

Read, Dec. 21ſt, 1798.

WHEN inquiries which have attracted the attention of a Franklin, a Prieſtly, an Ingenhouz and many other eminent perſons, without being decided, are undertaken by one whoſe abilities are ſo far inferior as mine, little ſucceſs can be expected. This remark announces the difficulty of the ſubject I am about to inveſtigate; nevertheleſs I am ſtimulated by the induſtry of my predeceſſors, and if I cannot promiſe much new matter, I hope to be at leaſt able to verify ſome obſervations and perhaps diſprove others; for in proportion as we remove errors we approach nearer to truth.

When we are fully perſuaded, that to live and to breathe are ſynonimous terms; and that the abſolute neceſſity of air to the maintenance of animal life has been fully eſtabliſhed by repeated and well concerted experiments, we need not be ſurpriſed to find many perſons engaged in an examination of the chemical qualities of our atmoſphere: the names of Scheele, Prieſtly, Lavoiſier, Fontana, &c. will for ever make this branch of ſcience reſpectable.

From the earlieſt ages it has been ſuppoſed that the atmoſphere has great influence on the human body in producing diſeaſe, as well as in reſtoring health; hence the accounts of Hippocrates, Sydenham and Huxham. Phyſicians ought always to notice the ſtate of the atmoſphere during the prevalence of epidemic diſeaſes.

Before facts were collected and experiments well performed, the atmoſphere was ſuſpected to differ materially in almoſt every ſituation; but latter experiments have proved that our notions have been erroneous to a great degree.

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In a former memoir which I had the honour to read before this ſociety, I paid particular attention to the atmoſphere over the ocean, rivers and neighbouring land, and hope that my experiments have been of ſome ſervice towards the eſtabliſhment of truth; in the preſent eſſay I intend giving an account of ſome experiments which I performed at different times on the air over marſhes.

A few general remarks reſpecting the common ſtate of our atmoſphere, perhaps become neceſſary for the ſake of future compariſons.

We no longer believe, for experiments have taught us the contrary, that our atmoſphere is an homogeneous element: the preſent ingenious doctrines of heat have thrown much light upon the ſubject; and with much reaſon ſome philoſophers are induced to believe "that the aeriform ſtate is a modification of bodies, dependent on the degree of temperature, and on the preſſure which theſe bodies undergo!"* This opinion has been extended ſo far as to induce ſome to ſay, "Perhaps alſo metals are contained in the atmoſphere."† Theſe ſentiments do not appear to be merely conjectural, for Chaptal has precipitated mercury from oxygen gas (which was obtained from red precipitate) by means of ice; and the family of Achard, ſuffered ptyaliſm from breathing in an atmoſphere where mercury had been expoſed for ſome time in a ſaucer.

The above opinions, if true (and I think them highly probable) prepare us to meet many difficulties in the analyſis of the atmoſphere. But all I expect to do is to open the paſſage, and I ſhall leave others to render it more certain; for numerous experiments, and thoſe often repeated, are the only means whereby we can aſcertain truth; and I fear the labours of one man are inſufficient to perform this taſk.

I am

* Lavoiſier's Elements of Chemiſtry, p. 59.

† Gritauner's Antiphlogiſtiche Chemie, p. 58.

I am not without hopes that others will engage in the inquiry, it is of importance to every citizen, more eſpecially ſince we find that our principal cities are almoſt yearly afflicted with a terrible epidemic, which has been by ſome attributed to the ſtate of the air. Future diſcoveries it is hoped will multiply the number of teſts for airs, and thus render the ſubject more eaſy.

Reſpectable chemiſts have determined the component parts of our common atmoſphere to be

Oxygen gas 27.
Azotic gas 72.
Carbonic acid gas 01.

Any deviation from this ſtatement muſt be attributed to local circumſtances.

I ſhall firſt endeavour to determine, whether or not the air of marſhes differs from that of other ſituations:

2. What are the cauſes of the differences which are found to exiſt: and

3. Make a few obſervations and remarks.

1. March 31ſt, 1798. Air was obtained by agitating ſtagnant water over marſhy grounds; the following experiments were then performed.

a. It burned when a candle was applied to it; the flame was blue: it did not explode when mixed with atmoſpheric air.

b. Agitated with lime water, a copious white precipitate was formed.

c. Its bulk was conſiderably diminiſhed by agitating it with lime water.

d. Equal bulks of it and nitrous gas were introduced into my eudiometer tube, and a diminution of $\frac{2}{100}$ of their bulk was perceptible.

Theſe experiments were frequently repeated and the reſults were ſimilar to the above-mentioned: they warrant the following inferences:

a. That carbonic acid gas enters largely into the compofition of the air examined.

b. That hydrogen gas is an ingredient in it.

c. That no oxygen gas was prefent: for the fmall abforption which took place, I attribute to the action of the water with which the airs were agitated.

The above experiments were performed on the air, which was obtained immediately as it was difengaged from the marfhy foil; it became neceffary to examine the air fituated at fome diftance above the marfh.

At different times during the fummer of 1798, I collected air from above marfhy grounds: the following experiments were performed on it.

a. When agitated with lime water, it afforded a precipitate, which was not fo abundant as in the former experiments.

b. Mixed with nitrous air, its bulk was diminifhed to almoft as great a degree as the air in the yard of my lodgings.

c. Either pure or mixed with atmofpheric air, it did not burn or explode when a candle was applied to it.

Hence it appears that the air obtained at the height of feveral feet above marfhes,

1. Contains little or no hydrogen gas.

2. That the proportion of carbonic acid gas is pretty confiderable; and

3. What is of great confequence to be obferved, a large quantity of oxygen gas enters into its compofition.

The laft mentioned facts induce us to believe that the air *above* marfhes is not confiderably different in its properties from the common atmofphere in other fituations, where animals refpire with eafe and enjoy perfect health, except the proportion of carbonic acid gas being greater; and this I am induced to believe diminifhes in quantity as we afcend: for facts related by travellers who have vifited the Grotto del

del Cane and other ſimilar places, prove that the gravity of this elaſtic fluid permits it to riſe but to an inconſiderable height.

After having proved that certain qualities do exiſt in the air over marſhes, which are different from thoſe poſſeſſed by the atmoſphere in other ſituations, we muſt next attend to our

Second object, viz. to aſcertain what are the circumſtances about marſhes which produce ſuch effects?

Before we proceed any further, it is of the greateſt importance to be ſatisfied reſpecting the changes which may be produced on common atmoſpheric air, when ſubjected to the action of the ſoil of marſhes.

At different times in the months of September and the commencement of October 1796, I expoſed atmoſpheric air to the action of mud, which I obtained from marſhes below the city. The ſame was done at different times in the months of April and July, 1798. The experiments were expoſed to the temperature of the atmoſphere. The reſults from the different experiments were ſimilar. The air was expoſed to the action of the mud which was contained in a tumbler, by means of an inverted glaſs jar, in a baſon containing a ſmall quantity of water. The following changes were noticed.

1. The air contained in the jar became much diminiſhed in bulk, as was proved by the water riſing into the jar.

2. The air, thus acted upon, when agitated with lime water, afforded a copious white precipitate and became diminiſhed in bulk.

3. In ſome of the jars, were ſuſpended papers, ſtained blue with litmus and yellow with turmeric, the blue received a reddiſh tinge and the yellow remained unaltered; the red was again changed to a blue by expoſure to the vapour iſſuing from a bottle containing ammoniac.

4. The air thus altered by the mud, when mixed with nitrous gas in the eudiometer tube, was in every inſtance

found to have loſt in point of purity; ſometimes no diminution of bulk whatever took place.

The following circumſtances ſeemed to influence the laſt mentioned experiments. 1ſt. Temperature. 2d. The length of time during which they were continued. And 3d. The proportion which the mud and air bore to each other, the ſurface of the mud being more or leſs extenſive, ſeemed alſo to have its effects.

The air thus affected by the action of the mud would in no inſtance burn or explode, when a candle was applied to it; hence it contained but a ſmall quantity of hydrogen gas.

This laſt mentioned fact induced me to engage in an eſſay to determine the origin of the hydrogen gas which abounds in the *air obtained by agitating ſtagnant waters.*

It is neceſſary to be obſerved, that in the above experiments with mud, but a ſmall proportion of water was added to it in the tumbler, the quantity was juſt ſufficient to promote putrefaction. I am of opinion that the hydrogen gas is afforded by a decompoſition of the ſtagnant water, effected by the putrefaction of the dead animal and vegetable ſubſtances, which enter largely into the compoſition of the ſoil of marſhes. I was induced to form this opinion, becauſe, firſt, pure water is a compound of but two elements, conſequently the affinity cannot be broken but by the action of a third ſubſtance. And ſecondly, we have no experiments which prove that pure water has undergone ſpontaneous decompoſition. My ideas are confirmed by a fact well known to all ſeamen, viz. when a candle is applied to the bung hole of a caſk containing *river water*, which had been for ſome time cloſely ſtopped, an elaſtic fluid eſcapes, which will inflame and appears in all reſpects ſimilar to hydrogen gas obtained by other means.

After forming the above conjectures, I determined to perform a few experiments which might tend to confirm or diſprove my opinion. With this view mud and water, with

with a very ſmall portion of atmoſpheric air, were at different times confined in bottles cloſely ſtopped and inverted over water: in ſome inſtances the experiment was continued during 20 and 30 days. They were ſubjected to the temperature of the atmoſphere. During the progreſs of the experiments, I perceived that an elaſtic fluid was diſengaged from the materials contained in the bottles, and that the water was evidently diminiſhed in bulk; the elaſtic fluid generated during theſe experiments, 1ſt, inſtantly formed a copious white precipitate when agitated with lime water; 2dly, it burned, when the flame of a candle was applied to it, and poſſeſſed the other properties, which are common to air obtained by agitating ſtagnant waters over marſhes.

Theſe facts are deciſive to me on the ſubject, and confirm the above conjectures reſpecting the origin of the hydrogen gas diſengaged from marſhy grounds. It is neceſſary to remark, that ſome danger attends theſe laſt experiments; for a large bottle which was cloſed by a ground ſtopper, was broken on the 25th day of the experiment, by an expanſion of the contained elaſtic fluid: the pieces, which were large, were thrown to the diſtance of 20 feet, and a report was heard louder than that from the firing of a muſket. In general, the bottles had corks faſtened by means of ſtrings bound round them: as ſoon as I cut the ſtrings, the corks were forced from the necks of the bottles with conſiderable violence.

The above experiments teach us that mud vitiates the atmoſphere in a very powerful manner. They alſo enable us to account for the preſence of the elaſtic fluid forming the atmoſphere of marſhes. It appears, that, the carbone of the mud unites with the oxygen of the decompoſed water, and forms the carbonic acid gas, whilſt the hydrogen gas is ſet at liberty. Theſe are truths not to be invalidated by gratuitous aſſertions, ſince their baſis is experiment.

It

It may be aſked, if mud ſeizes oxygen gas with the avidity ſtated, how comes it that eudiometrical experiments prove the air over marſhes to be nearly, if not quite, of the ſame degree of purity as that of other ſituations?

At firſt an anſwer to this important queſtion may ſeem difficult; but ſome examination of the circumſtances attending the ſituation of marſhes, enables us to account for it in a very ſatisfactory manner. It is to be remarked that in my trials with mud, the air was confined under glaſs veſſels over water, conſequently no circumſtances from without could have any influence on the experiments. The air over marſhy ſituations is very different, it poſſeſſes all the advantages of ventilation, &c. in common with the atmoſphere. Beſides theſe circumſtances, a large quantity of oxygen gas is afforded by the living vegetables which ſurround them in abundance. We may alſo obſerve, that frequently large ponds of water are found in their neighbourhood, and that often rivers are at no great diſtance from them: may not therefore a quantity of oxygen gas be diſengaged from theſe waters by the action of the ſun? Experiments are related by reputable authors, wherein water has been decompoſed by the action of the ſun's rays; of this more hereafter.

That the atmoſphere of marſhes, therefore, differs in certain circumſtances from that of other ſituations, and that the ſoil has conſiderable effect, in altering the air of the atmoſphere, I think, cannot be doubted. Let us therefore endeavour to diſcover the particular local cauſes which give riſe to theſe variations.

I have before hinted that the putrefaction of the animal and vegetable matters upon the ſoil of marſhes, was the great cauſe of the changes obſerved to exiſt: for every ſpecies of ſoil will not operate in the manner alluded to.

That the cauſe is in the putrefaction of theſe matters, and that this ſtate is abſolutely neceſſary to thoſe changes, I infer

infer from the following circumſtance; marſhes have no noxious influence, during the winter ſeaſon. They cauſe diſeaſe when the circumſtances are preſent which promote putrefaction; as, a proper degree of heat, a due quantity of moiſture and the contact of atmoſpheric air or ſubſtances capable of affording oxygen; as *water*. That a certain degree of moiſture is neceſſary, appears evident from White's experiments, related in the Philoſophical Tranſactions: he ſays, "a certain degree of moiſture ſeems neceſſary to produce the bad effects of marſhes; for mud when perfectly dry did not alter the air." He might have added, that too much fluidity will likewiſe prevent their bad conſequences, which is proved by the neighbourhood being healthy when they are overflowed. An overflow of water may operate by preventing the powerful effects of the ſun. Experience teaches us, that their bad effects are diſcontinued, when they become dry. Covering them with clay and other ſubſtances not liable to putrefaction, deſtroys their bad effects, ſo does cultivation, froſt, &c.

Living trees being planted in their neighbourhood renders the ſituation more healthy, by abſorbing the gas exhaled during putrefaction and affording oxygen gas.

White's experiments prove, "1ſt. During ſixteen hours, air confined in a phial over water did not ſuffer a change. 2dly. Pure clay moiſtened did not alter the purity of the air. 3dly. Sand moiſtened did not change the purity of the air." But 4th. *Mud (which conſiſts of earths intimately mixed with dead animal and vegetable ſubſtances) rendered the air very impure, as I proved by the experiments which I performed.*

The following reflections occurred to me ſome time ſince, and are copied from my note book.

To arrive at any certain knowledge reſpecting the manner by which marſhes can be ſuppoſed to affect the atmoſphere, we muſt inveſtigate their compoſition.

They

They ſeem to conſiſt of;

1ſt. More or leſs water. - 2dly. Different proportions of *dead* animal and vegetable matters. And 3dly. The earthy ſubſtances compoſing the original ſoil.

Animals and vegetables, when they have ſuffered death, are ſubject to the laws which govern inanimate matters in general, and they are liable to the various changes produced by chemical mixture and the laws of chemical affinity: they are acted upon by the powerful agents of nature, and thus ſuffer decompoſition and form new combinations.

All chemiſts acknowledge the analyſis of animal and vegetable ſubſtances to be imperfect. Lavoiſier has paid particular attention to the ſubject. He performed numerous and accurate experiments to determine their compoſition, and notices in a particular manner the reſults they afford during their putrefaction. According to him, they conſiſt chiefly of hydrogen and oxygen, combined with carbone: theſe ſubſtances, he ſays, are found in all vegetables, and none exiſt without them. Animal ſubſtances contain more hydrogen and azote than vegetables do, they alſo have carbone as a conſtituent part of their compoſition: ſome of both claſſes contain ſulphur and phoſphorus.

The above are the principles which I ſuppoſe are liable to be acted upon, and thus produce the effects we are about to conſider.

Before we can underſtand the changes to which the above ſubſtances are liable, we muſt take into conſideration, that our atmoſphere is compoſed of the azotic and oxygen gaſes, and a ſmall portion of carbonic acid gas: many view this laſt as adventitious and by no means neceſſary.

Heat, moiſture, the contact of atmoſpheric air and reſt we know are circumſtances attendant on marſhy ſituations during the unhealthy ſeaſons.

A priori, we might be induced to believe that the following phenomena would take place, under the above circumſtances.

1. That

1. That hydrogen gas would be disengaged. 2. That the oxygen combining with the carbone would form the carbonic acid gas. 3. That azote would unite with a portion of hydrogen and thus produce ammoniac; whilst another portion of it would, during its combination with oxygen, form the nitric acid. And 4th. That when sulphur or phosphorus were present, they with hydrogen would form the sulphurated and phosphorated hydrogen gases.

We shall now endeavour to discover whether or not these elastic fluids enter into the composition of the atmosphere of marshes.

1. Hydrogen gas. Doctor Franklin has long since demonstrated the production of this elastic fluid in marshy situations. Ingenhousz and others have confirmed the truth of his experiments and observations.

My experiments convince me that it is produced in a considerable quantity, and that it may be easily procured by agitating stagnant waters over marshes. It is also evident that this gas is in a state of mixture with the carbonic acid gas.

Although we are certain that a large quantity of hydrogen gas is disengaged from marshy grounds, we must nevertheless conclude that it bears but an inconsiderable proportion to the atmosphere at large; for we find that the air immediately above marshes will not explode upon the approach of a candle: indeed from its levity we might suppose that it occupies the inferior strata of the atmosphere but for a short time.

2. Carbonic acid gas. That this elastic fluid enters largely into the composition of the atmosphere of marshes, is easily proved by agitating it with lime water.

3. Ammoniacal gas. The production of this gas during putrefaction, is proved beyond doubt; therefore that it should exist in the atmosphere of marshes seems at least probable, indeed many have inferred considerable effects

from its presence, but as they did not detect it by any test with which we are acquainted, their opinion is entirely hypothetical.

The following are the results of the means I employed to discover whether ammoniacal gas is present in the atmosphere of marshes. 1. No white clouds appeared, when muriatic acid gas was mixed with air obtained by agitating stagnant waters. 2. Slips of paper stained yellow by turmeric, were suspended in a bottle containing mud and atmospheric air, it remained unchanged; whereas those stained with litmus received a reddish tinge. 3. I never could perceive the odour peculiar to this alkali, when I visited marshes.

The above experiments caused me to doubt the presence of this elastic fluid in the atmosphere of marshes. I was confirmed in this opinion by the following circumstances: 1st. Ammoniac combines readily with water: it is impossible to procure ammoniacal gas over water; therefore we are to suppose that if this fluid is produced it is immediately absorbed by the water of the marsh. 2dly. Carbonic acid gas is abundant in the atmosphere of marshes. By experiment, I ascertained that this acid and ammoniacal gas were very prone to unite and form the carbonate of ammoniac. The experiment was performed in a glass tube over mercury: as soon as the two elastic fluids came in contact, an absorption took place and the bulk of them was considerably diminished: at the same time the sides of the tube were incrusted with a white matter, which possessed all the properties of the carbonate of ammoniac. If such are the phenomena of these experiments, why will not similar effects take place in marshy situations?

4. Nitric acid. The experiments and observations of Thouvenel and others, have long since demonstrated the production of this acid during putrefaction. If it is formed in marshy situations, its presence cannot be proved in their atmosphere, and I am inclined to believe that it is immediately absorbed by the neighbouring waters.

5. Sulphurated

5. Sulphurated and phoſphorated hydrogen gaſes. If theſe elaſtic fluids conſiſt of hydrogen gas, holding ſulphur and phoſphorus in ſolution, it ſeems probable that they ſhould be generated during the putrefaction of ſuch matters as contain them as conſtituent elements. Although Chaptal in his Memoirs de Chimie, p. 141, obſerves: "Que la boue noire, degagèe de tout vègètal, ne donnoit plus d'air inflammable mais répandoit une odeur de foie de ſoufre." Still he relates no experiment whereby he detected its preſence in the atmoſphere of marſhes. Its ready abſorption by water; marſh air when agitated with a ſolution of the acetite of lead producing no change in it; ſilver not tarniſhing ſooner in theſe than in other moiſt ſituations; and the air poſſeſſing no peculiar ſmell, are all facts which tend to convince me that it does not exiſt; moreover, Kirwan ſays, that hepatic gas united with nitrous air will depoſit ſulphur. I agitated marſh air and nitrous air together in a glaſs tube and no ſuch phenomenon was noticed.

6. Azotic gas. If you burn candles in the air of marſhes, until all the oxygen be abſorbed, and then agitate the remaining air with lime water ſo as to abſorb the carbonic acid, an elaſtic fluid ſtill remains which poſſeſſes the properties of azotic gas.

7th and laſtly. Oxygen gas. A variety of facts prove that oxygen gas is a principal ingredient in the atmoſphere of marſhes; 1ſt, candles burn therein with the ſame luſtre as in other ſituations. 2. Animals breathe with equal eaſe as in other places. 3. Eudiometrical experiments prove that it forms as great a proportion here as in other atmoſpheres which are reckoned more healthy.

Auguſt 4th and 5th, 1796—July 8th and 10th, 1798—I collected air from over marſhy grounds to the ſouth and north of Philadelphia; when tried with the Eudiometer, they always proved as pure as the air in the yard of my lodgings. Chaptal in his Memoirs de Chimie, p. 141. aſſerts

aſſerts that the air over the ponds, which border on the Mediterranean ſea (the neighbourhood of which is equally marſhy if not more ſo than the neck formed by the junction of Schuylkill and the Delaware, as I convinced myſelf during my reſidence at Montpellier in the years 1795 and 1796) was equally pure with that of Montpellier, tried the ſame day. When I aſſert that the atmoſphere of marſhes is equally pure with that of other ſituations, I mean that it contains as large a proportion of oxygen gas as ſuch other atmoſpheres do. I do not by any means intend to be underſtood that it is free from foreign mixtures.

I have acknowledged that putrefaction is going on in marſhy places and likewiſe admit that this proceſs deſtroys the purity of the atmoſphere by abſorbing its oxygen; therefore it may ſeem difficult to admit the *abſolute purity* of the air being equal here to that of other places. People being able to breathe with eaſe over marſhy grounds, is ſufficient proof that the oxygen gas there is adequate to ſupport life. I ſhall now attempt to account for the purity of the air of marſhes as follows. Sennebier has proved by numerous experiments, that living vegetables placed in an atmoſphere of carbonic acid gas or in water ſaturated with this air, expoſed to the action of the ſun, thrive and grow very rapidly: during the experiments the carbonic acid is deſtroyed and oxygen gas is diſengaged. In addition to theſe experiments, Ingenhouſz has taught us that the aquatic plants, particularly ſuch as grow in the neighbourhood of marſhes, poſſeſs the power above ſtated to a ſurpriſing degree; ſee Experiences ſur les Vègètâux, Tom. 2. p. 401. Theſe facts when properly conſidered and connected with the remarks I made when ſpeaking of the effects of mud on the atmoſphere, I think are ſufficient to account for the phenomenon, which at firſt ſeemed at leaſt doubtful.

The above view of this difficult ſubject will perhaps in ſome meaſure alter our opinions reſpecting the utility of

marſhes.

marſhes. Heretofore mankind ſeem to have viewed their exiſtence as noxious to them and unneceſſary to their happineſs. I confeſs my former opinion reſpecting them coincided perfectly with that of the majority, but at preſent my ideas are very different: I conſider them as very neceſſary to keep the atmoſphere in a *proper degree* of purity, for it is not only the impure atmoſphere which kills animals, but the too pure alſo; and an ingenious philoſopher has well obſerved, that animals live too faſt in atmoſpheres overcharged with oxygen gas. They appear to me to have been inſtituted by the Author of Nature in order to operate againſt the powers which vegetables and other cauſes poſſeſs of purifying the atmoſphere, ſo that the oxygen may exiſt in a proper proportion, fit to ſupport animal life and combuſtion. I am of opinion that ere long marſhes will be looked upon by mankind as gifts from Heaven to prolong the life and happineſs of the greateſt portion of the animal kingdom. Perhaps it was originally intended that they ſhould remain uninhabited and that their only uſe ſhould be that of correcting the too pure atmoſpheres. Although their immediate inhabitants ſuffer diſeaſe from them, ſtill but a ſmall portion of the human race chooſe marſhy ſituations for their reſidence.

After I had read the above before the ſociety, a friend in converſation with me, objected to the operation of marſhes on the atmoſphere being intended to prevent a ſuperabundance of oxygen gas; he obſerved that this effect would be fully accompliſhed by the ordinary combuſtion and the reſpiration of animals. Upon reflection, his objections gave riſe to new confirmations of what I aſſerted: I remarked to him, that very extenſive tracts of country were ſufficiently warm without fires; that in theſe places nature gave uncommon powers to vegetable action, but at the ſame time ordained, that, in theſe very ſituations marſhes ſhould be moſt abundant. If we view moſt ſouthern

ern countries, I believe the above facts will be found to exist very generally. A further beautiful demonstration of my proposition may be adduced from a well known fact, that when vegetable life becomes paralized in the winter season the operation of marshes is then unnecessary and is likewise suspended by the same causes, viz. frost, &c.

An account

No. LXVI.

An account of a Kettle for boiling Inflammable Fluids.—In a letter from THOMAS P. SMITH, *to* ROBERT PATTERSON.

Philadelphia, June 14, 1798.

SIR,

Read, June 14, 1798.

WHEN we consider the many unhappy accidents that occur from vessels containing inflammable fluids boiling over and setting fire to the buildings in which manufactories of them are carried on, it must strike us as a matter of importance to form a vessel which should be so constructed as to prevent any of those accidents, and yet of so simple a form as to render it fit for general use. Impressed with these ideas, I take the liberty of offering for your approbation the following plan.

Let A B C D (*see figure*) represent a large kettle, D E, a spout running out to the distance of three or four feet, commencing at D, four or five inches from the brim of the kettle, and the termination of it E, just as high as the brim C. Let the bottom of this spout be covered with wet sponges or rags. Now suppose the kettle to be filled up to D with any fluid, then as soon as it commenced boiling it would rise in the kettle, and in rising but a small perpendicular height, would pass a considerable distance up the spout D E: here the liquor would soon cool and of consequence fall back into the kettle, and the whole subside to its original height. This would occur as often as the fluid rose above D, as the evaporation from the wet sponges or rags, would keep D E constantly cool.

It would perhaps be best to pass the spout through the side of the building into the open air, as thereby the evaporation would be increased, and consequently the spout kept at a lower temperature; in this case it might be covered.

In

In caſe of the fluid to be boiled poſſeſſing a very ſtrong elective attraction to caloric or the matter of heat, the ſpout might be extended to the width of the diameter of the kettle or a projecting ſhelf might be formed all round it, lined below with wet ſponges or rags.

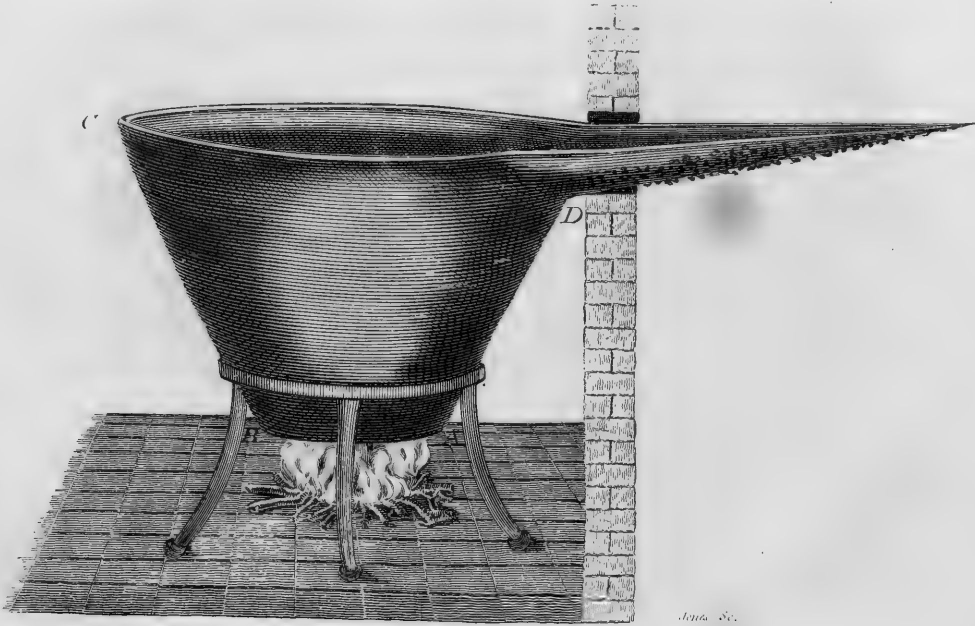
I remain, Dear Sir,

Yours, &c.

THOMAS. P. SMITH.

MR. ROBT. PATTERSON.

P. S. In conformity to the wiſh of the ſociety I procured a veſſel of the form here propoſed. I firſt tried the experiment with water, it boiled very rapidly, but every time the water roſe into the ſpout it immediately ſubſided, although the ſpout had for ſome time been directly expoſed to the heat of one of Lewis's furnaces: I afterwards attempted it with oil, but before the oil boiled the ſoldering of the veſſel, which was made of tin, melted.



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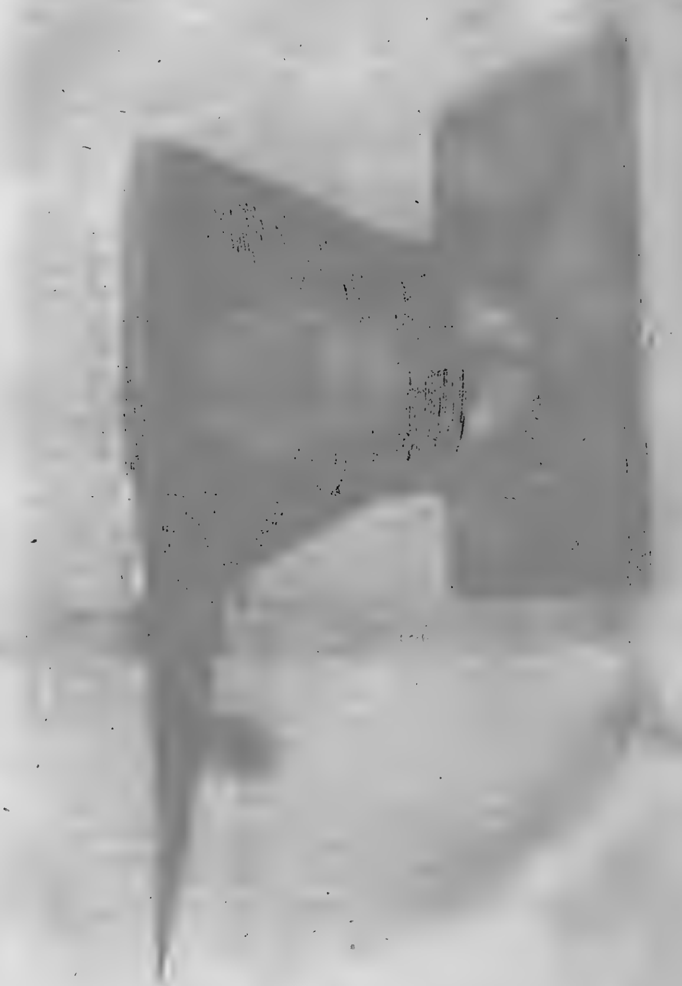


No. LXVII.

An Eſſay on a new Method of treating the Effuſion which collects under the Scull after Fractures of the Head. By J. DEVEZE, *Officer of Health, of the firſt claſs, in the French Armies.*

Read, May 6, 1796. OF the different caſes which require the operation of the trepan, I ſhall only conſider the effuſion between the dura mater and the ſcull, occaſioned by blows and fractures.

Mr. Petit, a celebrated ſurgeon of Paris, has contributed greatly to the improvement of this art, by pointing out the particular ſymptoms which diſtinguiſh effuſions under the ſcull from concuſſion of the brain. Theſe different accidents equally reſult from falls or blows received on the head; and previous to this diſtinction it was eaſy to confound them, a miſtake highly prejudicial to the patient who is affected with concuſſion only, as it requires a different treatment from effuſion and is not relieved by the trepan.

When there is a collection of blood from a blow or fracture of the ſcull, all authors adviſe the trepan, in order to diſcharge the collected fluid; but the difficulty of aſcertaining the part where it has accumulated, often makes frequent repetitions of the operation neceſſary before it is diſcovered. Mr. Mareſchal, firſt ſurgeon to Louis XIV. gives us an example of this, he trepanned a young lady twelve times before he found the effuſion occaſioned by a fracture of the parietal and temporal bones on the ſame ſide. This caſe, and many others of a ſimilar kind too numerous to relate, evidently ſhew how important it is to render an operation more eaſy, which is often repeated without real neceſſity, is painful to the operator, and ſometimes fatal to the patient.

I do not flatter myſelf with having obtained this object; but I think it a duty to communicate to the ſociety ſome ideas which have occurred to me, and by which I have been ſo happy as to ſucceed in a caſe I had an opportunity of attending, in the French hoſpital eſtabliſhed in Philadelphia.

In caſes of accumulated blood between the ſcull and dura mater, the adheſion which unites them, is deſtroyed in the place occupied by the fluid, the collected matter is circumſcribed in a larger or ſmaller ſpace, it expands the dura mater, and forming a tumor that oppreſſes the brain, produces the effects which require the operation of the trepan.

In this caſe a ſingle opening made in the ſcull on one of the points of effuſion, is ſufficient to give vent to the fluid, becauſe the blood preſſed on all ſides by the action of the brain, quits the place it had collected in, and flows towards the part that offers a paſſage. It happens in this caſe, as it does in that where the accumulation exiſts between the dura and pia mater, that the blood, equally preſſed by the brain, runs between thoſe two membranes, flows towards the opening by the trepan, and preſſes the dura mater outward, which indicates to the operator that this membrane requires inciſion, in order to give paſſage to the collected fluid.

It is only in the firſt caſe that the adheſion of the dura mater to the cranium, by retaining the fluid, requires a repetition of the opening, ſhould the operator not meet at firſt with the preciſe ſpot where the fluid is collected.

To avoid theſe inconveniences, I propoſe in ſuch a caſe to deſtroy the adheſion which unites the dura mater to the ſcull, and eſtabliſh a communication between the collected blood and the opening already made by the trepan; by this means a repetition of trepanning would be avoided, and the operation becoming more eaſy might ſave the life of the patient in any caſe not neceſſarily mortal, which

is

is particularly interefting, when the effufion is fituated at the bottom of the fcull.

If inftead of trepanning twelve times, Mr. Marefchal had feparated the dura mater from the cranium, following the direction of the fracture, he would have certainly reached the effufion, and the blood would have been evacuated by the firft opening, although it muft have rifen againft its own weight; this will be eafily underftood by phyfiologifts who advert to the force of preffure the brain exercifes on every part of the fcull, and compare it with the refiftance the collected blood may oppofe by its fpecific weight.

The danger arifing from a feparation of the dura mater, may perhaps be confidered as forbidding the method I recommend, but experience fhews this feparation is not dangerous, fince, as I have already faid, blood cannot collect between thefe two parts, without feparation, and yet they return to their natural ftate, when the fluid is evacuated by the trepan, even where the feparated parts have long remained divided from each other by the interpofition of the fluid.

Of the cafes which fupport my opinion, it will be fufficient to mention the following.

A young perfon after the fcàrlet fever, had a violent pain which fixed itfelf at the upper part of the head; every thing art could indicate was tried to effect a cure, bleeding, bathing, cathartics, internal remedies, topicals of every kind, and blifters on the affected part, all had failed: when I was confulted, I advifed the moxa, which was applied to the difeafed part, and though a plentiful fuppuration followed, the pain feemed to increafe, and for fix months continued to augment; when I was again requefted to give my advice. I prefcribed the trepan, which operation was immediately performed, in the centre of the painful part: the opening made in the fcull by this

 means

means gave vent to a quantity of pus of a greenifh white colour; the pain ceafed entirely, the patient was foon cured; and fince has enjoyed a perfect ftate of health.

The preceding obfervation clearly fhews the dura mater had been long feparated from the fcull by the matter, and proves that the feparation of this membrane is not dangerous.

It will be faid perhaps that this feparation did not produce any bad effect becaufe it took place gradually: my anfwer is, an effufion occafioned by violent blows is fuddenly formed, it forces the dura mater from the cranium with violence, and feparates it fometimes to a great extent. It may be again objected that nature though acting haftily, manages in a manner art cannot imitate in feparating the dura mater from the fcull. I will oppofe this objection by experience, and not argument.

The 29th of March 1795, there was brought to the French hofpital eftablifhed in this city, a man about thirty-eight years of age, of a middle fize and very robuft conftitution: he was comatofe, his face inflated and difcoloured with ecchymofis, his body covered with bruifes, and many wounds made with pointed inftruments: thofe who brought him, told me he had been ftruck with an iron bar which fractured his fcull; and had been trepanned on the fpot.

After uncovering the head, it was wafhed and fhaved: and I found the trepan had been applied on the upper part of the right parietal bone, about an inch from the coronal future.

I took away, with the lenticular knife, pieces of the internal plate which wounded the dura mater, and enlarged the wounds in the direction of the fracture on which the trepan had been applied; it proceeded from the fagittal future, and defcended almoft in a right line into the temporal region, at the upper part of which I bounded my incifion, although

although the fracture extended lower: I obferved another fracture in the upper and lower part of the fame parietal, which had feparated a piece of the bone about three inches long, and two wide; this piece was neither indented nor difplaced, was behind and a little above the part trepanned: blood iffued from the fuperior and pofterior fractures.

The next morning the patient was in the fame ftate, infenfibly voided his urine and could not fwallow. The dreffing was removed, much blood came from the opening by the trepan, and from the fractures.

When I vifited him in the evening he was comatofe; but little blood came from the wound, nor did the preffure I made on the dura mater produce more. I introduced a blunt flexible probe under the fcull in the direction of the fracture, from whence the blood proceeded in the laft dreffing, and endeavoured to do the fame by the fracture which defcended to the temporal region: at the diftance of about a quarter of an inch, was ftopped by a fudden refiftance, and it was at that moment reflection fuggefted the method I immediately put in practice.

I prefumed the comatofe drowfinefs which continued, was occafioned by collected blood, and that it exifted under one of the points of the fracture in the temporal region, becaufe thofe effufions which had been formed under the other fractures, were evacuated in the preceding dreffing.

Had I followed the ufual method, it is poffible I might have made many openings before I had fucceeded, or have failed finding it; confequently after the reflection which fuggefted, that the adhefion between the fcull and the dura mater might be feparated without inconvenience, I determined to feparate the membrane by following the direction of the fracture; and proceeded to this operation with a filver fpatula very flexible, the extremities of which were rounded: I took the precaution to prefs it towards the bone, and to bend my inftrument by degrees as I entered, to make

it

it take the form of the part upon which I acted, and often drew it back, to meaſure on the outſide the way it had made. At length after having entered half an inch below the temporal ſcaly ſuture, the reſiſtance ſuddenly ceaſed, and my inſtrument entered a hollow part, at the ſame moment the blood flowed in great abundance; when it ceaſed I drew out the ſpatula, which was followed by a ſmall quantity. The patient then began to move ſtrongly, tried to riſe and talked without knowing what he ſaid.

The next morning I found him tied in his bed, this method was neceſſary, becauſe he endeavoured to riſe, as as he ſaid to go and fight. More blood came away at the dreſſing. In the afternoon I found him better, he drank plentifully, and anſwered my queſtions: the next morning, being the fourth after the accident, he had perfectly recovered his ſenſes, and from that time continued to mend. As his head had been much wounded, many abſceſſes were formed on the exterior, the laſt was on the piece of the parietal bone already ſpoken of, and as it had no connection and was vacilating, I eaſily took it away, the dura mater recovered and followed the motion of the brain: the wound had ſuppurated and the cicatrix was much advanced, when the patient went out the 28th of December.

During the cure the patient felt no pain in that part of the head where I had ſeparated the dura mater; the cure of this trepan was neither longer or more difficult than uſual, if we except the complication from the gatherings, which are foreign to the ſubject.

I cited Mr. Mareſchal's obſervation, becauſe the caſe is ſimilar to that which makes the ſubject of this eſſay. Mr. Mareſchal's patient had a fracture which croſſed the parietal and temporal bones: mine had fractures in the ſame bones, and ſame places; there was alſo another, and ſome very ſerious bruiſes, which made the diſeaſe complicated. Mr. Mareſchal, trepanned his patient twelve times: I cured mine with one operation, and by a method which to the beſt of my knowledge, had never before been tried.

DEVEZE.

No. LXVIII.

Memoir on the Sand-hills of Cape Henry in Virginia.
By B. Henry Latrobe, *Engineer.*

December 19*th*, 1798.

Read, Dec. 21, 1798. FROM the falls of the great rivers of Virginia over the out-runnings of the granite ſtrata, the general level of the land gradually approaches the level of the ocean. At the falls it is elevated from 150 to 200 feet above the tide: on the ſea ſhore at Cape Henry, the original coaſt riſes not more than 15 feet above high water mark.

That the whole of this extenſive country, from the falls to the coaſt, is factitious, and of Neptunian origin, appears far from being hypothetical; and the foſſil teeth and bones, which accompany this memoir,* and which with many hundred more, were dug out of a well at Richmond, from the depth of 71 feet, prove that the depoſition of the ſuperſtrata is not of a date ſufficiently removed to have deſtroyed the ſoft and almoſt cartilaginous part of the joints, or

* The teeth appear to be thoſe of a ſhark. They are highly enamelled and extremely ſharp: their roots are perfectly ſound and entire, and the minute and almoſt tranſparent jags of many of them are as perfect as the reſt. They are found in every well, dug in or near Richmond, to a ſufficient depth; and, *as I am informed*, in every deep well for many miles below the city. The ſtratum in which they lie conſiſts of highly ſulphurated blue clay, abounding in pyrites, and which has the appearance of having been mud. They were firſt diſcovered in the beds of rivulets, which had worn their channels to the depth of this ſtratum; and obtained the name of *Indian Dart-points*, in the ſame manner, as the immenſe oyſterbeds, which have been quitted by the ocean, are vulgarly called *Indian* oyſter-banks.

The bones were dug from the ſame ſtratum. Among them are two out of ſix bones, which formed a *paw* of ſome animal unknown to me. Many very ſound vertebræ of fiſh, and a remarkably perfect thigh bone of a large bird have been in my poſſeſſion.

or to have injured the enamel of the teeth. The Neptunian theory of geogeny, has now very generally taken place the old volcanic ſyſtem, and, as far as conjecture and hypotheſis can forward ſcience, it is certainly more generally applicable. But along the coaſt of Virginia,* a proceſs is going forward, the reſult of which will be exactly ſimilar, and in which water has no immediate ſhare.

The ſhore, and the bed of the Atlantic near the ſhore, conſiſt of a fine ſand. The daily action of the flood tide carries a certain quantity of this ſand above high water mark, which being dried by the ſun and air, is carried further in land by the winds. The moſt violent winds on this coaſt, blow from the points between the N. Weſt and the Eaſt; and beſides, a gentle eaſterly breeze prevails the whole ſummer, during ſome part of almoſt every day. This eaſterly wind, which is in fact a trade wind, is felt as high as Williamſburg. It is ſaid to be felt, at this day, higher in land than formerly, and to be annually extending its influence; and it will no doubt, when the woods ſhall be more cleared away, blow health and coolneſs over a portion of lower Virginia, which is now conſidered as extremely unhealthy.

Theſe eaſterly winds blowing during the drieſt and hotteſt ſeaſon of the year, carry forward the greateſt quantity of ſand, and have amaſſed hills, which now extend about a mile from the beach. The natural level of the land, elevated little more than 10 feet above high water mark, has a very gentle declivity to the eaſt. It is now a ſwamp† of about

* I ſpeak only of the coaſt of Virginia at Cape Henry: for although I have the beſt reaſon to believe that the ſame natural proceſs has produced all the ſand banks, iſlands, and ſand hills from the Delaware to Florida: I have only *examined* that part of the coaſt, which is the ſubject of the preſent memoir.

† By a ſwamp I excluſively mean a piece of ground, the ſurface of which is wet and ſoft, but which has a ſound bottom. In this it differs from the Diſmal ſwamp, much of which is a *bog* or *moraſs*. Into the latter, a pole of any manageable length may be forced with great eaſe.

about five miles ſquare (25 ſquare miles.) The ſoil below the ſurface, is a white loamy ſand, and if the water falling upon, or riſing in it, had a free diſcharge to the ocean, it would probably be perfectly dry: this, however, the ſand hills prevent, and the water is diſcharged into the ſea to the ſouthward, and into the mouth of the Cheſapeak to the northward, by ſmall creeks, which find vent from the weſterly extremities of the ſwamp. Lynhaven creek is the moſt conſiderable of theſe drains. The ſwamp, or as the neighbouring inhabitants call it, the Deſart, is overgrown with aquatic trees and ſhrubs; the gum, *(L. ſtyraciflua)* the cypreſs *(cup. diſticha)* the maple *(acer rubrum)* the tree improperly called the ſycamore *(platanus occidentalis)* the magnolia glauca, the wax myrtle *(myrica cerifera)* and the reed *(ar. tecta)* are the principal. Of theſe many thouſands are already buried in the ſand, which over-tops their ſummits, and threatens the whole foreſt with ruin. Their deſtruction is ſlow, but inevitable. Upon the extreme edge of the ſand hills towards the ſwamp, the wind oppoſed by the tops of the trees, forms an eddy: the ſand carried along with it is precipitated, and runs down the bank into the ſwamp. Its ſlope is very accurately in an angle of 45°. By gradual accumulation, the hill climbs up their trunks, they wither ſlowly, and before they are entirely buried, they die. Moſt of them loſe all their branches, and nothing but the trunk remains to be covered with ſand, but ſome of the cypreſs retain life to the laſt.*

The Deſart abounds in deer, bears, racoons, and opoſſums. Its ſkirts are more thickly peopled than the ſterility of

* That the ſwamp with its trees extended to the ſea coaſt, perhaps *within* a century, is very evident from this circumſtance: between the ſummit of the ſand hills (ſee the drawing) and the ſea ſhore, and more eſpecially on the Cheſapeak ſide, the undecayed, though moſtly *dead* bodies of trees ſtill appear in great numbers. Being on the windward ſide of the ſand hills, they have not been more than half buried. At the light houſe there are none of the trees, (ſee the ſection) but to the weſtward and ſouthward are many.

of the ſoil would give reaſon to ſuppoſe; but the inexhauſtible abundance of fiſh and oyſters in the creeks, and the game, render it eaſy to ſupport a family.

The light houſe,* which was built about ſixteen years ago, is an octangular truncated pyramid of eight ſides, riſing 90 feet to the light, and ſunk 18 feet below the baſement courſe. Within a few yards of the light houſe, is the keeper's dwelling, a wooden building of two ſtories. Both are ſurrounded by a platform of plank, and, without any ſuch deſign in the architect, this platform has preſerved both theſe buildings from being buried in the ſand.

When the light houſe was built, it was placed upon the higheſt ſand hill at the Cape. Its diſtance from the beach may be 6 or 7 hundred yards, and the elevation of its baſe above high water, not leſs than 90 feet. At that time there was from the foot of the building, the moſt expanded view of the ocean, the Deſart, the Cheſapeak and its eaſtern ſhore. At preſent, a mound of ſand ſurrounds them, which overtops the keeper's dwelling, and has buried his kitchen to the eaves. The platform, which was laid upon the former level of the ſand, is an accurate ſtandard from whence to aſcertain its accumulation. The winds meeting in their courſe the elevated tower of the light, form a perpetual whirl around it, which licks up the ſand from the ſmooth ſurface of the timber, and heaps it around in the form of a baſon. Where the platform ceaſes, the ſand accumulates. The ſandy rim, while it protects the keeper from the ſtorms, renders his habitation one of the dreărieſt abodes imaginable. This rim is ſometimes higher, at others

* It is a good ſolid building of Rappahannoc freeſtone, but has the unpardonable fault of a wooden ſtair caſe, which being neceſſarily ſoaked with oil, expoſes the light to the perpetual riſk of deſtruction by fire. Such an accident might be attended with an incalculable loſs of lives and property, the mouth of the Cheſapeak being perhaps the inlet to more ſhips than any other in the United States.

others lower, according to the direction and ſtrength of the wind. Since the eſtabliſhment of the light, the hills have riſen about 20 feet in height (meaſuring from the platform) and have proceeded into the Deſart about 350 yards, from a ſpot pointed out to me by the keeper. I ſtepped the diſtance as well as I could, while at every ſtep I ſunk up to my ancles into the ſand. The height of the hill at the ſwamp, is between 70 and 80 feet perpendicularly. It is higher nearer the ſea, the inner edge being rounded off, and I think at its higheſt point, it cannot be leſs than 100 feet above high water mark. If the hills advance at an equal ratio for 20 or 30 years more, they will ſwallow up the whole ſwamp, and render the coaſt a deſert indeed, for not a blade of graſs finds nutriment upon the ſand.

Should this event take place, and ſome future philoſopher attend the digging of a well *in the high ſandy country, on the coaſt of Virginia*, his curioſity would be excited by foſſile wood, 100 feet below the ſurface. He would there diſcover a bed of vegetable and animal exuviæ, and going home, he might erect upon very plauſible ground, a very good-looking hypotheſis of a deluge, ſweeping the whole upper country of its ſand, and depoſiting it along the line of its conflict with the waves of the ocean.

B. HENRY LATROBE.

To Samuel Harrison Smith, Eſq.
one of the Secretaries of the American
Philoſophical Society.

P. S. The annexed drawing is a ſection of the Cape, in a direction N. E. and S. W. The ſcale is of courſe unequal, but the effect is true.

No. LXIX.

Supplement to MR. LATROBE's *Memoir.*

Read, Jan. 18, 1799. THE following notices were put into my hands, a few years ago, by an ingenious friend* of mine. They will, I think, form a very proper fupplement to Mr. Latrobe's paper, lately communicated to the Society.

BENJAMIN SMITH BARTON.

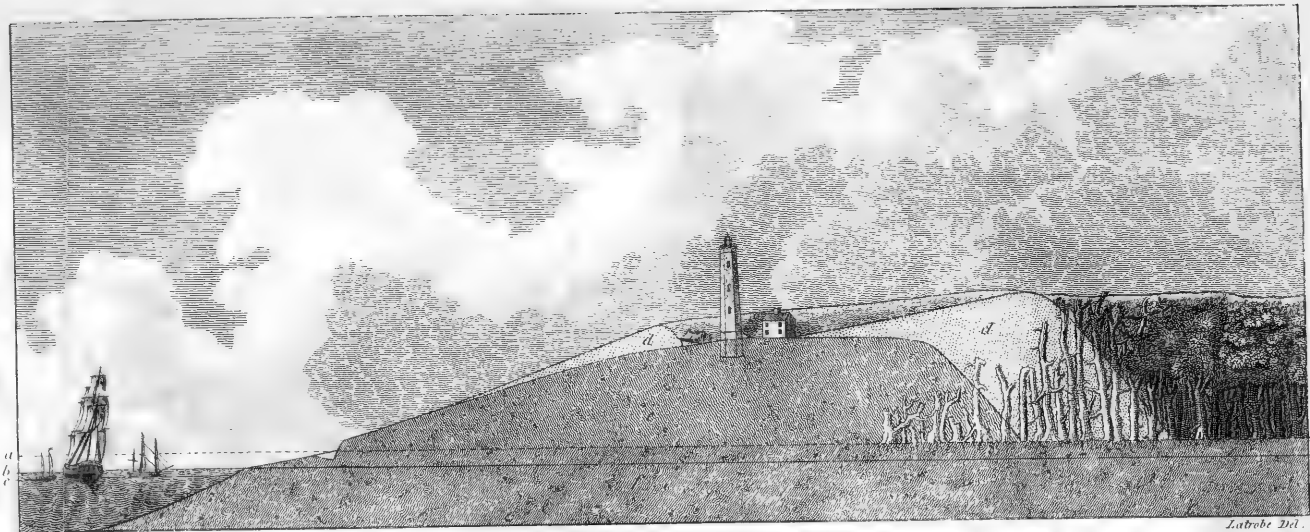
January 18th, 1799.

" I. The country below the Falls of James-River, in Virginia, is evidently an acquifition through the recefs of the fea. The mean diftance from thence to the fea is now one hundred miles. For demonftrative evidence, fee the gully in front of the eaftern door of the *Capitol*, about twenty feet below the level of *its* foundation, having the appearance of blue clay, but on examination will be found to be fea fand, containing fcolop, oyfter, clam, Englifh cockle, and various other fhell-fifh in their natural pofition, without any evidence of their removal by hand, univerfal convulfion, or feparation of the upper and under fhell. See alfo (the fame level perhaps) in the road between Mr. Selden's and Mr. Banks's plantations defcending the hill to the New Bridge, fix miles from the capitol, for the fame evidence.

" II. The wells of Dr. M'Clurg, W. Hay, Efq. and Samuel Swan, all on fpurs of the fame hill, where the Capitol ftands, contain at a depth from 50 to 75 feet (being many feet below the above-recited horizon) proofs of univerfal convulfion, fuch as the bones of marine and terreftrial animals, birds, fifhes, &c. with fome works of art, mixed promifcuoufly in a blue fea-fand (of hepatic quality perhaps). See the following Section."

Account

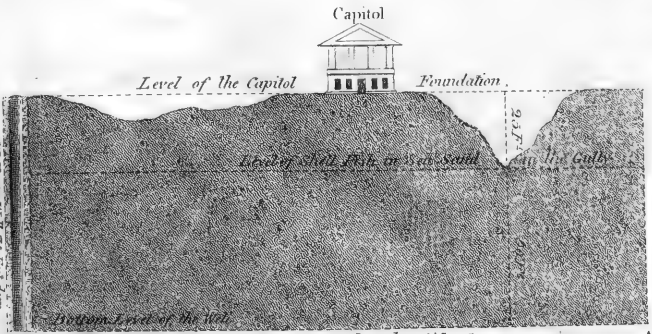
* Colonel William Tatham.



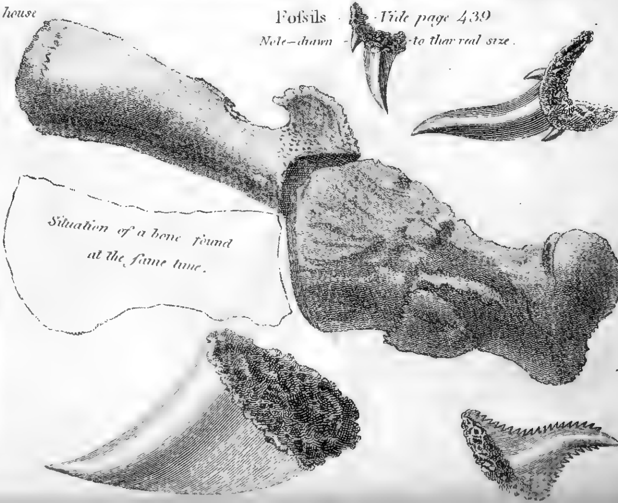
a. Original Land level
b. Level of high Water } difference 4 feet.
c. Do. of low Water

d. Accumulation of Sand since 1783
e. Do.... at the time of building the Light house
f. Soil of the Desart.

Section of the Coast of Virginia at Cape Henry.



A on this side the gully produces as on the other Side at - - - - - - A



No. LXX.

Account of CHRYSTALLIZED BASALTES *found in Pennsylvania*—*By* THOMAS P. SMITH.

Read, Jan. 18, 1799.

THE first place at which I found these basaltes was on the Conewaga hills, east of the Susquehanna and about half a mile to the north of Elizabeth-town. They are here to be found in considerable quantities, both chrystallized and amorphous—The chrystals are generally tetrahedral and of a very fine grain. There are great masses of it lying about amorphous, but it generally has a very strong apparent tendency to chrystallize.—As I travelled in the stage I had not an opportunity of examining this place as minutely as I could have wished. It is I think well worthy the attention of a minerallurgist whose time will permit and talents enable him to explore it accurately.

On my return from Northumberland by a different rout, I again found them at Campbell's town; they are here evidently a lateral branch of the Conewaga hills, and are scattered on the surface in the greatest profusion.

Soon after this I met with them on recrossing the Conewaga hills at Grubb's mines: as I now travelled in a private carriage, I had a much better opportunity of examining this part than near Elizabeth-town. At the foot of these hills Dr. Barton found a great quantity of regularly chrystallized *granite*, the predominate figure tetrahedral, higher up the chrystallized basaltes appeared; and what in my opinion is a strong corroborating proof of their Neptunian origin, they were interspersed with large masses of *brechia* composed of *silicious* pebbles evidently rounded by friction imbedded in the red free-stone of our mountains.

From

From theſe facts I am induced to believe, that if this chain of hills was accurately explored, it would be found to abound in its whole extent in chryſtallized *baſaltes*, and in this opinion I am ſtill further confirmed from having obſerved the ſtrong tendency the *whin*, as it is commonly called, has to aſſume a regular figure on a ſpur of theſe hills I croſſed in going from Lancaſter to Columbia.

No. LXXI.

Obſervations for determining the Latitude and Longitude of the Town of Natchez—By ANDREW ELLICOT, *Eſq. Commiſſioner on the part of the United States, for running the line of Demarkation between them and the Spaniſh Territory. Communicated to the Society by* R. PATTERSON.

Read, November 16, 1798.

1797. Plane of the Sector Eaſt.

Month	Day	Observation	°	′	″	
March	4th	Obſerved Zenith diſtance of Pollux	3	2	58	S.
	5th	do.	3	3	1	
	7th	do.	3	2	58	
	8th	do.	3	2	56	
Do.	5th	Obſerved Zenith diſtance of Caſtor	0	45	56	N.
	7th	do.	0	45	55	
	8th	do.	0	45	56	
Do.	5th	Obſerved Zenith diſtance of B. Tauri	3	7	59	S.
	7th	do.	3	7	57	
	8th	do.	3	8	0	

Plane of the Sector Weſt.

Month	Day	Observation	°	′	″	
Do.	9th	Obſerved Zenith diſtance of Pollux	3	4	0	S.
	10th	do.	3	3	59	
	17th	do.	3	3	56	
Do.	10th	Obſerved Zenith diſtance of Caſtor	0	44	55	N.
	17th	do.	0	44	57	
	19th	do.	0	44	50	
April	11th	do.	0	44	56	
March	14th	Obſerved Zenith diſtance of B. Tauri	3	8	58	S.
	17th	do.	3	8	58	
	18th	do.	3	8	54	
	20th	do.	3	8	55	
	22d	do.	3	8	57	
	23d	do.	3	8	56	

THE

THE RESULTS.

	°	′	″	
Mean obſerved Zenith diſtance of Pollux with the plane of the Sector E.	3	2	58.25	
Mean obſerved Zenith diſtance of Pollux with the plane of the Sector W.	3	3	58.3	
Mean or correct Z. D.	3	3	28.27	
Refraction			+ 3	
True Z. D.	3	3	31.27	S.
Mean declination of Pollux to the beginning of 1796	28	30	19.8	N.
Annual preceſſion till the 15th March, 1797			— 9.1	
Aberration			+ 0.8	
Nutation			+ 3.4	
Semi-annual equation			+ 0.3	
True declination	28	30	15.2	
True Z. D. add	3	3	31.3	
Latitude	31	33	46.5	
Mean obſerved Zenith diſtance of Caſtor with the plane of the Sector E.	0	45	55.9	
Mean obſerved Zenith diſtance of Caſtor with the plane of the Sector W.	0	44	54.5	
Mean or correct obſerved Zenith diſtance	0	45	25.2	
Refraction			+ 0.75	
True Zenith diſtance	0	45	25.95	
Mean declination of Caſtor to the beginning of 1796	32	19	10.4	
Annual preceſſion till the 15th March, 1797			— 8.5	
Aberration			+ 2.1	
Nutation			+ 6.9	
Semi-annual equation			+ 0.4	
True declination of Caſtor March 15th 1797	32	19	11.4	N.
True Zenith diſtance		—45	25.9	
Latitude	31	33	45.5	

Mean

	°	′	″
Mean obſerved Zenith diſtance of B. Tauri with the plane of the Sector E.	3	7	58.7
Mean obſerved Zenith diſtance of B. Tauri with the plane of the Sector W.	3	8	56.3
Mean or correct Zenith diſtance	3	8	27.5
Refraction			+ 3.1
True Zenith diſtance	3	8	30.6
Mean declination of B. Tauri to the beginning of 1796	28	25	15.2
Annual preceſſion till the 15th March 1797			+ 5.1
Aberration			+ 1.7
Nutation			— 1.0
Semi-annual equation			+ 0.5
	28	25	21.5
True Zenith diſtance add	3	8	30.6
Latitude	31	33	52.1

	°	′	″	
Latitude by Pollux	31	33	46.5	
Do. by Caſtor	31	33	45.5	
Do. by B. Tauri	31	33	52.1	
Mean	31	33	48	Latitude of the Town of Natchez.

OBSERVATIONS FOR THE LONGITUDE.

Obſerved the times, and diſtances of the Moon and Sun's neareſt limbs.

1797	Times. h	′	″	Diſtances. °	′	″	Longitude W. from Philad. °	′	″
March 3d	2	59	30	59	46	58	16	23	30
3d	3	54	50	60	3	51	16	26	45
3d	4	28	44	60	13	8	16	15	00
4th	2	10	27	72	6	29	16	24	45
4th	4	53	17	72	58	9	16	10	45
6th	2	37	42	98	12	14	15	52	30*
17th	21	9	31	109	41	5	16	13	30
21st	21	25	36	65	49	9	16	2	00
21st	21	36	30	65	45	56	16	15	00
22d	21	46	24	54	48	32	16	10	00
23d	21	26	7	43	52	28	16	25	45

OBSERVATIONS ON A LUNAR ECLIPSE.

1797		h	′	″			
Dec. 3d	Beginning	8	38	24	16	10	30
	Total Darkneſs	9	37	35	16	18	00
	End of total darkneſs	11	18	59	16	9	45
	End of the Eclipſe	12	18	12	16	11	00
				Mean	16	13	55

OBSERVATIONS

* If this obſervation, which appears to have been inaccurate, be ſtricken out, the mean of the remaining ten will agree with the mean of a like number of obſervations made on the eclipſes of Jupiter's ſatellites, within a *ſingle ſecond.* This fact is a ſtrong proof in favour of the accuracy of this method of determining the longitude of places.

R. P.

Note, to face page 451.

All the obfervations for the longitude of Natchez after the 23d of March, are entered as obferved by the clock, and will therefore require a correction to reduce them to mean folar time, which may readily be done from the following ftatement of the errors of the clock, with its rate of going, at different periods during the courfe of the obfervations.

		′	″	″
1797.				
June 12th	Clock too faft mean time	3	55	- - 2.9 daily lofs.
17th	. . . .	3	40.6	
26th	The clock was taken out of the tent, and removed into a houfe where it was not attended to till			
Sept. 28th	when I cleaned it, and fet it agoing.			
29th	Clock too faft mean time .	9	30.4	- - 9.1 daily gain.
30th	. do. .	9	39.5	- - 9.7 do.
Oct. 7th	. . do. . .	10	47.4	- - 11.4 do.
18th	. do. .	12	53.0	- - 11.4 do.
26th	. . do. . .	14	24.3	
Nov. 22d	Clock ran down, wound it up and fet it agoing, lowered the bob of the pendulum.			
24th	Clock too faft mean time.	16	22	- - 3.0 daily gain.
26th	. do. .	16	28	- - 0.2 do.
Dec. 4th	. . do. . .	16	30	- - 3.5 do.
6th	. do. .	16	37	- - 0.7 do.
8th	. . do. . .	16	38.5	- - 0.2 do.
16th	. do. .	16	40.5	- - 1.7 do.
18th	. . do. . .	16	44	- - 2.7 do.
21st	. do. .	16	52	- - 3.5 do.
1798.				
Jan. 1st	. . do. . .	17	31	
2d	Stopped the clock about 19 minutes and lowered the bob of the pendulum a fmall matter, but fcarcely difcernible with a magnifying-glafs.			
5th	Clock too flow mean time.	1	21	- - 0.3 daily gain.
8th	. do. .	1	20	- - 2.0 daily lofs.
9th	. . do. . .	1	22	- - 1.0 do.
15th	. do.	1	28.2	

OBSERVATIONS MADE ON THE SATELLITES OF JUPITER.

1797				h	′	″	Long. W. from Philad. °	′	″
June 12th	Immersion of 1st		Satellite	15	28	25	16	17	30
Sept. 28th	do.		do.	14	30	10	16	22	15
30th	do.		do.	8	59	19	16	19	45
Oct. 25th	Emersion of		do.	5	55	12	16	10	15
Nov. 24th	do.		do.	8	7	33	16	16	00
Dec. 7th	do.	of 2d	do.	7	56	31	16	17	45
17th	do.	of 1st	do.	8	24	30	16	16	00
24th	do.		do.	10	21	1	16	19	30
1798 Jan. 8th	do.	of 2d	do.	7	22	12	16	12	15
1798 Jan. 9th	do.	of 1st	do.	8	23	10	16	15	45
						Mean	16	16	42

From the above it appears that the Longitude deduced from the eclipses of Jupiter's satellites exceeds that deduced from the lunar observations including the eclipse of the Moon on the 3d of December last, by 2′ 47″—But I am of the opinion that the most dependence is to be placed upon the eclipses of Jupiter's satellites, which together with the Lunar eclipse may be further corrected if any corresponding observations should happen to have been made in any place where the Longitude has been accurately settled. The following statement in which the Longitude deduced from the eclipses of the satellites is given double the weight of that deduced from the Lunar observations will certainly give the Longitude of the town of Natchez with great accuracy.

	16°	13′	55″	
	16	16	42	
	16	16	42	
Mean	16	15	46	Longitude West from Philadelphia.

No. LXXII.

An Anſwer to DR. JOSEPH PRIESTLEY's *Conſiderations on the Doctrine of Phlogiſton, and the Decompoſition of Water; founded upon demonſtrative Experiments.* By JAMES WOODHOUSE, M. D. Profeſſor of Chemiſtry in the Univerſity of Pennſylvania, &c.

SECTION I.

Of the Conſtitution of Metals.

DR. Prieſtley in two late publications, entituled, Conſiderations on the Doctrine of Phlogiſton and Decompoſition of Water, has attacked that theory of chemiſtry, which is at preſent adopted by a large majority of chemiſts, in different parts of the world.

The doctor adheres to the doctrine of phlogiſton, and believes that metals are compound bodies, formed of this ſubſtance and a peculiar baſe or calx.

On the contrary, the antiphlogiſtic chemiſts reject phlogiſton.

Firſt. Becauſe it appears to be a mere creature of the imagination, whoſe exiſtence has never been proved.

Secondly. Becauſe all the phænomena of chemiſtry, can be ſatisfactorily explained, without the aid of this hypotheſis.

They believe metals to be ſimple ſubſtances, becauſe they have never been proved to be compound bodies.

They conſider a metallic calx, to be an union of a metal and the baſe of vital air, called by them oxygen, as it is the principle of univerſal acidity. The proofs that metals, in being converted into calces, abſorb oxygen are,

Firſt. That all the calces of mercury give out oxygenous gas when expoſed to a red heat, without any addition.

Secondly. If a metal is calcined in oxygenous gas, the whole of it will be abſorbed.

Thirdly.

Thirdly. If the procefs of calcination is performed in a variety of gafes, containing fome oxygenous air, the oxygen only will be imbibed by the metal, and the others will be left unaltered.

Fourthly. If any fubftance is added to a metallic oxyd, and the calx is revived, a compound body will be produced, formed of the agent ufed and the oxygen contained in the calx.

Thus, if the filings of pure bar iron are mixed with red precipitate, and expofed to a red heat, the iron will be converted into a calx and the mercury will be revived. If pure charcoal is mixed with the precipitate, carbonic acid will be produced; and if the mercurial calx is revived in hydrogenous gas, water will be formed.

The firft objection of Dr. Prieftley, to this theory of the calcination of metals, is as follows.

He fays, if turbith mineral is expofed to a red heat, a calx remains which cannot be revived in any degree of heat, without the aid of fome fubftance, fuppofed to contain phlogifton. Before we proceed any further in this inveftigation, it is abfolutely neceffary to determine the real compofition of turbith mineral.

According to the French philofophers, this fubftance is a pure oxyd of mercury.

Fourcroy and Baumé declare, that it does not contain one particle of the fulphuric acid. Dr. Prieftley is doubtful whether it is a falt or a calx; and in the Edinburgh Difpenfatory and London Pharmacopœia Chirurgica, it is called hydrargyrus vitriolatus flavus.

The following experiments were made, to afcertain the compofition of this fubftance.

Firft. One ounce of pure turbith mineral was expofed to a red heat, in a long glafs tube, which communicated with an hydropneumatic apparatus, when thirty-three ounce meafures of oxygenous gas were obtained. Upon breaking the glafs, a quantity of fluid mercury was found in the tube.

tube. Two drachms of the ſulphate of mercury, of a white colour and ſtrong acrid taſte, had ſublimed on the ſides of the glaſs. A part of the ſulphate of mercury, was coloured by an immenſe number of minute particles of revived mercury, which gave it the appearance of mercurius cinereus.

Secondly. One ounce of turbith mineral, was boiled fifteen times, ſix hours each time, in half a pint of diſtilled water, which was renewed every time; and it could not be freed from the ſulphuric acid, for the water always precipitated a ſolution of muriated barytes.

Thirdly. One ounce of turbith mineral was boiled three hours, in a ſolution of cauſtic potaſh, when it loſt its yellow colour, and was converted into a calx of the colour of brickduſt. Upon being dried it was found to have loſt one hundred and ſixty grains in weight.

The liquor in which it was boiled, by ſpontaneous evaporation in the open air, gave chryſtals of vitriolated tartar.

Theſe experiments were repeated with turbith mineral, made by precipitating a ſolution of the ſulphate of mercury by potaſh, with the ſame reſult.

They clearly prove, contrary to what has been advanced by Lavoiſier, Monnet, Bucquet, Fourcroy, Chaptal and other French chemiſts, that turbith mineral, is not a pure oxyd of mercury, but contains ſulphuric acid, and may be conſidered as a ſulphate of mercury.

The reaſon that thoſe gentlemen were deceived in regard to the compoſition of this ſubſtance muſt have been, either that they did not break the veſſels in which their experiments were made, to diſcover any reſiduum, or from the circumſtance, of obtaining oxygenous gas from the turbith, equally as good as from any acknowledged calx of mercury.

The reaſon that turbith mineral yields oxygenous gas, when it is expoſed to a red heat is, that the ſulphuric acid quits

quits one part of it and joins to another, which ſublimes in the form of a white ſalt. That part which the ſulphuric acid leaves, is converted into a calx, is revived without addition, and yields oxygenous gas.

This ſulphate of mercury is the ſuppoſed calx, to which Dr. Prieſtley refers. It is ſometimes obtained of a red colour, owing to ſome impure matter contained in the turbith mineral, which by depriving a part of the ſulphuric acid of its pure air, converts it into ſulphur, which uniting with part of the revived mercury, forms cinnabar, which gives the whole of the ſublimed ſalt a red colour.

That it is a ſulphate of mercury, we have an additional proof, from an experiment of Dr. Prieſtley, for he procured ethiops mineral, by heating this ſuppoſed calx in inflammable air, by means of a burning lens, which he could not have obtained from a pure calx of mercury, treated in the ſame manner.

The ſize of the veſſel in which turbith mineral is heated, will vary the reſult of the experiment. No reſiduum can be obtained, by expoſing it in a crucible to a red heat, for the whole of it flies away, and leaves only a mark on the bottom of the veſſel. The ſame circumſtance will take place, if a ſhort glaſs tube is uſed.

Having thus determined, that the ſubſtance which remains after expoſing turbith mineral to a red heat, is a neutral ſalt coloured red by cinnabar, and not a metallic calx, we ſee that the firſt objection of Dr. Prieſtley, to the theory of the calcination of metals, adopted by the antiphlogiſtic chemiſts, loſes all its force, for certainly it does not follow, that becauſe the ſulphate of mercury requires to be deprived of its ſulphuric acid, before running mercury can be procured from it, that therefore all mercurial calces require the addition of phlogiſton, to be converted into mercury.

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The ſecond objection of Dr. Prieſtley, to the new theory of chemiſtry is, that when a metal is reduced to a calx, it throws out ſomething which forms phlogiſticated air. He ſays, that when the focus of a burning lens, is thrown upon iron confined in atmoſpheric air, the dephlogiſticated air is not merely ſeparated from the phlogiſticated air, but that the phlogiſton from the iron, unites with the dephlogiſticated air and forms azotic gas.

In order to ſee if this aſſertion was juſt, the focus of the burning lens belonging to our ſociety, which is eleven inches in diameter, was thrown upon ninety grains of the filings of bar iron, filed for the purpoſe, confined in thirty-two ounce meaſures of oxygenous gas, which had been well waſhed in lime water, and which was ſo pure, that nearly the whole of it was devoured by the teſt of nitrous air. Twenty-eight ounce meaſures of the pure air were abſorbed by the iron, which was reduced to a calx.

The quantity of carbonic acid produced, which was formed by a ſmall quantity of coal, which all iron of commerce contains, uniting to a part of the pure air, amounted to one ounce meaſure.

When the fixed air was abſorbed by waſhing it in lime water, the remaining air was in no manner injured.

The focus of the lens was likewiſe thrown, upon ſixty grains of the filings of copper, confined in ſixteen ounce meaſures of oxygenous gas. Twelve ounce meaſures of the pure air were abſorbed by the metal, which was converted into a calx. No carbonic acid or azotic gas was formed, and the remaining air was perfectly pure. Theſe experiments prove, contrary to what has been ſaid by Dr. Prieſtley, that when a metal, containing no foreign ſubſtance, is calcined in oxygenous gas, the pure air only is imbibed, no ſubſtance is emitted from the metal, and no azotic gas is formed.

SECTION

SECTION II.

Of the Solution of Iron in the diluted Sulphuric and Muriatic Acids.

The next thing which engages the attention of Dr. Prieſtley, is the ſolution of iron, in the diluted ſulphuric and muriatic acids.

The queſtion to be decided is, whether the hydrogenous gas which is produced, comes from the iron, or from the water which the acids contain.

The antiphlogiſtic chemiſts contend, that it comes from the water, for the following reaſons.

Firſt. If concentrated ſulphuric acid is boiled upon iron filings, ſulphureous gas is produced, but no inflammable air, and the ſulphuric acid ſuffers a decompoſition and a loſs in weight.

Secondly. If the ſulphuric acid is digeſted upon iron in the cold, it remains in a quieſcent ſtate, but the inſtant water is added, a violent action enſues, accompanied by a diſcharge of hydrogenous gas.

Thirdly. They believe that the hydrogenous gas comes from the water, becauſe no inflammable air, can be produced from iron without water, and the hydrogenous gas obtained is in ſtrict proportion to the water, which the acids contain.

Fourthly. They believe, water is decompoſed in diſſolving iron in the diluted ſulphuric acid, that its oxygen calcines the metal, while its hydrogen eſcapes, and that the acid acts upon the calcined metal without being decompoſed, for it will ſaturate as much alkali, after the proceſs of ſolution, as it did before.

Fifthly. They prove that water is compoſed of oxygen and hydrogen.

Dr. Prieſtley's objection to this explanation is, that as one hundred parts of water, according to the advocates of the

the new ſyſtem of chemiſtry, are compoſed of eighty-ſeven parts of oxygen and thirteen of hydrogen, which is nearly ſeven times as much of the former as of the latter, there muſt be a great depoſition of oxygen ſomewhere, when iron is diſſolved in the diluted ſulphuric acid, which he cannot diſcover.

He denies that it unites to the metal, and declares there is no addition of oxygen in the proceſs, and conſequently that there is no decompoſition of water in the caſe.

That there is a quantity of oxygen, which unites to the metals, when diſſolved in acids, I think can be eaſily proved.

In order to do this I will ſhew, that when pure metallic calces, which are acknowledged by Dr. Prieſtley to contain oxygen, are heated in hydrogenous gas, that the oxygen of the calces unites to the hydrogen and forms water, and that the diſappearance of the inflammable air, is always in ſtrict proportion to the pure air, which the calces contain.

I will then prove that the calces of copper and iron, obtained from the ſulphates of theſe metals by ammoniac, have this property of making large quantities of inflammable air diſappear. The oxyds which are acknowledged to contain oxygen are mercury, lead and manganeſe.

The focus of the lens was thrown upon two drachms of red precipitate, confined in thirty-two ounce meaſures of hydrogenous gas, obtained from the ſulphuric acid diluted with water and the filings of bar iron, and which had been well waſhed in lime water. Twenty-two ounce meaſures of the inflammable air diſappeared, the mercury was revived and no carbonic acid gas was produced. The air which remained behind was not altered.

According to Dr. Prieſtley, fixed air ſhould have been formed in this proceſs, for he ſays, when any ſubſtance known to contain oxygen, is heated in inflammable air, fixed air is found, but this is not the caſe.

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I agree with the Doctor, that carbonic acid gas will be obtained by reviving minium, or mercurius precipitatus per ſe in inflammable air, for theſe calces generally contain it, but if the minium be converted into maſſicot, no fixed air will be generated.

Here we have a ſtrong proof of the poſition we are endeavouring to eſtabliſh.

Two drachms of red lead, make twenty ounce meaſures of inflammable air diſappear, when heated in it by the burning lens, but when converted into maſſicot, only eight ounce meaſures.

Now, if Dr. Prieſtley's theory was true, that the metal imbibed the air, maſſicot ought to abſorb more inflammable air than minium, as it contains more lead than an equal weight of minium.

In making red lead into maſſicot, nothing but pure air with a ſmall quantity of fixed air eſcapes, and the loſs of the pure air is the true reaſon, that one calx of the ſame metal, will make more inflammable air diſappear than another.

But we have ſtill ſtronger proofs, to prove that our ideas on this ſubject are juſt.

The focus of the lens was thrown upon one drachm of the oxyd of manganeſe, confined in thirty ounce meaſures of hydrogenous gas, when twenty-two ounce meaſures of the gas diſappeared, and the metal was not revived. How then could the inflammable air have entered into its compoſition ?

A quantity of the oxyd of manganeſe, was expoſed to a red heat for three hours, and a part of its pure air was driven off, when upon throwing the focus of the lens upon one drachm of it confined in inflammable air, none of the air diſappeared, whereas if this quantity of the oxyd, had not been expoſed to a red heat, twenty-two ounce meaſures of the air would have vaniſhed.

Some manganeſe was alſo precipitated from its ſolution, in the muriatic acid by ammoniac, and when freſh made it would never make any inflammable air diſappear, when heated in it by the burning lens, but after being expoſed a few days to the action of atmoſpheric air, one drachm of it made four ounce meaſures of inflammable air diſappear. In all theſe caſes we evidently ſee the operation of oxygen. Not knowing the exact quantity of pure air, which iron and copper abſorbed, one drachm of the filings of bar iron were melted by the burning lens in oxygenous gas when twenty-ſix ounce meaſures were imbibed by the iron, and the ſame quantity of the filings of copper treated in the ſame manner gave an abſorption of thirteeen ounce meaſures.

One drachm of the precipitate of iron, from a ſolution of the ſulphate of iron by ammoniac, was then heated in forty-ſix ounce meaſures of hydrogenous gas, when thirty-ſix ounce meaſures of the air diſappeared.

The ſame quantity of the common ruſt of ſteel, and the carbonate of iron, obtained from green vitriol by a ſolution of mild pot aſh, and what Dr. Prieſtley calls a nitrated calx of iron, formed by adding nitric acid to a calx of iron and expoſing it to a red heat, when treated in the ſame manner, made exactly as much air vaniſh.

One drachm of the precipitate of copper, from a ſolution of blue vitriol by ammoniac, expoſed to the action of the lens in hydrogenous gas, made eighteen ounce meaſures of the air diſappear.

Here then are two metals, one of which the iron, abſorbs twice as much oxygen, when melted in it, as the copper, and its calx following the ſame proportion when heated in hydrogenous gas, makes exactly twice as much of the air diſappear.

After one drachm of the calx of iron, had made thirty-ſix ounce meaſures of inflammable air diſappear, it was expoſed to the action of the lens in oxygenous gas, when four

four ounce meaſures of the air were abſorbed, and after this being again heated in hydrogenous gas, ſix ounce meaſures of the air vaniſhed.

In all theſe experiments nothing but water was produced. The carbonic acid gas was not obtained, unleſs it previouſly exiſted in the calces.

It is not however denied, that fixed air may be generated by heating a pure metallic calx, in a particular kind of inflammable air. Thus it may be made by reviving red precipitate in hydrogenous gas, obtained from expoſing the flowers of zinc and coal to a red heat, or from paſſing alcohol over red hot iron, but none will be procured from that made by the diluted ſulphuric acid and malleable iron, or from that obtained by paſſing the ſteam of water over malleable iron.

Upon reviving three drachms of red precipitate, in thirty-ſix ounce meaſures of hydrogenous gas, from the flowers of zinc and coal, and which had been well waſhed in lime water, there was an abſorption of only two ounce meaſures.

After the operation, there was a great production of carbonic acid gas. Water was not formed in this proceſs, for the coal held in ſolution in the hydrogenous gas, had a ſtronger attraction to the pure, than to the inflammable air, and conſequently fixed air was generated.

Had the ſame quantity of precipitate been revived in inflammable air, from malleable iron, upwards of thirty ounce meaſures of the air would have vaniſhed.

Dr. Prieſtley, ſuppoſing that the inflammable air, or the phlogiſton it contains, enters into the compoſition of the metals, has made a calculation of the quantity of this air abſorbed by an ounce of ſeveral of them. He calculates from the metal actually revived. According to him, one ounce of mercury abſorbs three hundred and ſixty-two ounce meaſures of hydrogenous gas. The quantity mentioned here, is far too great. One drachm of red precipitate, which con-

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tains more than fifty grains of mercury, makes twelve ounce meaſures of inflammable air diſappear.

It is a difficult matter to be exact in this experiment, for ſome of the precipitate always diſperſes in reviving the mercury, and a part of the metal ſublimes and adheres to the ſides of the veſſel which is uſed.

As I believe, that when a metallic calx is heated in hydrogenous gas, the oxygen of the calx, unites to the hydrogen and forms water, I always calculate from the quantity of hydrogenous gas that diſappears, from heating a given quantity of a calx in this air.

According to my experiments, one ounce of red precipitate, mercurius precipitatus per ſe, and the calx obtained by boiling a ſolution of cauſtic pot-aſh on turbith mineral, makes 112 ounce meaſures of inflammable air diſappear, when heated in it by the burning lens.

Red Lead	88
Maſſicot	32
Litharge	32
Manganeſe	192
Copper	144
Iron	288

Upon diſſolving half a drachm of the precipitate of iron, which had made ſixteen ounce meaſures of hydrogenous gas diſappear, in diluted ſulphuric acid, as much inflammable air was obtained, as two grains of the filings of malleable iron would have produced. According to this experiment, were I to calculate in the ſame manner as Dr. Prieſtley, I would ſay, that one ounce of bar iron abſorbs 3840 ounce meaſures of inflammable air, but this quantity of the metal by ſolution in the ſulphuric acid and water will yield no more than 365 ounce meaſures of hydrogenous gas.

If an ounce of mercury abſorbs 362 ounce meaſures of inflammable air, it ought to give out this air when diſſolv-

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ed in an acid, or ſome ſubſtance into which it enters as a conſtituent part. But mercury revived from red precipitate by inflammable air, boiled in ſulphuric acid gives ſulphureous gas, and when added to nitric acid, nitrous air, neither of which contain inflammable air.

It ſhould alſo exhibit ſome properties, when ſubjected to the action of chemical agents, different from that which is revived from a mercurial calx merely by an increaſe of its temperature, which is not the caſe; and if mercury abſorbs inflammable air, that which is revived without addition, when heated in inflammable air, ſhould abſorb ſome of it which it will not do.

It certainly is not probable, that an ounce of mercury containing more than twelve quarts of hydrogenous gas, ſhould have the ſame external appearance, and exhibit the ſame chemical properties, as that which does not contain one particle of this air.

Dr. Prieſtley not only believes, that when red precipitate is heated in hydrogenous gas, the inflammable air enters into the metal, but that, the pure air of the metallic calx is diffuſed through the hydrogenous gas which remains behind.

As a proof of this he mentions an exploſion, which happened from reviving red precipitate, in inflammable air. I have performed this experiment, with different proportions of red precipitate, twenty times, and never met with any accident.

The inflammable air that Dr. Prieſtley uſed, muſt have been mixed with atmoſpheric air, or an exploſion would not have happened. That the pure air of the metallic calx is not diffuſed through the inflammable air which remains behind, appears evident from the following circumſtance.

If one drachm of red precipitate, is revived in ſixteen ounce meaſures of hydrogenous gas, twelve ounce meaſures

ſures of the inflammable air will diſappear, and the remaining four ounce meaſures, will not be diminiſhed by the teſt of nitrous air.

When the proportion of precipitate is large, and the inflammable air ſmall, after the inflammable air diſappears, the precipitate will give out its oxygen, and the air which remains, will be diminiſhed by the teſt of nitrous air.

This circumſtance has happened in ſome of the experiments of Dr. Prieſtley.

Another objection brought forward by Dr. Prieſtley is, that if hydrogen be nothing more than a component part of water, it never would be produced, but in circumſtances in which either water itſelf, or ſomething into which water is known to enter is preſent. He tells us, that upon heating finery cinder together with charcoal, inflammable air is produced, though according to the new theory no water is concerned.

The antiphlogiſtic chemiſts never ſaid, that hydrogenous gas could not be produced without water; for it is a conſtituent part of other bodies, as alcohol and ammoniac.

To aſcertain the quantity of hydrogenous gas, afforded by charcoal and finery cinder expoſed to a high degree of heat, an ounce of the ſcales of iron and the ſame quantity of charcoal, both reduced to a very fine powder, were ſeparately expoſed in covered crucibles, in an air furnace, well ſupplied with fuel, for five hours. They were then taken out of the fire, and mixed while *red hot* in a *red hot* iron mortar, were triturated with a *red hot* peſtle, formed of an iron ramrod, were poured upon a *red hot* piece of ſheet iron, and inſtantly put into a *red hot* gun barrel which was fixed in one of Lewis's black lead furnaces, and which communicated with the worm of a refrigeratory, a part of a hydropneumatic apparatus. Immediately after luting one end of the gun barrel to the worm, one hundred and forty-two

two ounce meaſures, of inflammable air came over in torrents, mixed with one-tenth part of carbonic acid gas.

This experiment has puzzled all the advocates of the antiphlogiſtic ſyſtem, to whom it has been mentioned. Many conſider it as a powerful blow at the new doctrine, and every perſon explains it in a different manner.

Dr. Prieſtley's theory of it is very unſatisfactory, for he ſays the water from the finery cinder, uniting with the charcoal makes the inflammable air, at the ſame time that part of the phlogiſton from the charcoal, contributes to revive the iron.

This explanation will not do, for the iron is not revived, and it will not account for the production of the carbonic acid.

By conſidering the ſcales of iron, as a combination of iron, oxygen and water, there will be no difficulty in the buſineſs. The finery cinder ſupplies the coal with water, which is decompoſed; its oxygen unites with the coal and forms carbonic acid, while its hydrogen eſcapes, diſſolves part of the coal, and forms the carbonated hydrogen gas.

The celebrated Mrs. Fulhame, a lady whom I am proud to quote on this occaſion, is the only perſon I know, who ſeems properly impreſſed with the idea of the agency of water, in many chemical operations. This diſtinguiſhed lady, who is equally an example to her ſex, and an ornament to ſcience, has properly conſidered a metallic oxyd as a combination of a metal, oxygen and water.

There are other ſubſtances beſides finery cinder, which mixed with coal and expoſed to a red heat, yield hydrogenous gas and carbonic acid, in large quantities. Theſe airs may be obtained from the common ruſt of iron, or from any precipitate of iron, and coal which has ceaſed to yield air. They may alſo be procured, from the flowers of zinc and red hot coal.

One drachm of the flowers of zinc and twelve grains of red hot coal, which had ceafed to yield air, being expofed to a red heat gave fifty-eight ounce meafures of hydrogenous gas, every portion of which was mixed with fome carbonic acid.

One drachm of the precipitate of zinc, from a folution of white vitriol by ammoniac, expofed to a red heat half an hour, when mixed while red hot, with red hot coal, which had ceafed to yield air, gave fourteen ounce meafures of inflammable air, mixed with carbonic acid.

The flowers and precipitate of zinc in thefe cafes, fupplied the coal with water which was decompofed. The metal was not revived.

SECTION III.

Of Finery Cinder or the Scales of Iron.

The antiphlogiftic chemifts confider the fcales, which the blackfmiths ftrike off from red hot iron, to be iron partially oxygenated.

On the contrary Dr. Prieftley fuppofes, that when iron is heated in oxygenous gas, it parts with its phlogifton, and is converted into a fubftance refembling finery cinder, but he will not allow that the air which difappears in this procefs, is imbibed by the iron, but only the water which was its bafe, while the oxygenous gas, he fays, always goes to form the fixed air which is found in the experiment.

He declares that the quantity of carbonic acid, is quite fufficient to take all the oxygenous gas that difappears in the procefs.

That the Doctor's ideas are not juft on this fubject, we have the moft conclufive evidence.

If

If half a drachm of the filings of bar iron, are melted in twenty ounce meaſures of pure air, thirteen ounce meaſures of the air will be abſorbed by the iron, which will be converted into finery cinder. Half an ounce meaſure of carbonic acid gas will be produced.

Lavoiſier tells us, if the iron is pure, no fixed air will be obtained; and certainly Dr. Prieſtley will not ſay, that thirteen ounce meaſures of oxygenous gas enter into the compoſition of half an ounce meaſure of fixed air, which muſt be the caſe if his theory is true.

Here then are twelve and a half ounce meaſures of pure air, which cannot be accounted for according to the ſyſtem of Dr. Prieſtley, and when we ſee a ſubſtance produced, by melting iron in oxygenous gas, reſembling the ſcales of iron, in every property, and cannot account for the air which diſappears but by ſuppoſing it is imbibed by the iron, can we heſitate to pronounce, that the ſcales of iron contain oxygen.

The Doctor likewiſe ſuppoſes, that if oxygen was lodged in a calx of iron, it would dephlogiſticate the muriatic acid which minium inſtantly does, and which we grant does not contain a third as much pure air as a calx of iron.

To determine if finery cinder would dephlogiſticate the muriatic acid, four ounces of the acid, were diſtilled upon three ounces of the powdered ſcales of iron, without ſucceſs.

An attempt was alſo made to dephlogiſticate the acid, by diſtilling two ounces of the ſulphuric acid, upon three ounces of common ſalt, and as much of the ſcales of iron, without effect. The quantity of oxygen contained in theſe ſcales, muſt have been ſeveral hundred ounce meaſures.

Theſe trials however do not invalidate any thing, which has been advanced by the antiphlogiſtic chemiſts, for the oxygenation of the muriatic acid, does not depend ſo much upon the quantity of pure air contained in a calx, as upon its readineſs to give out this air to the acid; when the

 attraction

attraction between the oxygen and metal is greater than between the oxygen and the acid, the acid will not be oxygenated. This is the caſe with iron.

A proof that the oxygenation of the muriatic acid, does not depend merely upon the quantity of oxygen contained in a calx is, that a drachm of manganeſe, which has been expoſed ſeveral hours to a red heat, and parted with the greateſt part of its pure air, will oxygenate the muriatic acid to a greater degree, than one ounce of mercurius cinereus, or the calx obtained by boiling cauſtic alkali upon turbith mineral, which contain thirty times as much pure air.

The Doctor likewiſe obſerves, if finery cinder was iron partially oxygenated, it would go on to attract more oxygen from the atmoſphere, and in time be converted into a ruſt of iron.

In order to determine if finery cinder would attract oxygen, the focus of the lens was thrown upon a quantity of it, confined in pure air, which was not abſorbed.

The ſteam of water was alſo paſſed over it for ſeveral hours, when red hot in an iron tube, but it ſuffered no alteration.

One ounce of it reduced to a fine powder, was expoſed to the action of atmoſpheric air upwards of twelve months, and ſprinkled with water ſeveral hundred times, and at the end of this time, was as free from ruſt, as when firſt expoſed, while an ounce of iron filings moiſtened with water, were covered with ruſt in three days.

I acknowledge that finery cinder cannot be converted into ruſt, but cannot ſee in what manner this makes againſt the antiphlogiſtic ſyſtem. When bar iron is converted into finery cinder, it parts with the ſmall quantity of coal it contained, and abſorbs oxygen and water.

The ruſt of iron differs from it materially, for it contains a portion of carbonic acid, and although the French chemiſts

chemiſts conſider this preparation as a carbonate of iron, I do not think it is entituled to this appellation, for one ounce of it yields but four ounce meaſures of fixed air, whereas the ſame quantity of the precipitate from green vitriol by the common pot-aſh of the ſhops, yields thirty-two ounce meaſures, and deſerves this character with more propriety.

A ſtrong proof that finery cinder contains oxygen is, that when it is heated in hydrogenous gas, it makes a large quantity of it diſappear, and I have ſhewn, that when metallic calces are heated in this air, that the diſappearance of the inflammable air, is always in ſtrict proportion to the pure air which they contain.

SECTION IV.

Of Carbonic Acid or Fixed Air.

According to the advocates of the antiphlogiſtic ſyſtem, the carbonic acid or fixed air, is a combination of charcoal and oxygen. They are of this opinion for two reaſons.

Firſt. If charcoal be plunged in a veſſel of oxygen gas, the whole of it will be conſumed, and carbonic acid gas will be produced.

Secondly. It is well known, that all the calces of mercury may be reduced without any addition, and will afford oxygenous gas, but if charcoal be mixed with them, the carbonic acid gas will be formed, and the charcoal will be conſumed.

Dr. Prieſtley in oppoſition to this opinion declares, that large quantities of fixed air have been procured in his experiments, where neither charcoal nor any thing containing it was concerned.

He

He ſays, when the pureſt malleable iron is heated in dephlogiſticated air, a conſiderable quantity of fixed air is formed. He tells us, in the firſt edition of his works, that there is but a ſmall portion of fixed air, formed in this proceſs.

Four experiments were made to determine this queſtion.

Melting by the burning lens, half a drachm of the filings of bar iron, filed for the purpoſe, in twenty-four ounce meaſures of oxygenous gas, which had been well waſhed in lime water, eleven ounce meaſures of the air were imbibed by the metal, and half an ounce meaſure of carbonic acid gas was produced.

One drachm of the ſame kind of filings, melted in thirty-ſix ounce meaſures of oxygenous gas, gave one ounce meaſure; one drachm and a half, an ounce and the eighth of an ounce meaſure; and two drachms, one ounce and the ſixth part of an ounce meaſure of carbonic acid gas.

One ounce of this iron in ſmall pieces, diſſolved in the ſulphuric acid and water, left a reſiduum of one half grain of charcoal.

There was evidently then not a ſufficient quantity of coal, contained in this iron, to account for the carbonic acid produced, by melting the iron in oxygenous gas, according to this analyſis, which is certainly, very imperfect.

The inflammable air, produced by diſſolving bar iron, in diluted ſulphuric acid, holds a portion of charcoal in ſolution, which is not eaſily detected, owing to the very ſmall quantity of coal, being equally diffuſed through a large quantity of hydrogenous gas, for the portion of coal cannot be more than three grains, in three hundred and ſixty-five ounce meaſures of inflammable air.

That the carbonic acid produced in this proceſs, does actually proceed from the charcoal contained in the metal, we have the moſt concluſive proofs, for the quantity of it obtained

obtained, is always in proportion to the coal contained in iron.

Bar iron contains a ſmall quantity of coal, compared to caſt iron, and by heating caſt iron in oxygenous gas, much more carbonic acid may be produced, than from bar iron.

Dr. Prieſtley ſays, that the plumbago contained in iron, could not be diſengaged from it in this proceſs, and if it could, it would not yield the hundredth part of the fixed air that is produced.

The charcoal contained in plumbago, can certainly be diſengaged from it with the greateſt eaſe, for every particle of it, is expoſed to a high degree of heat in oxygenous gas.

Two other arguments uſed by the Doctor, to prove that fixed air may be procured without charcoal, are:

That a great quantity of this kind of air, may be produced from heating a mixture of iron filings and red precipitate, or iron filings and turbith mineral.

Five attempts were made to obtain carbonic acid gas, by expoſing from half an ounce to an ounce of red precipitate, mixed with an ounce and two ounces, of the filings of bar iron, filed for the purpoſe, to a red heat, in a clean iron tube, without ſucceſs. The mercury of the precipitate was revived, no air was obtained, and the iron was reduced to a calx.

Mixing five drachms of the ſame kind of filings, and as much turbith mineral, and expoſing the whole to a red heat, the ſame reſult happened.

Having then recourſe to caſt iron, half an ounce of red precipitate was mixed with an ounce of the borings of cannon, and thirty-two ounce meaſures of air were obtained, eleven of which were fixed, and twenty-one inflammable.

One ounce of this iron, without any red precipitate, expoſed to a red heat, gave forty ounce meaſures of air, eight of which were fixed and thirty-two inflammable.

One

One ounce of theſe borings, diſſolved in ſulphuric acid and water, left a reſiduum of thirty-four grains, eighteen of which were coal and ſixteen ſilicious earth.

The carbonic acid gas obtained in theſe experiments, evidently proceeded from the coal, contained in the caſt iron.

The Doctor alſo obtained carbonic acid, by heating the charcoal of copper in dephlogiſticated air. This charcoal of copper is made by paſſing the ſteam of alkohol over red hot copper, and as it conſiſts principally of carbon, which is one of the component parts of alkohol, no argument can be adduced from it, in ſupport of his hypotheſis.

He alſo ſuppoſes that the fixed air, procured in animal reſpiration, is formed without charcoal, but as we feed upon vegetable ſubſtances, which contain coal, the carbonic acid, thrown out from the lungs, muſt be formed of this coal, uniting to the pure air taken into this viſcus in inſpiration.

SECTION V.

Of the Nitric Acid.

It is unneceſſary to refer Dr. Prieſtley, to the experiments of various chemiſts, to prove that the nitric acid is compoſed of oxygen and azote, as he muſt be well acquainted with every thing that has been done upon this ſubject.

As the Doctor obtains this acid at pleaſure, by decompoſing by the electric ſpark, a mixture of oxygenous and hydrogenous gaſes, in the proportion of a little more than one meaſure of the former to two of the latter, he ſuppoſes the acid is formed of theſe airs. But let us attend ſtrictly, to what takes place in experiments of this kind.

Thirty-

Thirty-two ounce meaſures of oxygenous gas, obtained from red lead and the ſulphuric acid, and ſixty-four ounce meaſures of hydrogenous gas, procured from the borings of cannon and diluted ſulphuric acid, both of which had been well waſhed in lime water, were introduced into a copper tube, and decompoſed by the electric ſpark. About one ounce of water, remained in the tube, which after the exploſion, was filled with an immenſe number of fine particles of matter, and which being collected upon a filter and analyſed, turned out to be copper.

The water was of a pale blue colour, and did not turn litmus paper red. Evaporated to dryneſs, it yielded one grain and a half of the nitrate of copper.

This experiment was repeated with the ſame kind of airs, and gave the ſame reſult.

Trying the hydrogenous gas from muriatic acid and zinc, and oxygenous gas, from red lead and ſulphuric acid in the ſame proportions, no difference took place.

Increaſing the quantity of oxygenous gas to forty ounce meaſures, and reducing the hydrogenous gas to fifty-ſix ounce meaſures, and excluding the water, nitrous acid was produced.

Repeating this experiment over diſtilled water, with the ſame quantity of oxygenous gas, obtained from red precipitate, and hydrogenous gas from malleable iron and diluted ſulphuric acid, the ſame quantity of nitrous acid was produced, and no muriatic acid was formed, as appeared by the acid not precipitating a ſolution of ſilver in nitric acid.

Introducing into the tube, thirty-two ounce meaſures of azotic gas, forty of oxygenous gas, obtained from the ſulphuric acid and manganeſe, and twenty-four of hydrogenous gas, from malleable iron by the diluted ſulphuric acid, the quantity of nitric acid did not appear to be increaſed.

 Repeating

Repeating the experiment with ſixteen ounce meaſures of azotic gas, fifty-ſix of oxygenous gas from red precipitate, and twenty-four of hydrogenous gas, from malleable iron and the diluted ſulphuric acid, the greateſt quantity of nitric acid was produced.

The acid obtained in any of theſe experiments, was not equal to three grains of concentrated nitric acid, conſequently the theory of Dr. Prieſtley muſt be wrong, for it is not probable, that fifty-ſix ounce meaſures of oxygenous gas, enter into the compoſition of three grains of nitric acid.

The Doctor is certainly right when he ſays, if phlogiſticated air be purpoſely introduced into the mixture of dephlogiſticated and inflammable air, it will not be affected by the proceſs. It is neceſſary however, to have regard to the quality and proportion of the oxygenous and hydrogenous gaſes; when theſe airs are pure, and contain no azotic gas, which is ſcarcely ever the caſe, water only will be formed. When azotic air is mixed with them, which it almoſt always is, that part of the oxygen, which does not unite to the hydrogen gas and form water, joins with the azotic gas and forms the nitric acid.

When carbonated hydrogen gas is uſed, carbonic acid, water and nitric acid will be generated.

That inflammable air does not enter into the compoſition of nitric acid is evident, for none of it, nor any thing into which it enters, as a conſtituent part, can be procured from the nitric acid, nor any combination of this acid with alkalies, earths or metals.

On the other hand, nitric acid may be ſeparated into its elementary parts, oxygenous and azotic gas; and if the acid was compoſed of pure and inflammable air, it could be made by heating red precipitate in inflammable air.

Mr. Keir who analyſed the liquor obtained by Dr. Prieſtley, from the exploſion of pure and inflammable air, ſuppoſed

ſuppoſed that the muriatic acid was always generated along with the nitrous.

As no muriatic acid was obtained in my experiment, when made over diſtilled water, it is probable that Dr. Prieſtley filled his tube with pump water, containing ſea ſalt, or that the water of his hydropneumatic tub contained ſome marine acid.

I cannot conclude this diſſertation, without acknowledging my obligations to Dr. Prieſtley, for his polite attention in ſhewing me a variety of experiments, when at his houſe in Northumberland, and for the inſtruction derived from reading his very valuable diſſertations, on different kinds of air.

Although I do not agree with the Doctor, in the theory which he has adopted, yet I conceive his entrance, on that branch of philoſophy, denominated pneumatic chemiſtry, will ever be conſidered, as marking an æra in the ſcience.

No. LXXIII.

Philological View of ſome very Ancient Words in ſeveral Languages. By the Rev. NICHOLAS COLLIN, D. D. Rector of the Swediſh Churches in Pennſylvania.

Read, June 1, 1798.

A WISH to explore the obſcure ſcenes of remote ages, ariſes from good and energetic principles in our nature. Strangers upon earth, and paſſing with all mankind on that rapid ſtream, which has carried away all former generations, and ſhall ſweep off all the ſucceeding till the end of time, we muſt make many penſive inquiries on the opening and cloſe of this myſterious drama; on the characters and fortunes of the multitudes that have acted their parts, and of thoſe who ſhall finiſh the remainder; on their deſtinies under future modes of life in other regions of the vaſt univerſe! The Creator has confined our view of his deſigns within narrow limits; but this deſire of the mind to pry far beyond the ken of mortal eyes, and this ſympathy embracing the whole human race, are clear indications from him, that our intellectual powers ſhall obtain a wide and bliſsful ſphere of exertion, and that we ought to be ſatisfied with the fruits of their faithful eſſays in this world. Numerous and exact obſervations on the complicated ſyſtem of human nature are effectual means of its improvements, and afford virtuous enjoyments in this dawn of our exiſtence.

The languages of nations are fabrics raiſed from rudiments to various forms and magnitudes, far leſs by accidents, than by application of thought and ſpeech to the various and growing circumſtances of human ſocieties. The co-operation of theſe faculties is a ſpecies of common ſenſe: we often hear children aſk, *what is this, how is it called?* Many illiterate but intelligent perſons in all countries are remarkable for ſuch queſtions: in rude nations many

many examples occur of giving ſignificant names to new objects, and in difficult caſes, after mature deliberation.* To trace the early rudiments of languages is therefore important in ſeveral reſpects:—Words made for new objects, prove the previous want of them.—If their etymology can be aſcertained, it ſhows the relation of theſe objects with other previous things.—The ſimilarity and diverſity of primitive terms points out the early diſtinctions of tribes; and guards againſt the hiſtorical errors, ſo common, of tracing whole nations from the ſame ſtock, by whatever ſimilarity of languages, without diſcriminating what reſults from the mingling of different ſtocks.—Among the great part of mankind, that has neither writings, nor other monuments, a contemplation of their languages, will yet diſcover many things otherwiſe inſcrutable.—Nations that have authentic ancient records, and other monuments, will yet derive knowledge of greater antiquity from a critical ſtudy of their language, becauſe their anceſtors ſpoke on many things before they could write hiſtory, compoſe fables, or form any ſignificant and laſting ſpecimens of arts. Though languages change from various cauſes, and ſometimes from whim, yet mankind in general do not make ſudden and great alterations: old words will for a long period retain their eſſential features; and when diſmiſſed from general uſe, remain for ages in local diſtricts, or among the ſimple claſſes of ſociety: when finally loſt, they often leave kindred words

* G. H. Loſkiel relates in his hiſtory of the Evangelical Moravian Miſſion among the Indians in North America, that ſometimes a large aſſembly conſults on the moſt proper name for ſome new intereſting object: thus, f. e. they named *brown* by a word that means a medium between black and white; they called ſhoe buckles *metallic bands*. 1ſt part, 2d art.

The people of Kamtſchatca called bread *the Ruſſian root*, becauſe it was unknown to them before the arrival of that people, and they make uſe of a root, called Saranna, in lieu of it. They alſo called the Ruſſian clergyman *Bog-bog*, becauſe he often repeated *Bog*, the Ruſſian name for God. See Steller's Travels.

words behind, that convey at leaſt a part of their ſignification.

The mixture of mankind has from very early times been ſo extenſive and diverſified by migrations, conqueſts, and friendly incorporations, that languages have very general affinities in various degrees. The kindred words of many ancient families are diſperſed over diſtant countries, and not ſeldom diſguiſed by the tones and inflections of an idiom very different from their own. Therefore we cannot inveſtigate the antiquities of any nation within the limits of its own language; nor can the antiquities of mankind be ſtudied without a conſiderable knowledge of many languages. A true philologiſt is not miſled by general complexions of languages, as oriental, and occidental, maternal and filial, ancient and modern, ſavage and civilized, &c. to draw falſe lines of ſeparation; nor does he extend their cognations beyond evident marks, ſatisfied with what is known, and leaving the reſt to future diſcoveries.

Languages of all kinds are mines of human antiquities, with different but not yet aſcertained values:—Thoſe of illiterate modern nations merit great attention, not only for their own qualities and mutual affinities, but alſo for the cognation they may have with ancient and modern civilized nations: Some ſcalping heroes of America may be kinſmen of Alexander, Cæſar, and the proudeſt conquerors of Europe; as they probably are of Tamerlan and Ogus Chan; ſeveral languages of North America are more allied with the Aſiatic and European than is generally known: —The claſſical languages are edifices, whoſe groundworks were laid in a wilderneſs, on materials brought from diverſe quarries of barbarous tongues; the roots of many claſſic words may therefore grow in Tartary and Æthiopia; many etymons and coæval words may be found in the ancient European languages, and even in their modern deſcendants. The claſſics therefore do not merit the exceſ-

ſive

five praise for antiquity, so generally bestowed on them (especially on the Hebrew) but they are very valuable for their ample writings, by which their affinities with each other, and with many other languages can be known: the Greek, as both copious and ancient, is of particular importance.*—The written reliques of the Celtic, Moeso-Gothic, Teutonic, Scytho-Scandian, Anglo-Saxon are sufficiently esteemed; yet as they are all within 1600 years, and the greater part much later; and as the whole is not copious; we must not believe that they embrace all the essential words of the British, Irish, Gallic, Belgian, Cimbric, and Scandinavian languages; but that many others are contained in the printed books and living languages of Sweden, Norway and Denmark, Germany and some Swiss Cantons, Holland and the Netherlands, parts of Ireland and of Scotland, Wales, Bretagne in France, Cantabria in Spain.†—The Russian, Polish and Bohemian,

* I consider them here not as vehicles of historical and scientific erudition. Homer lived about 900 years before our æra; Herodotus, Thucydides, Plato, Aristoteles, Xenophon within the 5th and 6th centuries before it.

† The Scandinavian, Cimbric, and Islandic historical fragments, called *Sagor*, and the heroic songs, *Skaldequæden*, are generally deemed later than the 8th century, though some might have been composed much earlier. In Sweden the epitaphs on the *Runestenar* are generally estimated posterior to the fourth century: 1173 of these inscribed stones are represented in a work styled *Bautil*, published by the order of the Swedish government in 1750. The Ulphilan Gospels are commonly referred to the fourth century; but some learned philologists deem them later by 400 years. The oldest Anglo-Saxon specimens are the laws of Æthelbert, king of Kent, made between 561 and 616: the next are those of Ina, king of the West Saxons, from 712 to 727. The remains of the Swedish, Danish, and Norwegian laws are more recent; but older than those of the other northern nations. There is great reason to believe that a part of Sweden had written laws about the year 600, from the adoption of several thereof in the main body of the present general code formed seven hundred years afterwards which is mentioned in the preface to it. The ample specimens of Scandinavian and Islandic writings came in the 13th century: the celebrated northern historian Sturleson, born in Iceland, wrote then. The oldest Irish manuscripts cannot be traced beyond the 10th century: the British

Bohemian, which are the principal branches of the Sclavonian, are near relatives: they have a greater affinity with the Celtic, and ſtill more with the Teutonic claſſes, than has been ſuppoſed: they are ancient, and of great local extent.*—The Hungarian differs enough from the Sclavonic, not to be claſſed with them: it agrees leſs with the Finnic and Lapponic than is believed, and more with the Teutonic, particularly Swediſh, than has yet been obſerved: it has various and old Aſiatic relations, with other mixtures; and is in the whole very intereſting.†—The languages of the Finnic claſs are very ancient, and ſpread over a vaſt though not populous country: their relations with the Sclavonic, Teutonic, Celtic, Oriental, Tartaric, &c. are various; and what proper ſtock they may have, is not aſcertained.‡—Thoſe European languages which are

Britiſh perhaps attain the 6th: a few ſcraps of the *Bards* may neverthleſs be much older. Want of dates is a great loſs in all theſe northern monuments.

* The authors of the *Linguarum totius orbis vocabularia comparativa*, which began by order of the late Ruſſian empreſs, aſſert that the Ruſſian language is ſpoken throughout this vaſt empire, with few exceptions. I. W. Pohl author of a good Bohemian grammar in German, publiſhed 1783, and dedicated to the late emperor Joſeph, ſays in the preface of it "The Bohemian language, which is improved to greateſt perfection and purity in Bohemia, prevails not only there, and in the countries incorporated with it, Sileſia and Moravia, but extends alſo through Hungary, Poland, Sclavonia, Croatia, Dalmatia, Servia, Boſnia, Bulgaria, Moldavia, Ukraine, Moſcow, and little Tartary, Natolia, unto Armenia and Perſia." Strabo mentions *Roxolani*, which was no doubt a part of the future Ruſſian nation. The Bohemians were reſpectable enemies of the Roman empire already in the time of Auguſtus: See Vell. Paterculus, lib. ii. c. 8.—The Ruſſian Bible tranſlated from the Greek is reputed by ſome near 800 years old. Of the Poliſh a few ſpecimens are found in the hiſtory of Kadlubec, written in the tenth century, and fabulous.

† The Hungarians come from more than three ſources, as is generally ſuppoſed.

‡ The greater portion of their materials are probably contained in that of Swediſh Finland; a country nearly equal to England and Wales, with a million of people. This language is alſo beſt known by the tranſlation of the bible, the Swediſh laws and other books: both this and the Lapponic have been illuſtrated by learned Swedes, among whom are biſhop S. Juſlenius and Mr. Örling, reſpective authors of a Finnic and Lapponic Dictionary.

are commonly confidered as entirely derived from others, will be found on clofer infpection to poffefs words that are not found in thefe, and alfo roots of correfponding words in them.—Thus the whole of the Englifh cannot be accounted for from the Anglo-Saxon, Danifh, Norman, French and Britifh: the French, Italian, Spanifh and Portuguefe have relations beyond the wide circle of Latin, Teutonic, and Gothic, Greek, Hebrew, Celtic, and Arabian.—Provincial words and modes of fpeech are important, whether they be reliques of an original people, or kindred of a different language.—The jargon of the populace affords many interefting hints.—The collective ftores of ancient and modern European languages have an extenfive proportion common with many, particularly in Afia: among thefe the Perfian affinities are beft known: thofe of the Chinefe (particularly with the Scandinavians) have been hitherto beft fhown by the late Prof. *Rudbeck*, a Swede—this language, which has records beyond our æra, is very important.—A belief that the whole European ftock is Afiatic does, however, exceed our prefent knowledge.

I. *Art.* *On the Early State of Mankind.*

Some objects have fuch conftant relation to human life, that a frequent mention of them was indifpenfible in the earlieft

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Explication of the lingual marks—E. Englifh—S. Swedifh—D. Danifh—G. German—H. Holland—If. Iflandic—AS. Anglo-Saxon—Go. Gothic (meaning ancient Scandinavian) MG. Moefo-Gothic—R. Ruffian—P. Polifh—B. Bohemian—F. French—It. Italian—Sp. Spanifh—Po. Portuguefe—W. Welch—C. Cornifh—A. Armoric—Ca. Cantabrian—Ir. Irifh—F. Finnic—La. Lapponic—Hu. Hungarian—T. Turkifh—Pe. Perfian—CM. Calmuck-Mungalian—Ma. Manfuri-Tartars—Ch. Chinefe—Ja. Japonefe—Mal. Malaian—H.-Ch. common to the Hebrew and Chaldaic—Gr. Greek—L. Latin—+obfolete.

earlieſt ſociety, and that they accordingly had coæval names. In tracing theſe names through the labyrinths of languages we approach the ſacred groves that envelop the nurſeries of mankind; let us proceed unſwayed by any prejudices, guided by the principles of true philology, animated with eager curioſity, yet checked by reverential awe! If we cannot lift the veil that hides the cradles of our ſpecies, we may diſcover ſome of their infant thoughts and liſping accents!

Several eminent authors have laboured to prove that the ſynonyma in different languages for each of thoſe reſpective objects (as fire, water, hand, foot, &c.) are ſo numerous and ſimilar, as to evidence one common origin. Some have done this in the view of corroborating the Moſaic hiſtory of creation: others with a deſign to eſtabliſh a philoſophical ſyſtem of amazing extent and variety on ſimple principles of uniformity. Among the latter M. Court de Gibelin is the moſt celebrated, who in his *Monde primitif analyſé and comparé avec le monde moderne* endeavours to trace a natural hiſtory of human language, by ſhowing that it was originally a natural exertion of the reaſon and organs given to man by the Creator, and became in the proceſs of time a variety of dialects which yet preſerve moſt of their parental features.* Other philoſophers have been led by reflecting on the extreme rudeneſs of ſome ancient and modern tribes to aſſert, that mankind original-ly

* This ample work is very valuable by the great collection of words from many languages, and by the lights thrown on ſeveral important parts of human hiſtory. His candour is alſo praiſe-worthy in the very attempt of proving affinities between quite diſſimilar words. At the ſame time a critical peruſal will be a ſalutary antidote againſt this and ſimilar ſyſtems. It is alſo uſeful to remark, that his favourite idea *tout eſt un dans l'univerſe* is one of thoſe equivocal, which in minds as his aroſe from or led to the belief of one Supreme God, but in others, weak or corrupt, have foſtered the ſimple yet many-headed monſter of materialiſm, ſo prevailing in our times, and ſo near akin to atheiſm.

ly wanted articulate ſpeech, and that languages are totally artificial works like all other improvements. Among theſe ſtands conſpicuous Lord *Monboddo*, in his work on *the riſe and progreſs of language.**

A waſte of ingenious labour is a matter of ſerious regret, as it retards the progreſs of truth, cauſes great trouble both to thoſe who plant errors, and to thoſe who tear them up, and confirms the illiterate in their contempt of ſcience by the faults and diſſentions of its votaries; it is therefore neceſſary to clear all important inquiries from whatever opinions that bias the judgment, whether philoſophical or religious: In reſpectful ſympathy for theſe, which many individuals have interwoven with ſome very ſalutary truths and noble feelings of the heart, I beg leave to remark that the confuſion of tongues, which is an article of their creed, gives full permiſſion to ſeek new origins; and that mankind would now have been much better, and conſequently more happy, if theologians in general had enforced plainer texts: ſuch as *Our Father: God is not to be mocked; for whatſoever a man ſoweth that ſhall he alſo reap: God created man to be immortal.* Admiring true philoſophy, of which theology is in reality the principal branch, I obſerve that a patient collection of many and widely ſcattered facts muſt precede general theories; that we ſhould not preſume to appreciate the deſigns of God by our favourite opinions, but humbly ſeek, and for what we can find to adore.

To place the firſt theory on the moſt favourable ground, let us admit every degree of plauſible etymology, and alſo allow ſeveral words for ſome of thoſe objects, as two for

* His genius and claſſical erudition claim eſteem from thoſe who cannot approve either of his wide premiſes nor his too confined view of languages. His ſpecimens of ſavage life are very intereſting, though not warranting the inference that men have paſſed ſeveral ages with a few ſimple cries; but thoſe who with acrimony have exploded this ought to weigh the incredible things among ſo called highly civilized nations: the giddy round of ridiculous and pernicious faſhions: wars for gain, religion, liberty, &c. &c.

for the paired bodily organs on account of right and left, five for the fingers, various for fire and water becaufe of different qualities, a number for the fun and moon as objects of admiration, &c. and we fhall ftill have a long lift of totally different and unaccountable words. We cannot derive the change from an alteration, in the organs of fpeech beyond facts: nor will that apply to languages of congenial pronunciation. To charge it on the levity of mankind and other caufes further than their ufual operation, is to make arguments from mere poffibility. The numerous affinities of copious languages will conceal this original diverfity to thofe who do not compare them with critical accuracy, becaufe they arofe in a great meafure from a mixture of different materials: thus the Greek has a dozen words for *feeing*, and as many for other things by which it is kindred to many; but how could all thofe have been formed in one fociety? The Irifh abounds in fynonymas above any European language, and they are generally members of large families that have fpread through Europe and many parts of the world: it is particularly valuable for the prefervation of many radical fubftantives.

A felection of fpecimens, and reference to fources of more information is all that my limits permit.* A fhort feries of cardinal numbers not exceeding ten being a part of

* The latin numbers are omitted as readily occurring, and fo like the Greek, except, 1, 4, 5. The Teutonic branches refemble the A. S. with few exceptions: the M. G. 4 is *fidvor* and *fidur*, 6 *faihs*, 7 *fibun*, 10 *taihun*—Celtic variations are: C. *padzar* 4, *huih* 6: Ir. *dis* 2, *koraid* and *kuingir* a pair—The R. and P. vary thus: R. *odin* 1, *fem* 7: P. *piec*, 5, *dziewięć* 9.—The Perf. Turk. and Mal. are taken from the travels of *Herbert*. Prof. Thunb. Mal. differs from him only thus: *ampat* 4, *tujo* 7: the reft is immaterial. The Del. and Mahak. are in the Swedifh Indian catechifm: the Chip. and Naud. are given by *Carver:* the Chinefe is from *Duha'de*, as the moft authentic. The Cantabrian is in *Lhuyds* Archæol; but taken from *Bonav. Vulcanius*. The Choroeich is by Stralenberg.

of early language, is an important witneſs in this cauſe; eſpecially the five firſt. It is alſo the leaſt capable of original diverſity, becauſe it would have been abſurd to call any by more than one name. Gibelin does accordingly place great weight on ſome numeral coincidences in all the parts of the globe.

	H.-Ch.	G.	As.	W.	Ir.
1	אחד	εἷς	an	ŷn	aon
2	שנים	δύο	tva	doy	do
3	שלוש	τρεῖς	threo	tri	teora
4	ארבע	τέσσαρες	feover	peduar	kethra
5	חמשה	πέντε	fif	pymp	kúig
6	שש	ἕξ	ſix	xuêx	ſeiſhear
7	שבעה	ἑπτά	ſeofon	ſaith	ſheaxd
8	שמינה	ὀκτώ	eahta	ûyth	oχt
9	תשה	ἐννέα	nigen	nau	nyi
10	עשר	δέκα	theo	dêg	deix.

B.	H.	F.	Pe.	T.
1 geden,	egy,	yxi,	yeck,	beer,
2 dwa,	kettö,	kaxi,	dew,	ekee,
3 tri,	három,	kolme,	ſe,	ewch,
4 ctyry,	negy,	neljæ,	char,	dewrt,
5 pět,	öt,	wiide,	panch,	beaſh,
6 ſſeſt,	hat,	kunde,	ſheſh,	altee,
7 ſedum,	hét,	ſeitzemæ,	haſt,	yedté,
8 oſm,	nyoltz,	kæhdexæ,	haſht,	ſekez,
9 dewet,	kilentz,	yhdexæ,	no,	dockoz,
10 deſet,	tiz,	kymmene,	dah,	one.

	Mal.	Delawares.	Chippewas.	Mahakuaſs.
1	ſatu,	ciútte,	paſhik,	onſkat,
	dua,	niſſa,	ninch,	tiggene,
	tiga,	náha,	niſſou,	áche,
	enpat,	næevo,	neau,	vajéné,
5	lyma,	pareenach,	naran,	wiſck,
	nam,	ciuttas,	ningoutwaſſou,	jajáck,
	touſion,	niſſas,	ninchowaſſou,	tzadack,
	delappan,	haas,	niſſowaſſou,	tickerom,
	ſambalan,	paéſchun,	ſhongaſſou,	waderom,
	ſapola,	thæræn,	mittauſſou,	wáſha.

	Naudoweſſies.				Cantabrian.	
1	wonchaw,	6	ſhawco,	1	bat	ſey, 6
	noompaw,		ſhawcopee,		bi	ſhaſpi,
	yawmonee,		ſhahindohin,		iru	ſhorci,
	toboh,		nebochunganong,		lâu	vedracy,
	ſawbuttee,	10	wegochunganong,	5	boſt	amar, 10

Ch.

1 y,
eul,
ſan,
ſſee,
ou,
lou,
tſc,
pa,
kieou,
che,
Koroek of Kamſchatka,
1 innen,
2 niach,
3 nioch,
4 nyzacha,
5 milchin,
6 innen-milchin,

We perceive in theſe no agreements but what may be explained from the mingling of tribes. The Hebrew has only the *ſcheſs* related to the 6 in the Gr. L. Sclavonian, Celtic, and Teutonic, with all which it has ſo many other affinities: its *aechad* does very little reſemble the 1 in the Finniſh, with which it has alſo conſiderable affinity, even in conſtruction. This and the Hungarian are more related than appears from their numerals. The wide range of the *duo* would have been more remarkable if always attended by the *one* and *three*.

Water has numerous, and many quite different words. Modern Europeans are theſe:—E. and H. *water*—S. *vatn*—D. *van*—G. *waſſer*—F. *væti*—R. P. B. *woda*—It. Sp. Po. relatives

7 niachmilchin,
8 niochmilchin,
9 chonatſchihi,*
10 minegilki.

tives of *aqua*—W. *dûr*, *duvr*—C. *dour*—A. *dour* —Ir. *uiſge*—Hu. *víz*—Fr. *eau*—Is. *aa*—Lap. *kietze*—Ca. *vra*. *Modern* Aſiatic are:—*ſu*, *ſui*, *ſchui*, &c. among the Turks and ſeveral Tartar nations, to which the Chineſe *chue* may be related—Ma. *muke*—C. M. *uſu*—Ia. *mis*—Pe. *aab*—Mal. *aijer*:—in diverſe large northern diſtricts ſeveral diſtinct families with reſpective dialects; *ûthia* : *uth* : *u* : *yth*—*loo*—*kinſi* : *ſchin* : *tzyn*—*gadar*—*mimil*—*uhl* : *cu* : *kuhl*—*woe*. Modern African are:—*moihe* an extenſive Arabian with ſeveral variations—among the negroes, *nuh* —*itchi*—*inſuo*—with the Hottentots *kamma*, and others. Modern American:—in the north, *bij*—*bih*—*'mbi*—*'nbey* —*nippe*—*nibi*—*noepe*—*noop*—*umpe* : *empye*—*oneegha*—*ochneca*—*hohnekah*——*caneega*—*chabaiian*—*orenpeoc*—*ſandooſtea* : *tſandooſteek*——*awoo* : *awwa* : *auweau* : *auwen*——*okah* : *ookka* : *okaw*——*ommah*—*ammah* : *ama*—*meneh*—*wewa*—*ejau*:—in the ſouth, *atle*—*atl*—*atte*—*ael*—*ro*—*ko*—*ha*—*ig*—*unuy*—*doolah*—*touna* : *tona*.

Obſolete European words are:—As. *ea*—Ir. *an*—*ean*—C. *guaſ*—*goyf*—Ir. *dovar*—*eaſk*—*eaſkong*—*gil*—*byal*—*fual* —*beathra*—*bir*—*bior*—*oixe*—*lo*—*lûa*. Some of theſe are not referable to any of the modern; others are not to European, but Aſiatic and American.

The

* See *Voyages en Guinee*, &c. par Paul Erdman Iſert, tranſlated from the German, printed at Paris 1793. The author gives a ſmall collection of words in three languages, which, though within a circle of 20 Daniſh (about 130 Engliſh) miles, differ not leſs than the French and German. They call fire *la*—*egia*—*dio* : eye, *hinmé*—*vannua*—*onoku* : head, *ithu*—*otri* —*ota* : arm, *nindeh*—*oſu* : fiſh, *loh*—*agunnialla* : teeth, *hgennedy*—*uiſſe*—*adu* : belly, *muſſu*—vafnu—*dommé*.

See Thunbergs travels 2d vol. In a ſmall ſample are this word, numbers till 10, &c. They have a pretty ſufficient language that varies in dialects, and has curious claps or ſmacks, dental, palatine, and guttural. See alſo Kolbe, and Sparrman.

The Greek ὕδωρ, may be a relative of dûr; or more probably a compound. The Hebrew מַיִם—מֵי are cognate with מֵי—and similar Chald. and Arabic, from which several modern spring.

Names of fire are equally distinct:—modern European, E. *fire*—G. *feur*—H. *vuur*—S. *eld*: D. *ild*—R. *ogn*, *ogon*—P. *ogién*—B. *oben*—Fr. *feu*—It. *fuoco*—Sp. *fuego*—Po. *fogo*—W. C. A. *tân*—Ir. *teine*—Ca. *sua*—Hu. *tüz*—Fi. *tuli*—La. *tolle*:—modern Asiatic Turk. and Pers. *ateſch*—Ma. *tua*—Ch. *choa*: *lo*—C. M. *gall*—Ia. *fi*, *finoko*—Mal. *api*—several Tartar tribes, *od*—*ot*—*oot*—*oth*—*ott*—various nations and tribes—*datt*—*ari*—*ſchapko*—*may*—*muiga*—*milbyt*—*koth*—*till*—*ſaan*—*ſiggau*—*zzah*:—American: in the north—*tænda*—*tinda*—*tendew*—*tintewey*—scute: scutau—*ſquittah*—*kotaweh*—*cheera*—*cheela*—*ſtauw*: stauuh—*bucktouw*—*paatha*—*toatca*—*toutkah*—*loak*—*loowak*—*luwock*—*cheeſtah*—*ogeeſta*—*otſchiſta*—*uthſyſta*—*ocheelch*—*utchar*—*rau*—*oua*—*aſh*: in the south; *ouattou*—*ouapoto*—*tata*—*quetal*—*cúthal*.

European obsoletes are; W. *yvel*—As. *æled*—S. and Is. fyr. Ir. *ydh*—*aodh*—*daig*—*doigh*—*boit*—*buite*—*breo*—*ur*—*drag*—*breo*. Some of these are distinct from all the preceding.

The Hebrew אֵשׁ; Gr. πυρ; and L. *ignis*, are kindred with some of the mentioned.

I shall presently show that some of the obsoletes for water and fire which are not referable to any of the modern, have yet extensive kindred families, when they with others come under some interesting views; in mean time I remark how narrow the classic and modern European limits are for the search of primitive words; and that many of these might have been lost. The mentioned classic words were the only current ones in the respective languages: the relatives of *water* (real or apparent) engross local three-fourths of modern Europe, and a part of Asia; yet how

how numerous are the words in all theſe languages relative to water and fire! and how many have no radicals yet known in any part of the world! without inſiſting that ſuch implying neceſſary origines from fire and water, the number of thoſe which were of primitive uſe, as *rain*, a *ſpring*, &c. is ſo great as to confirm the great improbability of all languages having a common ſource. Whoever has leiſure and ability to compare the numerous words for other things, as for the principal members of the body, &c. will be the more convinced of this truth.

We can diſcover among a great part of mankind very ſcanty and rude commencements of language, marks of a very ſimple ſtate, yet ſtamped by the rational faculty: a glimpſe of this animates theſe laborious inquiries, which would otherwiſe be fatiguing dreams.

The firſt number ſeems not to have had an original abſtract ſenſe, but to have denoted *ſomething*, and been applied to all the objects which had yet no ſpecific name. It is accordingly ſtill uſed in many languages as an article: —in all the Teutonic, as: E. *a man*—G. *ein man*—S. *en man*—in the French, Spaniſh, Italian, and Portugueſe, as Fr. *un homme*—Po. *huma porta* a gate—in the Finnic, as *yxi waimo*, a woman. It has alſo a plural in ſeveral languages, of a ſimilar meaning, as E. *ones*—S. *enar*, thoſe, ſuch—Fr. *les uns*, ſome.

Several names of the ſecond number imply *addition* and *much*, as appears from their near affinity with the terms for thoſe: Go. *tæ*, too—G. *zuviel* and H. *te veel*, too much.

Several names of the third relate to words expreſſive of greatneſs and ſtrength:—שָׁלִישׁ a military chief—τρις and *ter* were uſed to expreſs the extraordinary, both ſimply and in compounds. Some of the others are alſo analogous with terms for augmenting: as the Hebrew 4 with —רָבָה to increaſe; and its 5 and 10 with Arabic words for thickneſs and conſociation.

 The

The cognation of the firſt numeral names with thoſe of the fingers is in ſeveral caſes diſcoverable; and came from the primitive mode of counting; which is alſo preſerved in ſeveral phraſes that remain both in ancient writings, and in modern languages:—πεμπάζω is to reckon on the fingers, to count, to conſider—*per digitos computare* is the Latin —Herodotus has in his Thalia επι δακτύλων, εμβαλλεςται.—Perfect knowledge of a thing is expreſſed in having it at the fingers end—Fr. *ſcavoir ſur le bout* du doit: P. *na palcach wiedziek;* and ſtupid perſons muſt ſometimes hear, *if you cannot count by the fingers, get help from the toes.**

Moſt nations have the ten cardinal numbers different, and then advance by adding the firſt and the reſt in ſucceſſion till 20, as L. *undecim,* 11; but ſome have begun the compounding from five, or ſix, &c. as appears from ſome of the given ſpecimens. This proves that mankind endeavoured to form ſignificant words in the early ſtate of language, and its progreſs bears evident marks of the ſame method. Anaylſing languages on a large and extenſive ſcale we perceive that the iſolated words bear no proportion to the kindred, and alſo that the greater part of theſe are derived; we can trace many families from totally different roots, ſee the manner of their early growth, and how they gradually entwined with numerous important objects of human life. I ſhall therefore preſent ſome ancient and intereſting words in their family connections: —*Light* has theſe relatives: fire, ſun, moon, ſtars; day, the dawn, and evening-glow; the ſky, lightning, and lucid meteors; eyes, and the human face, ſeeing; viſible, clear, bright; principal light colours; beauty of complection, eſpecially fair and ruddy; mental qualities: intellectual, as, contemplating, thinking, believing, gueſſing, and

* See Lhuyds remarks on the Cantabrian numbers.

Some modern tribes have made little progreſs in arithmetic in compariſon with other arts, as fiſhing, hunting, &c. Few among the Kamtchadeles can count to 100; the greater part reckon firſt on the fingers, then on the toes; and exclaim whither now!

and moral, particularly candour; celebrity; felicity in various forms, ſerenity, joy, gaiety, comfort; gold, ſilver, and precious ſtones; trees, flowers, and plants of analogous qualities, &c:—Ir. *ſolas* light, *ſoilear*, clear, *ſoillſighim* to ſhine; *ſuil*, eye, *ſilleadh*, aſpect; *ſolaſam* to comfort, pleaſe; *ſual*, celebrated—W. *ſylhy-ar*, to ſee clearly; C. *ſell*, look, ſight —A. ſellas, aſpect—L. and S. ſol, D. ſoel; R. *ſolnze*, P. ſlonce, B. *ſlunce*, the ſun—G. *ſelig*, H. *zalig*, S. *ſalig*, bleſſed: in modern ſenſe, eſpecially the ſouls in heaven: the German ſignifies alſo a defunct of illuſtrious memory —A. S. *ſeolfer*, +S. and D. *ſölf*; G. *ſilber*, S. *ſilver*, H. *zilver*, ſilver—Gr. Σέλας, light, ſplendor, Σελάω to ſhine; Σελένη, the moon—R. *zélen*, P. *zielén*, the green colour— F. *ſilmæ*, eye:—Gr. αυγή, light, ſplendor+eye, break of day; αυγέω to ſhine; αγαίω to wonder, envy, αγαῖος, wonderful, elegant, αγαίων the ſun:—A. S. *aegh*, M. G. *augo*, G. *auge*, H. *oog*, S. *öga*, eye; Is. *eige* to contemplate:—Ir. *grian*, *grioth*, the ſun; *grianſtad* the ſolſtice, *grionach* ſunny; *grioſaidh*, embers, +*gris* fire—S. *gry*, to dawn, *gryning*, dawning—G. *grün*, H. *groen*, S. *grön*, green— W. A. gurés, Ca. *goria*, heat; P *gorze*, R. *goriu*, B. *horim*, to burn—G. *gæhren*, to ferment:—Ir. *teine*, fire, *tinlighe*, fiery; *teinteach* lightning—A. S. *tinan*, S. *tænda*, M. S. *tandian*, A. S. *tendan*, Is. *tendra*+E. *tind*, to kindle—A. S. *twinkle*, F. *etinceler*, S. *tindra*, to twinkle—E. *tinſel*, gay trapping—F. teint, a tinge; Gr. τίνγω, L. *tingo*, to tinge —S. *tunder*, tinder—Ch. *tien*, Ia. *ten* heaven—Ch. *tan* red:—Ir. *dearg*, red, crimſon; *deargam*, to make red, bluſh, kindle+*dearc* an eye; *dearcam* to ſee—W. drŷx, a face, mirror, *edryx* to ſee—Gr. δέρκω, δερκομαι, to ſee; δέργμα aſpect; δέρκξις viſion—Ir. *drag* fire, anger; *draigeighean*, a chafing diſh; *dragbod*, fire tail, (name of the leſſer bear ſtar; *draig*, a dragon; +*dreach*, a figure, image:—P. *biali*, R. *béluji*, white: P. *palam*, B. *palim*, to burn; R. *palenie* burning—AS. *báel*, *baelfyr*, a funeral pile; S. *bål* the pile

 on

on which the bodies of great malefactors are burnt after execution—A. *paelon* a frying pan; Fr. *poële*, Po. palio, a ſtove—L. *palam*, in open light—*palleo*, *pallidus*, &c. relative of *pale*, ſignifying faint white—Gr. Απολλω and Παλλας, originally, afterwards poetically, ſun and moon—The Eſtlandians, Carelians, and Aſſani (an Aſiatic tribe) call heat *pallaw*, *palava*, *pala*—The Chickkaſas, and Choktahs in North America call the ſummer *tóme palle*: the former call warm, hot *palle*:—I.+*breo*, fire, flame; *breoghan* to bake—S. *brenna*, G. *brennen*, to burn—AS. *beorhte*, light; *beorn* a prince—MG. *bairht*+S. *biart*, bright—AS. *bredan*, S. *bråda*, to broil—W. *brydio* to heat—Gr. πρηθω to burn—W. *bore*, A. *beure*, the dawn—S. *bry* to moleſt, irritate—‡*ber*, *beer*, eye:—Ir. *daigh*, *doigh* fire, hope, truſt, opinion, conjecture+daighead to burn—S. *dag*, G. *tag*, H. *daag*, *day*; S. *dagas*, to dawn—W. *teg*, S. dägelig, handſome:—Ma. *tua*, fire; tuara ſight—P. *twarz*, B. *twár*, face; P. twarz, S. *tværs*, to ones face—W. *tunni* to ſhine—AS. *tungel* a planet, *tungla*, ſtars; *tungol-cræft* aſtronomy, magical aſtrology—S. *tungle*, the moon: yet a current word in ſeveral provinces—Ch. *toung* the eaſt—F. tunne, to know; *tunnus thæti* a miracle:—C. *miraz*, to ſee; *miras* look, aſpect: Ia. *miru* to look, gaze—L. miror, to admire, gaze—F. *miroir* a mirror—W. *mirain*, ſplendid; E. *mirth*, pleaſure, gaiety—AS. *mærlic* illuſtrious; *mærrneſſa*, enſigns—S. *mærka*, S. mercken, F. *remarquer*, to remark, obſerve:—Ch. *ſun*, a luminous object, reſpectable—Ma. *ſchun*, MG. *ſunno*; AS. and Iſl. *ſunna*, the ſun; S. *ſunnan* the ſouth; C. M. *ſuun*, F. *ſuvi*, ſummer—S. ſyn ſight, ſynas to appear—Ir. *ſona*, proſperous, bleſſed:—Ir. *meanann*, very clear—L. *mane*, break of day; *manifeſtus*, clear; *monile* a jewel—AS. *mane*, *mona*, S. *måne*, D. *maane*, H. *maan*, G. *mond*, Pe. *maue*, moon—S. *mena*,

G.

‡ The words thus marked are taken from the *Vocabularia Comparativa* above mentioned.

G. *meinen*, H. meenen, to think, mean—AS. *menas*; jewels—Fr. Ch. *mien*, countenance—*meon* the ſun among ſome of the North Americans :—AS. *ſteorra*, M. G. *ſtairn*, H. *ſterre*, G. *ſtern*, S. *ſtierna*, D. *ſtierne*; Pe. *ſtarb*, *ſter*, a ſtar—W. *yſtyried* to obſerve; Is. *ſtara*, S. *ſtira*, to ſtare —G. *ſtirn* the forehead—Gr. τείρεα ſtars—AS. *torth* ſplendid, illuſtrious; *torthe ſt-tungla*, the ſun (brighteſt planet) —Thor the celebrated northern God, whoſe name is preſerved in many things: S. *tors-månad*, January; AS. *thorsdaeg*, S. *torsdag*, G. *donners*–H. *donder-dag*, thurſday; S. *tor-ăk*, thunder, (the rattling coach of thor)—L. *torris* a fire brand, *torreo*, to burn, parch: S. *torr* dry, *torka* to dry—Fi. *pæiwæ*, La. *baiwe*, day—Gr. φοιβάω to purify and brighten; φοῖβος, ſplendid: poetically the ſun; alſo an aſtrological prophet:—Ir. +*kaiſh* an eye; *keaſam* to ſinge——S. +*gaſam*, curious; *giſſa*, to gueſs; E. *gaze* to look eagerly—Gr. καυσις, heat—Fi. kaeſi, the month of June:—*kaſwonſa* face, *katzomaan* to behold—H. Ch. קיץ ſummer; חָזָה *chaſah*, to ſee, with relatives for ſight, window, lightning, and חָזוֹן oracle, or divine viſion—in North America kindred words have a wide range:—*keeſeque*, *hkeeſque*, eye; *kiſſiqua*, *keeſhkoo*, day; *kieſuck*, *kiſhek*, heaven; *kiſchis*, *kiſcheſſu*, *keſhuſe*, *keſhow*, *keſus*, *keſis*, for ſun and moon; the laſt for both among the *Pottawatameh*—In a part of Northern Aſia ſummer is called *keza*, *kiſtet*, *kiſchtin*, and a ſtar *kieſi*, *kiſcheka*:—אור light, אור fire, flame, jewel: *urim* and *tummim*, the precious ſtones on the breaſt-plate of the Hebrew highprieſt; the laſt word has puzzled the philologers much, becauſe they tranſlated it *perfection* from a wrong derivation, it being referable to the mentioned *tome*, and the Corniſh *tomder* heat: the extenſive family of the *ur* both in the eaſt and weſt is known, as *uro*, *aurum*, &c. I only remark that the Finnic *auringo*, the ſun, is ſimilar, as the French *jour*, day:—W. *golae*, C. *golou*, A. *goulou*, light; W. *golug*, eye—+S. *gloo* to eye; *glöd* fiery coals; AS.

glovan to glow—AS. G. *gold*, S. *guld*, *gull*, gold—S. *gul*, G. *gelb*, yellow—Ir. *geal*, white; *gealac* the moon—S. *glad*, glad, *glüdias* to rejoice:—H. *ſzem* eje, relative of ſeeming—:|: *Si* eye—Is. *ſia*, S. *ſe*, G. *ſehen*, to ſee, &c. in all the Teutonic: S. *anſigte*, G. *angeſicht*, the face:—H. *nap*, ſun, day—*napue* to burn, ſcald, in the language of *Greenland*:—R. *glas*, eye, *glaju* to poliſh, brighten—W. A. Ir. *glas* green, Ir. *glaſbhán (green white)* pale; *glaſanach* the dawn—AS. *glaes*, S. *glas*, G. *gláſs*, H. *glaz*, glaſs:—Ia. *ſi*, the ſun—H. *fenni* ſplendor; *fenyöſa*, pine tree,*—S. +*fon*, fire—AS. *ſindan*, S. *ſinna*, to fine—It. F. S. *fin*, G. *fein*, F. *fine* fine, F. *fineſſe*, cunning:—F. *walkeus*, light, *walkia*, white, fire—E+*welkin*, the ſky:—B. *meſyc*, P. *mieſiac*, R. *méſiaſtch*, the moon—Ir. *maiſeach*, bright, fair, brave; *maiſeachd*, pleaſantneſs, elegance; maiſighim to adorn—AS. *leoht*, *lyht*, M. S. *liuhats*, Is. G. H. *licht*, S. *lius*, D. *lys*, Ir.+*leos*, light: S.+*lhoa*, to ſhine: AS. *lige*, *lias*, G. *lohe*, S. *lâga*, flame: Ch. *lo* fire—L. *lux*, light, with many proper and Greek relatives—W. *lheyver*, *leuyrx*, light; *lhygad*, eye—W. *lhhyad*, *lhoer*, C. *lûr*, A. *laor*, the moon—R. *lizie*, G. *anlitz*, S. *anlete*, face;—all theſe may have one ſtock, at moſt they are reducible from two:—L. *fax*, W. *fagal*, G. *fackel*, S. *facla*, a torch: L. *facies*, the face; Ir. *feacam* to behold, *feachain* a view, *feachadoir* a wizard: S. *fager*, beautiful:—Gr. αθρέω, to ſee: W. *trem*, *drem*, ſight: G. *traum*, H. *droom*, S. *dröm*, a dream—AS. *dream* *melody*, joy: E. *trim*, neat, pretty; (provincial) *trimpot*, the ſame—S. *ătră*, deſire.

Sound is another ſource of very ample derivation, both by its general property, and many variations: of names for wind, ſtorm, breeze, &c. cataracts, roar of billows, purling of brooks, &c. thunder in diverſe modes; for quadrupeds,

* The Latin *pinus* has probably this origin: its German name *tanne*, Swediſh *fur*, *far*, E. *fir*, relate to fire, light; before the uſe of candles, torches were made from it, and are yet in frequent uſe among the northern country people.

quadrupeds, birds, ſnakes, inſects expreſſive of their peculiar notes: for hearing and ear, tongue, voice, ſpeaking, calling, naming; particular modulations of the voice, as hallooing, whiſpering, whiſtling, ſinging, cries of joy and ſorrow, anger, fear, courage: terms for audible, notorious, good and bad fame, &c. In cultivated ſociety, former general words are applied to muſic, eloquence, poetry, reading, teaching; the feelings of the heart are told in congenial words, that well diſtinguiſh the tender *ſigh* from the *groan*; the ſublime and affecting voices of inanimate nature, and the melodies of birds, are marked in proper terms:—E. *peal*, a loud ſound, as of thunder, *bells*: Ir. *bella* to claſh loudly—G. *bellen* to bark—:|: *pel*, *pael*, *pal*, among thirteen Aſiatic tribes ear: Ca. and La. kindred, (Chileſe call ears *pilum*)—F. *appeller*, to call; *epeller* to ſpell: to ſpeak: AS. *ſpellian* to relate, teach; *ſpel*, fable, hiſtory, doctrine; *ſpellunge*, colloquy; *ſpel-bok*, book of homilies; *ſpelboda*, ſpeaker, ambaſſador—S. *ſpel*, G. *ſpiel*, H. *ſpeel*, any kind of muſic, alſo play, game, all with ſeveral correlates—E. *ſpell*, charm, originally incantation: —H. Ch. קול *kól*, voice, any noiſe, as thunder—Fi. *kieli*, CM. :|: *kelle*, *kill*, *keli*, tongue—T. *kulak* ear: Fi. *kuulla* to hear, *kuulkat*, hear ye—Gr. καλέω, S. *kalla*, to call, name —S. *gala* to crow, is an ancient word of a very large family: L. *gallus*, a cock; AS. *galluc* a hen; *gale* a nightingale, called in G. nachtegall, and in S. H. nearly ſo; C. M. *galo* gooſe; *gorgol* a wild cock: Is. *gale* to ſing, hollow; G. *gall*, a loud cry; S. *gäll*, clear and loud— AS. *galan*, to inchant; *galdere*, inchanter, *galdor-cræft*, ſorcery by incantation; Is. *galldur* means the ſame art, to which many other northern words relate as Ir. *gallraghad*, divination:—AS. *hlowan* to bawl, *hlowung* lowing, any vociferation; *hlyd* tumult; *hlyſt* hearing; *hlyſe* fame, *hlyſan* to celebrate: AS. *hlud*, S. *liud*, G. *laut* loud; S. *lyſna* to liſten—W. klŷſt ear, W. *klywed*, C. *klouaz*, A. *klevet*, Ir. *kluynim*, *kluiſim*, to hear—W. *klodvaur*, Ir. *cluiteach*, L. *inclytus*,

inclytus, famous—Gr. κλύω, to hear, attend, obey; κλυτος, audible, celebrated:—αυδη, voice, discourse, message; αυδάω, to cause a sound, speak: αυδήεις, loud, celebrated—Lat. *audio* to sound, hear, attend, obey:—Is. *quedia* to speak; S. *quacda* to sing:—Gr. ἠχη sound, ηχέω to resound: άκοη, ear, hearing, report, oration; ακεω, to hear, understand, be named—P. B. R. *ucho* ear—in most European languages *echo* resonance: —Gr. βράχω; to sound: S. *braka* to crash; *braeka* to bleat; *spraka* to snap as some firewood—AS. *spraekan*, G. *sprechen*, H. *spreeken* to speak: S. *språka* to chat; G. *sprache*, H. *spraak*, S. *språk* language:—Ir. *buireadh*, to bell, roar, bray —MS. *waard*, H. *woord*, AS. *word*, G. *wort*, S. *ord* a word:—E. *toll* to sound a bell—S.+*tulla* to sing: AS. *tellan*, to tell: S. *förtælia* to relate, *tælja* to number—T. and 15 Tartar tribes ‡ *till*, *tell*, *dil* tongue: S. *tal*, *speech*, *oration*, *tala* to speak:—H. *harangozas*, a sound: *harang* a bell: Go. *bark* a noise, *baren*, to hollow: S. *harugla* a species of very loud owl—AS. *hearpe*, G. harfe, Fr. *harpe*, H. *harp*, S. Po. Sp. *harpa*, a harp—Fr. *harangue*, oration: Caraib. *arianga* to speak—L. *orare* to speak, L. *auris*, G. *ohr*, H, *oor*, Fr. *oreille*, S. *öra*, Is. *eira*, AS. *eare*, ear—*organ*, *orchedter*, &c. are relatives; and probably Orpheus the celebrated *Thracian* who charmed Tartarus itself by the plaintive strains for his Eurydice.——— These facts with many more throw a light on the rudiments of early languages and manners:—as the terms for speaking were congenial with the general pronunciation, they indicate a mixture of different tribes: as φράττω, έπω, καλέω, in the Greek; *tal* and språk in the Swedish: the different qualities of the sounds express congenial mental dispositions, as lively and dull, strong and weak, polite and coarse; the rudeness of a tribe must have been the grosser, as it called its own speech, and the noises of groveling or fierce beasts by one name. In the progress of language the primæval terms for speech are accordingly either so polished as to be almost changed, or appropriated to natural

ral ſounds and to the voices of animals:—Thus W. *lolio* a relative of λαλέω means *prating*, S. *prat*; which are nevertheleſs of the reſpectable φραττω, and φραδη, knowledge, prudence.

Some forms in nature are very prominent, and alſo common to numerous objects; many of which have from this cauſe obtained ſimilar names, however different in other reſpects. Among theſe the *convex* in various modes make an ample claſs: the heavenly vault; ſwelling hills and mountains; bending valleys; bays of the ſea, coves of lakes and rivers, meanders of brooks; the heads of many trees, ſhrubs and plants, more or leſs globular, oval, conical, and the arches of their branches; fruits in general, among which elegant rounds are ſo prevalent, from the lofty cocoa-nut to the ſtately pine apple, and its humble rival the beautiful and delicious ſtrawberry; ſeveral parts of animal bodies, as the head, breaſt, belly, rounds of the arms, thighs, and legs, balls of the hands, feet, and eyes, knuckles, elbows, and knees. The following few examples are terms that imply convex, and take in parts of the human body:—W. *pêl*, C. *pellen*, A. *bul*, AS. *pil*, H. *bol*, G. *ball*, S. *băll*, F. *balle*, *boule*, Po. *bola*, L. P. *pila*, a ball—AS. Go. *bolla* a round cup, bowl—H. *bol*, S. G. *bulle*, a round loaf of bread—G. *polſter*, AS. S. *bolſter*, a bolſter—G. *beule*, S. *bălde* a boil—*bulla* a packet; hence letters, mandates, &c. as the *Pope's bull*—AS. *bolt* a houſe; *bolde* a village: H. G. S. *boll-verck*, bulwark (all from circular fortification, and alſo hilly ſituation—H. *bol*+E. *poll*, the head: this remains in *poll*-tax, *pollard*-trees, &c.—W. *bol*, Ir. *bolg*, S. *bälg*, G. *balg*, belly—AS. *bilig*, *bellows*: many Teutonic relatives for veſſels of convex ſhape:—Ir. *bor*, ſwelling, *borr* a bunch, knob—AS. G. H. S. berg, mountain, hill—AS. *berien*, G. H. *beer*, S. *bær*, berries —AS. *beorg*, G. S. *borg*, a fortified place: from which is M.G *baurgs* and E. *borough*, a town—Ca. *burrua*, the

 head:

head : *peruque*, wig, a general European word :—L. *collis*, S. *kulle*, a hill—S. P. *kula*, G. *kugel*, H. *kogel*, a ball—Ir. + *coll*, the head :—S. *kupa*, a *hilloc*—AS. *kope*, G. *kuppe*, H. *kop*, S. *kopp*, Ir. *kupa*, Fr. *coupe*, Po. *copa*, Gr. κυββα, a round cup—*cupola*, convex roof : relatives in arts, &c.—Gr. κύβη; G. *kopf*, H. *kop*, the head :—Gr. λόφος, a hill, the neck, &c.—R. *golova*, P. *glova*, B. *hlava*, the head :—Ir. *bask*, round, *bascharnte*, globular—T. ⁝ *bash*, the head :—S. G. *brink*, W. *bryn*, a hill—W. *bron*, breast; S. *bringa*, breast of animals, but in partial use for human : Ir. *broin*, belly ; *bru*, womb :—AS. *breost*, G. *brust*, S. *bröst*, H. *borst*, breast—to *burst* implies swelling—*armborst*, a species of bow, very formidable, often mentioned in ancient northern history :—AS. *eægæpl*, G. *augapfel*, H. *oogæppel*, the eye ball : *aval*, *apel*, &c. being an old word for many kinds of round fruits, and relative of L. *avellana* hazel nut :—AS. *bugen*, G. *beugen*, S. *böja*, to bend—AS. *bog*, arch, bough of a tree—W. *bûa*, Ir. *boya*, S. *băge*, H. *boog*, G. *bogen*, a bow for shooting—H. *bogt*, a gulf —S. *bog*, G. *bug*, the bow of a vessel, shoulder of animals—H. *buik*, G. *bauch*, S. *buk*, belly—AS. *earm-eln-boga*, G. *ellbogen*, H. *elleboog*, S. *armboge*, elbow—The Teutonic abounds in relatives, simple and compound :—R. *gnu* to bend ; AS. *hnigan* to nod—AS. *kneou*, Is. hnie, H. G. *knie*, S. *knæ*, Gr. γόνυ, L. *genu*, knee :—Gr. καμπτω, to bend —W. C. Ir. *cam* crooked—Ir. *camog* a bay : W. *cum* a valley—W. *cam*, A. *camet*, Ir. *keim*, a step ; *keimnyin*, to walk—AS. *cuman*, MG. *quiman*, S. *komma*, G. *kommen*, to come—It. *gamba*, F. *jambe*, leg : It. *camino*, Po. *caminho*, F. *chemin*, way, road ; It. *caminare*, to walk—AS. *hamm*, fold of the knee : G. *hamme*, F. *jambon*, a ham, gammon :—W. *guyro*, to bend : L. *gyrus*, a circle—Sp. *jarrete* the ham, F. *jarret*, fold of the knee—Hu. *jarni*, to walk, *jaras*, going—C. *garr*, leg ; garas to walk—E. *garter* is related.

The

The extent of derivation in the human body appears further in theſe examples :—names of *blood* and *red* are evident correlates in the H. Ch. דָּם, אָדוֹם—Hu. *vér*, *véres*—Ir. *cru*, *cruan* : *ſlan*, *ſlann* :*—Compound words for ſome parts ; F. *cou de pied*, (neck of the foot) the wriſt : *gras*, and, *pommeau de la jambe*, calf of the leg—The Greeks called it γαστροκνημια, (belly of the leg, before they adopted σοῦρα : The Poles and Ruſſians call it *ikra*, which alſo ſignifies the eggs in fiſh, and a ſoft ſubſtance in general. The Greeks, Romans, and Britiſh called the toes fingers of the feet, as the French, Ruſſians, Poles ſtill do.

It is alſo a remarkable fact in the hiſtory of languages, that general names were applied to parts or ſpecies, when a better diſtinction became neceſſary, from a wiſh both to preſerve old words, and to leſſen the number of new. As different portions of the people did not always adopt the improvement at once, and afterwards might apply the firſt name to different parts and objects ; and as in the mingling of tribes and languages names were ſometimes by miſtake applied to ſimilar things, or adjoining parts ; (f. e. that of thigh to leg) the proceſs of diſtinction cannot be traced without prolix inquiries in many caſes ; I ſhall therefore ſelect a few clear ſpecimens :—H. Ch. שׁוֹק denotes generally the leg, but ſometimes the whole limb above the foot to the body, though the thigh with hip and loin had a ſeparate name יָרֵךְ : יָד hand repreſents not ſeldom the whole arm, as in the odd expreſſion, *arms of his hands* (Gen. xlix. 24)—Gr. χέιρ, hand, is by ancient authors uſed for the whole arm : σκέλος, leg, frequently includes the foot—L. *pes*, foot, denotes the whole forequarter of an ox in Virgil's Georg. V. 55 :—W. *yſguidh*, C. *ſkudh*, A. *ſkoas*, ſhoulder : Ca. *eſcuas*, hand—Ca. *beſoa*, arm : Ir.+ *bos*, hand : W. *bŷs*, A. *bes*, *bis*, C. *bez*, finger—W. *koes*,

 loin,

* The Delawares in N. America call blood *moocum*, red *machkue*, *machkteu* morning and evening red, *machcumen*, to dye red.

l in, hanch : Ir. *cos*, leg, foot—Ir. *lairge*, thigh, leg; *lorga*, foot, *lorg*, a footſtep—C. *fer*, leg : Ir. *feren* thigh —W. *braix*, A. *brêx*, C. *breh*, arm : Ir.+*brak*, arm, hand :—The Poles and Ruſſians have no peculiar name for the hand, for the reſpective *ręka*, *ruka*, ſignify alſo the arm ; nor do they well diſtinguiſh this from the ſhoulder, P. *ramie*, R. *pletſcho*, meaning both :—The Germans name both the thigh and leg *ſchenckel*, though the latter is alſo called *bein* : G. *ſhinka*, H. *ſhink*, S. *ſkinka*, a gammon : AS. *ſconc*, S. *ſkank*, leg; (the modern is only vulgar for the human, but more common for that of animals, as E. ſhank—S.+*ſkunk*, a fold, *ſkunka* to limp.

It is very probable that ſome tribes had at firſt only one name for the whole limb that comprehends the loin, thigh, knee, leg, and foot, which they conſidered as a *bow*, and named it accordingly. The whole arm was viewed and called in a ſimilar manner by ſome, as appears from names of the parts, implying curvature—thus ὠλὲν means elbow, arm, and part below it : W. A. *elin*, C. *gelen*, I. *ulin*, elbow : T. *æl*, with Tartar-varieties, hand : all akin to *el* in the mentioned *elbows*. Perhaps a common name ſerved for both the upper and lower branches among ſome—ἀγκύλη, which is of a large curve-family ſignified the bend of both arm and knee—AS. *earmſcancan* meant the lower parts of the arms—Some words of the ſame root ſignify both walking, &c. and actions of the arms, as, Ir. *gabham* to go paſs, take, receive, beat : *gabhal*, travelling,+*gabhail*, *ſpoil*, booty—*gabhal* a fork : *gabhal ſhir* the groin (fork of the thighs) related to numerous Teutonic and Celtic words, as S. *gaffel*, G. *gabel* a fork for eating, ſtirring the fire, &c.)—W. *gavael*, *kymmeryd* to apprehend :—P. *bieze*, to run, MS. *byſa* to run to and fro ; S.+*böſta* to ſtir buſily : E. *buſy* and *buſineſs* imply exertion, and ſpeed.

I leave this article with a trembling glimpſe on the manners of primæval men ! reflecting on the rudeneſs of

ſavages

ſavages that ſtill occupy one-third of the globe, on the follies, vices, and crimes in modern civilization, the foibles of the beſt among us, I anxiouſly inquire, does a conſiderable portion of the human ſpecies prefer falſehood to truth, malice to goodneſs, and miſery to happineſs! or is there a divine ray in the human mind, that gradually diſſipates the twilight and fogs of morning, and a heavenly ſeed in the heart, that in its growth ſuppreſſes by degrees the weeds and thorns of vice! and changes the wild waſtes both of the earth and of human ſociety into a delightful garden! my ſoul confides in the progreſſive improvement, and final perfection, of all that ſprung from the *ſource of good*, and it abhors the doctrines of original depravity and revolving changes of good and evil! *if the infancy of of our ſpecies was ignorant and freaky*, let us hope that *the fooliſh and wicked boys of our times will be ſucceeded* by *men.*

Some of the names common to the limbs of men and beaſts ſhow the near approach of ſavage to mere animal life: ancient and modern languages have ſuch, for example, thoſe of our arms and their anteriors—H. Ch. זרוין:

arm is often uſed in this manner (as Num. vi. 19, 20. Deut. xviii. 3—βραχιων occurs likewiſe in ancient Greek for the ſhoulder of quadrupeds—our Teutonic arm is akin to the Latin *armus*, that ſignified the ſame. The fangs and clutches both of bipeds and four-footed are in Greek, Hebrew, and other languages called hands, and not only figuratively; becauſe many etymons, and many obſolete names of hand ſtill uſed for thoſe animal organs, make a primæval identity very probable, as:—S. *taſſe*, G. *tatze*, a paw—S. *taga*, to take; Gr. ταζω, to apprehend: L. *tagax*, rapacious—H. *taag*, F. *tache*, a taſk—H. *taak*, a branch: S. *tagg*, a pricket—F. *griffe*, G. *greiff*, claw of large prey-birds—S. *gripa*, D. *gribe*, G. *greifen*, AS. *gripan*, to apprehend, gripe—Ir. *griov*, hand, claw, foot—S. *grip*, a large falcon: Gr. γρυψ, L. *gryphus*, G. greiff, the gryffin:—

ſin :—E. *fang* clutch, is a relative of *finger*, which belongs to all the Teutonic, and of many others, as AS. *fangan*, S. *fănga*, to catch, captivate—Ir. *fang*, a raven; S. *făng*, a ſpecies of owl :—A. *palv*, the palm, appears related to paw; and W. *lhâu* to claw, which is with variation in the whole Teutonic. Plundering and fighting being the chief buſineſs of the hand in a ſavage ſtate, it well deſerved the ſame name with the clutches of lions and vultures; and this character is recorded in many derivative words and phraſes :—C. M. *gara*, hand: C. *gurey*, S. *giöra*, to act, do—S. *gierning*, action, ſignifies in the law aſſault : E.+ *gare* to wound—D. *kaard*, a ſword :—Pe. *daſt*, hand—S. *antaſta*, G. *betaſten*, to attack : E. *put to the teſt* is related : —C. *dorn*, hand, is the root of the tournaments ſo famous in ancient chivalry :—Ma. *gala*, hand—gallant a general term for courage :—Ir.+*frag*, hand—S. *frægd*, bravery, active talents :—AS. *ellen*, power, fortitude; *ellen-rof*, mighty, illuſtrious; *ellen-læka*, a boxer.

Nevertheleſs I cannot find any word that implies praiſe of abſolute murder; and the ferocious Scythian languages have ſome that reprobate it when committed by treachery or in cold blood. Among theſe is the AS. *nithing*, with its relatives : its meaning is well preſerved in the 12th chapter of the Swediſh criminal code, which defines and puniſhes *nidings værk*, a general term for ſeveral baſe kinds of aſſault and murder, to wit, ſecret; inſidious; on perſons incapable of defence, as minors; thoſe who are aſleep, ſwimming or bathing, &c.* Some words of barbarous origin come to ſignify true heroiſm in a civilized ſociety : thus the Swediſh *kæmpe*, figures as a hero in modern military poems, though he is a brother of the Britiſh *kampiur*, a boxer, and of all the European *champions :* the Swediſh

* *Nid-ſtang*, and *riding the ſtang*, which in ſome parts of Scotland is an infamous chaſtiſement of men who beat their wives, are mentioned by *John Callander*, Eſq. in his comment on two ancient Scottiſh poems : *the gaberlunzie man*, &c.

Swedifh *berama*, to appoint, order, is ufed only in folemn public acts, as *væl beramad Riksdag*, well ordered diet; yet it fprings from *ram*, fang of a bear or lion, and is a relative of *rama* to catch, clutch, and of the Polifh *ramie*, arm.

Art. II. *On the Early Condition of the Earth, Animals, and Vegetables.*

Many ancient words contain important records on thefe objects: I fhall fketch a few, and firft fuch as will clear up the problem, whether the water has formerly covered a greater part of the earth? for this purpofe we muft examine the names of land which are derived from water, and alfo the names of water, which imply a former greater depth or extent. Mountains, hills, woods, plains, and habitations, as villages, manors, &c. were frequently named from adjacent parts of the fea, lakes, and rivers; has the water retired from many of thefe, and how far? extenfive low lands may ftill retain the names of moraffes? Wide tracts which are but a few feet under water may fignify priftine depth? creeks, ponds, and brooks may tell that they have been bays, lakes, rivers. Thefe inquiries demand a comparifon of modern, obfolete, and local words of water, and of its various collections in the languages of feveral countries: confiderable light is alfo attainable from the appellations of aquatic animals, and vegetables; and from the proper names of lakes, rivers, iflands, &c. many of which denote water.

That part of Europe which continues a miry wafte, would if cultivated fupport millions, while millions have been deftroyed for conquefts in icy wilderneffes, in the burning climes of the Eaft and Weft Indies, and for a little more elbow room on the Rhine, whofe pure ftream has

has been for centuries tainted with human blood! but perhaps many of these moraſſes have been deep and wide haunts of ſea monſters! within a few years how many bogs that ſwallowed the unwary traveller, and poiſoned the adjacent villages, have been changed into flowery meads! the human heart will alſo be cleanſed! if ſinks of corruption are neceſſary, they will be few and narrow! the following large mire-families are near relatives of great waters:—Is. *mær*, AS. *mere*, *moor*, S. *moras*, *myra*, G. *moraſt*, H. *mæras*, F. *marais*, a moor—W. A. *môr*, Ir. *muir*, AS. *mere*, R. B. *more*, P. *morze*, G. *meer*, Fi. *meri*, L. *mare*, the ſea. The root of all is very ancient, perhaps prior to the Gr. μύρω to flow, and the lake *mæris* of Egypt. Pliny mentions *morimaruſa* as a part of the northern ſea, obſcurely known, but no doubt ſo named from freezing (Fi. *marras*, winter; P. marznąe, to congeal:—S. *moſſar*, moſſes—Gr. μαιώτις, the Mæotic lake, that communicates with the Black ſea—Hu. *motſar*, a moraſs: R. *mojos*, *motſchu*, P. *moczę* to dip, moiſten:—*Fens*, extenſive in ſome parts of ancient England, and remaining in part: the word, though Gothic, is not underſtood in a great part of Sweden; but many places there have kindred names—*Funen*, one of the Daniſh iſlands—*Sinus Venedicus* in ancient geography—L. *fons*, a ſpring:—Fi. *ſuo*, a *moor*, or moſs: S. *ſump*, G. *ſumpf*, a pool—AS. *ſeo*, the ſea: H. *zee*, G. *ſee*, S. *ſiö*, ſea, lake: Ia. *ſuiſſi*, a ſeaman. The ſame words mean both lakes and moors in ſeveral languages, which indicates that their difference was not ſtriking; as Gr. λιμνη; W. *lhynn+grelyn;* S. *træſk;* Fi. *jærſvi.* In Lapland and Finland are bodies of ſhallow water above an hundred miles in length, with numerous iſlands, ſome places of depth, and ſtored with fiſh. The fens and meers of England were formerly ſimilar: Camden deſcribes the *Wittel's mere* lake in Huntingdonſhire as ſix miles in length, and three in breadth, clear, deep, and full of fiſh.* As the ſhores of

* Britannia Antiqua. p. 500.

of these waters grow by the gradual accumulation of mud, they may finally shrink into a narrow compass, still retaining the original name, though it comes to signify what they really are; but this must not prevent our exploring the etymon: thus the large hollows in the woods of Sweden called *lăgor*, often dry, are probably relicks of lakes, and relatives of the W. *lhûx*, Ir. *loch*, names of the fine lakes in Ireland and North Britain; the rather as several marks indicate their ancient use in Scandinavia: proper names of some lakes, particularly the old *Laugur* of *Mælarn*, a lake that at stockholm opens into the Baltic, 80 miles long: the Finnish *lăki* for a bay, &c.

R. *lugia*, G. *lache*, ponds, are of the same family. As all the names for morasses are related to rivers, lakes, &c. and not seldom the same word signifies the one in one country, and the other in another, they merit consideration. Names that in modern sense mean only a brook, do not prove that it was always so, for many examples show the ancient want of distinct names: as Gr. ποταμός; W. *avon*, signify rivers of very different kinds.

Many names of meadows denote wet:—Gr. λειμών—A. *fænneck*—Ir. *leana*, (from *leann*, W. *lhyn*, liquor.)—R. *luga*: P. *laga*—G. *wiese*: *auen*:*—When the sea retires, extensive lands retain the names of shores, as the *Downs*, the *marches* in Germany and Scotland, &c. but in time these will not be intelligible without knowing obsolete names for the sea. The same applies to places in the vicinity of that, lakes, rivers:—hills in low lands frequently signify islands, as *holme*, an ancient general Teutonic, and still the common name for small islands in the Swedish lakes.†

* In some parts of Sweden large tracts of grassy shores are called *mur*, which is but *myra*, or *moor* altered by time; yet this word is a matter of wonder in those parts, where *mosse*, &c. are used for the other, and the more, because *mur* also is the common name for a wall.

† Extensive and accurate knowledge of the very numerous names for water, and its relatives would happily illustrate both this subject, and the

The analogy ſo viſible in the order of Divine Providence makes it very probable that a rude earth and barbarous men had congenial animals; and that ſome of theſe became extinct in the courſe of moral and phyſical improvement. Works of ancient naturaliſts, and popular traditions confirm this; a true philoſopher will not deem the whole fabulous, becauſe a part is extravagant. That the *hydra* in the *Lerna*-marſh had ſeven heads is leſs probable; but that monſters with more than one have exiſted is very credible to thoſe who know the double headed ſerpents of America.* The terrible venom of ſome ſerpents appears in their names—Gr. πρηστηρ; H. Ch. פתן and שרף are literally burners—H. Ch. צפע was named from its poiſonous breath —ſuch are at this time found about lake *Erie.*† All Aſia and Europe have traditions about the dragon, as a huge,

hiſtory of man. The copious derivates from different roots is a further proof that languages were formed on ſeparate grounds. The ſame ancient names for lakes, rivers, &c. in Aſia, Europe, America, indicate the early migrations of mankind. Among many ſtriking ſpecimens are theſe:—C. M. *nur*, the ſea—many lakes with names of *nor*, in Tartary, &c. from China to the Caſpian ſea, as *kirkir-nor*, *lop-arall-palcati-nor*—many lakes and rivers in Sweden, *nora*, and *nor*-ſiö—*Nore* in Scotland—Po. *nora*, an engine for drawing water:—*Tona*, water (American)—P. *tonie*, to ſink: R. *tonia*, a draught of fiſh: G. *tuncken*, to dip: S. *tong*, reed: Ir. W. *tonn*, a wave: Ir. *tonach*, waſhing; *tonuog*, a water-bird: *ton*, *tunna*, &c. a water-veſſel, in moſt European languages: Gr. θύννος, La. *tunnus*, a tunfiſh—H *tenger*, the ſea: *Don* the river *Tanais*:—C. M. *goll*, a ſtream—F. *golfe*; It. Po. *golfo*; H. *golf*, a gulph, bay—W. *golchi*, A. *gelxi*, to waſh—*Holland*, and *Holm-gărd*, ancient name for a part of Ruſſia on the Baltic—E. *holm*-oak, water oak:—R. *ſtekáio*, to flow down: *Stockholm*, means the iſſue of waters; the *Mælar* falls there through two ſtreams into a bay of the Baltic:—The name of Britain on which ſo many conjectures have been made, means ſimply an iſland; Gr. βρύω to flow: AS. Go. *brym*, the ſea, *brimflod*, a deluge: Go. *brine*, ſalt, foaming: S. *brenning*, the ſurf: Po. *brindar*, to drink; F. *abbreuver*, to give drink: *brig*, a ſea veſſel, &c.—Gr. χθών, +G. *ton*, Mal. *tanna*, land.

* That they form a ſpecies is probable from their relugar form, and the number obſerved, at leaſt ſix: I have ſeen two, one in Mr. Peale's Muſeum, the other in *Yale-College* of Connecticut.

† They blow with great force a ſubtile and nauſeous wind, which if drawn in with the breath, brings on a decline that proves mortal in a few months. *Carver's Travels*, p. 105.

huge, winged, fiery ſerpent. Its names are: Gr. δρακω, G. *drach*, H. *draak*, S. *drake*, Fr. *dragon*, R. *dracon*, W. *draig*, &c. Ia. *firio;* Ch. *lum;* which all mean fire. Its figure was alſo adopted on armorials and military ſtandards—both render its exiſtence probable.* Amphibious animals of inland waters muſt diſappear with theſe: thus tribes of waterſnakes and lizards may be gone; and the dreadful crocodile will alſo depart——Large land quadrupeds decreaſe faſt as men increaſe, becauſe they cannot hide from them nor find ſufficient food. In new countries, as great parts of America, extinctions may be recent; and conſequently many undecayed reliques may be found.

Old names for woods diſcover their former extent, and the progreſs of human ſettlements.† Names that ſignify ſpecies of trees, ſhrubs, and plants, ſhow the former places of ſuch. Vegetables of remarkable properties were generally named accordingly at an early period: in ſome caſes the knowledge of ſuch is loſt; but may be recovered by exploring the names. Reflecting from this principle on the many plants in ſeveral languages that imply qualities both for preſerving and reſtoring health, I often wiſh with a ſigh, that fanatical and inhuman medical theoriſts would conſult ſimple country people, nay ſavages! for my part I infinitely prefer the *Indian fever-buſh* to the *arſenic ague drop*, and all the chemiſtry of corroſive minerals.

It was a general and very ancient cuſtom to diſtinguiſh the ſeaſons by their influence on animals and vegetables;

* See Duhalde on the Chineſe modes——The Roman enſigns were called *draconarii* from bearing the *ferventes dracones*——Keiſler has in his travels I. vol. p. 32, copied a recorded flight of a monſtrous dragon over Lucerne in Swizerland in May 1499: *draco igneus immani ſpecie, patulis auribus, craſſidudine vituli, longitutine octo cubitorum.*

† Europe was a wilderneſs not long ago: Cæſar deſcribes the vaſt *Arduenna* in the north of Gaul, and the *Hercinian* foreſt that covered great part of Germany——Camden records that the *Andreſwald* in England had been 130 miles long, and 30 wide——Within 600 years the north and ſouth diſtricts of Sweden were called *nordan*-and *ſunnan-ſkog*; a proof that land and wood were almoſt the ſame—G. *wald*, a wood: Hu. *föld*, land: *Pole* (whence Poland) denotes many things, as hunting grounds.

comparifon of refpective words will therefore illuftrate climates and natural hiftory: thus the Poles call April *Kwiećien*, and the Swedes May *Blomfter-mănad*, month of flowers—P. *Liftopad*, B. *Lyftopad*, fall of the leaves, is the name of November—AS. *Trimilcki*, month of May, from milking the cows three times in the day, an etymon rejected by thofe who know not the rapidity of northern vegetation; *Haleg-monadh*, September, from fifhing (Hu. *Hal*, fifh.—Several North American nations call March the *Worm* month, becaufe the worms then come out from their winter retreats, May month of *Flowers*, November *Beaver*-month, becaufe the beavers begin to go into winter quarters, January the *Cold*, February the *Snow*-month.*

Languages are widely fcattered and jumbled fragments of a mirror, which when fkilfully joined and polifhed will prefent inftructive pictures of men and things in priftine times. True philology is therefore fo far from being a mere amufement, as to deferve the application of individual talents, and the cherifhing care of nations.

* Carver, p. 160. I have for twenty-eight years obferved that January is generally too cold for fnowing in the middle ftates.

N O T E S.

Left the wide fcale of this concife treatife may to fome readers appear fhowy, I fhall candidly ftate the lefs obvious means of information. The Swedifh language, known in its whole compafs of modern, obfolete, provincial, has relations of amazing extent, near with all the Teutonic, confiderable with the Celtic, Roman, Sclavonian, Hungarian, Perfian, Turkifh, and many other Afiatic, Greek, Hebrew, Arabic, &c. It has of all European been the beft illuftrated: particularly by the late Profeffor *Ihre* in his *Lexicon Svio-Gothicum.* Its affinity with the Englifh, modern and ancient is difplayed by the late Bifhop *Serenius* in his *Englifh-Swedifh* and *Swedifh-Englifh* Dictionaries, both with correfponding Latin words. A Swede has therefore fuperior advantage for general philological acquifitions. He will become intimate with the ancient Teutonics by adding to his native ftores the writings of Iflandic, Danifh, German, Dutch, Englifh, Antiquaries: among the laft the excellent work of *Hickes*, the concife Anglo-Saxon-Latin Vocabulary

bulary of *Benſon*, &c. On a ſhort acquaintance with the Celtic he perceives the groſs error of thoſe Engliſh hiſtorians who aſſerted that the modern Engliſh is a pure inheritance from their Saxon anceſtors becauſe theſe totally deſtroyed the Britons (how general and longlived it was is well proved by the Rev. *Wittacre* in his hiſtory of Mancheſter) : by attentive ſtudy he diſcovers Teutonic affinities beyond the knowledge of the beſt Celtic antiquaries, among whom excels *Lhuyd*, author of *Archæologia Britannica* ; and marks alſo the reliques of ſeveral different idioms, which guard him againſt the opinion that the anceſtry of moſt European nations had one Celtic tongue, which *Pelloutier* in his *Hiſtoire des Celtes*, *Vallancey*, author of an *Iberno-Celtic*, or *Iriſh*, *grammar*, &c. and others, have endeavoured to prove (writers nevertheleſs eſtimable). A Swede is at firſt puzzled in the Sclavonian woods ; but he ſoon finds that the Poles and Ruſſians with whom his anceſtors continually fought, are his couſins, though theſe for want of *h* ſay *Golland*, *Gamburg*, &c.

My aids in the Sclavonian have been : the above mentioned Bohemian Grammar by *Pohl*, and the New Teſtament in that language : the Ruſſian-German-French Dictionary of *Nordſtet*, publiſhed at Peterſburg 1780 : another very good, original *Latin-German* tranſlated into Ruſſian ; a New Grammar ; a few books : the Poliſh-French-German Dictionary of *Trotz*, printed at Leipſig 1764 ; another in German ; the Poliſh Bible, Telemach. The Hungarian-German Grammar of *Farkadsfalva*, printed at Vienna 1779 has been of peculiar, though not excluſive, ſervice in that langnage. In the Celtic I have had conſiderable reſources, as the Welch Bible, *Antiquities* of *Cornwall*, by *Borlas*, diverſe Britiſh, Iriſh and Erſe pieces, *Boxborn's Origines Gallicæ*, &c.——My knowledge of the Aſiatic and American is far inferior; but the ſpecimens are carefully ſelected : the Chineſe are partly in *Duhalde's* Work, and partly in *Bayer's Muſeum Sinicum*, printed 1730 : the Japaneſe and Maleſe are in *Thunberg's Travels :* the C. M. Perſian, Turkiſh, Manſhuri, and others not ſpecified, are taken from the *Vocabularia Comparativa*, and judicious Travellers, as *Strahlenberg*, *Bell*, &c. I owe the American to ſeveral authors, among whom Dr. Barton merits honourable mention, who has begun a compariſon of American with Aſiatic languages, in his New *Views of the Origin of the Tribes and Nations of America*. I choſe the H. Ch as embracing much of the Syric, Arabic, &c. the ſpecimens are found in *Simonis Lexicon Manuale Hebraicum et Chaldaicum*, improved by *Eichhorn*, and printed 1793.

The Ruſſian has beſides the Greek ſome other letters totally different from all European ; want of types for theſe obliged me to ſubſtitute ſuch Roman, as nearly convey the ſound. A ſimilar defect is the reaſon why ſome of the Poliſh *l* have not the oblique croſs-line which alters their ſound ; and why ſome of the Swediſh diphthongs have only a half circle in lieu of a whole.

The limits of this eſſay do not permit detailing the rules of pronunciation, and the changing modes of kindred words in ſeveral languages ; a touch on them would not be neceſſary for the learned, and of little uſe to others.

MEMOIR

No. LXXIV.

MEMOIR on the Extraneous Foſſils, denominated Mammoth Bones: *principally deſigned to ſhew, that they are the remains of more than one ſpecies of non-deſcript Animal. By* George Turner, Member of the A. P. S. Honorary and Correſponding Member of the Bath and Weſt of England Society, &c.

Read, July, 21ſt 1797.

THE intereſting remains which form the ſubject of this Memoir, have excited various conjectures concerning their nature and origin. By ſome they were thought to be mineral ſubſtances; and by others, animal. The latter opinion ſoon prevailed, and is now univerſally received.

But another queſtion remained to be anſwered:—To what animal, or claſs, were the bones to be aſſigned? Here was a difficulty not ſo eaſy to overcome. It engaged the attention and drew forth the labours of ſeveral eminent men. Some aſcribed them to the elephant;* others to the hippopotamus; and others, again, to ſome unknown creature, larger than either, and of the carnivorous kind.† To this animal incognitum common conſent has given the name of Mammoth.‡

Depoſits of his remains are very frequently found in Siberia and other parts of the old world. In North America

* As Sir Hans Sloane, Gmelin, Daubenton, Buffon, &c. Buffon, however, admits that they beſpeak an animal whoſe cubic volume muſt have exceeded, by five or ſix times, that of the elephant.

† Dr. Wm. Hunter. *Vide* Tranſ. Roy. Soc. vol. LVIII. p. 42: alſo "Notes on Virginia."

‡ Strahlenberg, in his Hiſtorico-Geographical Deſcription, obſerves, that the Ruſſian name is *Mammoth;* which is a corruption from *Memoth*, a word derived from the Arabic, *Mehemot*, ſignifying the ſame as the *Behemot* of Job. This word is applied to any animal of *extraordinary* bigneſs: for inſtance, *Fyhl* is the Arabic appellation for an elephant of ordinary ſize; but when of uncommon magnitude, the adjective *Mehemodi* is always added.

rica they are abundant. The countries bordering upon the Ohio and its tributary ſtreams, have already furniſhed numerous diſcoveries of the kind; and, it is ſaid, the banks of the Miſſouri, alſo, abound with them.

Nature having bleſſed our tranſmontane regions with a bountiful ſupply of ſalines, or ſprings of ſalt water; the earth there being ſoft or ſpongy and impregnated with mineral ſalts, is rendered peculiarly fit for the reception and preſervation of certain bodies which, in other places, would undergo a ſpeedy decay. Hence the profuſion of Mammoth bones beyond the mountains; while on the Atlantic ſide of them, where ſalines are ſcarce, ſuch remains have but rarely been found:—I ſpeak here comparatively.

Hitherto but few of the remains in queſtion have appeared to the ſouthward of the 36th. degree of north latitude: and hence an opinion, that the Mammoth was not an inhabitant of the warmer climates. The ingenious author of "*Notes on Virginia*" ſeems to be influenced by this belief when, alluding to ſome diſcoveries made farther ſouth, he obſerves, —"They are either ſo looſely mentioned, as to leave a doubt of the fact; ſo inaccurately deſcribed, as not to authorize the claſſing of them with the great northern bones; or ſo rare, as to found a ſuſpicion that they have been carried thither, as curioſities, from more northern regions."

Since the publication of the "Notes," however, at leaſt one additional fact has occurred, that favours the aſſigning of a *wider* range to this incognitum: for, in cutting the Santee and Cowper river canal in South-Carolina, there was lately turned up a collection of bones, anſwering by deſcription to thoſe of the Mammoth. Their number, variety, and arrangement were ſuch, as forcibly to preclude the idea of their having been "carried thither as curioſities."*

The

* Since writing this paper, ſimilar remains have been diſcovered at Wilmington and near Newbern, both in North-Carolina and without the limits above ſuggeſted for the reſidence of the Mammoth.

The late Dr. W. Hunter was the firſt to relieve the learned from an error they had long indulged. Having carefully compared a few ſpecimens of the American bones with others of the Siberian non-deſcript, and theſe again with ſimilar parts of the elephant, hippopotamus, &c. he became convinced, that the two firſt were veſtiges of one and the ſame ſpecies of animal; but differing eſſentially in ſize and form from the bones of any other at preſent known to us: that, conſequently, they were not parts of the elephant, nor of the hippopotamus; but of ſome huge carnivorous animal.*

Had the opportunities of this accurate obſerver been greater than it appears they were; or, in other words, had his materials been leſs ſcanty, he would have diſcerned the remains of a ſecond incognitum, whoſe ſtature was not, perhaps, inferior to that of the other. Theſe ſecond remains evince a member of the *herbivorous* order; and, from their extraordinary ſize, I have no heſitation in believing, that they belonged to ſome link in the chain of animal creation, which, like that of the Mammoth, has long been loſt.†

Both ſkeletons of theſe incognita being uſually embedded in company, they have hitherto been confounded together by writers, under the ſingle appellation of Mammoth bones.

The parts which more decidedly mark the remains of a ſecond animal, conſiſt, firſt, of a grinder excluſively worn by thoſe of the herbivorous or graminivorous kind; and, ſecondly, of two tuſks *(defenſes)* differently faſhioned.

Although I do not preſume to aſſert, that, contrary to the received opinion, neither of theſe tuſks belonged to the Mammoth: yet if the nature of his purſuits be conſidered, taking it for granted, as I ſhall endeavour to ſhew, that he was partly (if not wholly) carnivorous;—that there is

* Tranſ. Roy. Soc. vol. LVIII. p. 42.

† It is with reluctance, that I feel myſelf conſtrained to offer here an opinion ſo contrary to that which has been held by two ſuch able writers as Mr. Jefferſon and Mr. Pennant.

is no place for their infertion in the lower jaw, (the upper I have not feen) and that fuch tufks would appear to be incompatible with the natural purfuits of fuch a creature— can we hefitate to afcribe them to fome other animal?

I fhall confine my ideas to *two* diftinct fkeletons only; fince no difcovery has yet occurred of a third tooth, or other bone, to juftify the dividing of the tufks between a fecond and a third defcription of *incognita*. I am neither prepared to admit nor deny, that *defenfes*, fo differently fafhioned as thefe will appear, were worn by one and the fame animal: and yet, the probability is, that neither of them belonged to the Mammoth. The difference between the *defenfes* is indeed remarkable. One of them, the longer of the two, bears a near refemblance, in fize, form and fubftance, to the tufk of an elephant: the other defcribes a greater curve, and is fo flattened or compreffed on two oppofite fides, in its whole length, as to produce a greater breadth than thicknefs, in the proportion of about two parts and a half to one. The curvature inclines on the edges; that is, the tufk is bent edgewife. Both *defenfes* are good ivory.

With refpect to the teeth, all that I have feen of either kind are *dentes molares*. They unqueftionably befpeak the remains of two diftinct fpecies of non-defcript animals; the one carnivorous, or mixed; the other herbivorous, or graminivorous.

The mafticating furface of the Mammoth tooth is fet with four or five high double-coned proceffes, ftrongly coated with enamel: whereas that of the other *incognitum* is flat, nearly fmooth, and ribbed tranfverfely, fomewhat like the elephant's grinder, but lefs prominently marked. The writer has counted from fifteen to twenty of thefe tranfverfe lines on a fingle tooth of this fecond *incognitum*; while on that of the elephant, they feldom exceed half the number.

The lower jaw of the Mammoth is furniſhed with four teeth, two on each ſide; and being unaſſociated either with *inciſores* or *canini*, it may reaſonably be inferred, that this animal was of a nature not wholly carnivorous, but mixed.

Another part of what we term Mammoth remains, conſiſts of fragments of ribs of a ſingular conſtruction; being all bent on the edge. Such a form is eminently calculated for ſtrengthening a frame which, perhaps, was ordained to ſubſiſt by the deſtruction of other animals, both active and powerful.

I ſhall take the liberty to give, in this place, the ſubſtance of a few obſervations made by certain writers concerning the Mammoth ſkeleton. It may aſſiſt us in forming ſome idea of the uncommon ſtature of the animal.

In the Memoirs of the American Academy of Arts and Sciences, vol. II. part 1ſt, there is a deſcription of a tuſk found ſeveral years ago in the river Chemung, or Tioga, a branch of the Suſquehannah. It was ſix feet nine inches long, twenty-one inches around at the larger end and fifteen at the ſmaller; and was incurvated nearly into the arc of a large circle. This, however, was but a fragment; for it appeared as if the length of two or three feet had periſhed at each end.

Strahlenberg* relates, that an entire ſkeleton of the Mammoth was diſcovered in Siberia, near lake Tzana Oſero; that it meaſured thirty-ſix Ruſſian ells in length;† and ſo great was the diſtance between the oppoſite ribs, that a man ſtanding upright on the concavity of a rib, as the ſkeleton reſted on its ſide, could not quite reach the oppoſite one, though with the aid of a pretty long battle axe which he held in his

* Hiſtorico-Geographical Deſcription of the North and Eaſtern Parts of Europe and Aſia, p. 104.

† The Ruſſian ell is equal to 28$\frac{1}{10}$ inches Engliſh.

his hand. This account is given as coming from the mouth of the man himſelf, and who was one of thirty others, all eye witneſſes to the fact. Strahlenberg then obſerves, that a Doctor Meſſerſchmidt had ſeen the bones of a whole ſkeleton of a monſtrous ſize, lying in a heap in a ditch between Tomſkoi and Kaſnetſko, on the banks of the river Tomber. He next tells us what he himſelf had ſeen. He ſaw, at the city of Tumeen, a ſkull of the Mammoth, two ells and a half in length: but this the Ruſſians informed him was one of the ſmalleſt ſize. He had alſo ſeen Mammoth tuſks, each upwards of four Ruſſian ells in length, and nine inches in diameter at the thick end.

It is to be regretted, that the world has not yet been favoured with a particular and ſcientific deſcription of the whole ſkeleton of an *incognitum* ſo intereſting as the Mammoth. Both Muller and Iſbrandes Ides, indeed, have gone ſo far, as to deſcribe his ſtructure, ſize, colour, &c. But what credit can be given to ſuch idle ſtories, when Ides himſelf confeſſes, that he knew of no perſon who had ever ſeen a living Mammoth? The perſon who ſhall firſt procure the complete ſkeleton of this *incognitum*, will render,—not to his country alone, but to the world, —a moſt invaluable preſent.*

In my mind it is highly probable, that both ſpecies of *incognita* in queſtion, have long ſince periſhed. This opinion derives countenance from ſeveral diſcoveries of other foſſil bones, in Germany, in South-America, and in Virginia. We are now acquainted with the ſkeletons of five ſeveral large animals, all of whom are, at preſent, unknown:

* I have often expreſſed a belief, that whenever the entire ſkeleton ſhould be found, it would appear to have been armed with claws. I am now more confirmed in the opinion; for after this Memoir was written, the Society received a collection of the bones here treated of, and among them the os calcis, or heel bone, of a *clawed* animal.

unknown: and as two of thoſe ſkeletons* were but recently brought to light, may we not expect to be gratified, in theſe times of reſearch, with other diſcoveries of a ſimilar kind? Can we believe, then, that ſo many and ſuch ſtupendous creatures could exiſt for centuries and be concealed from the prying eye of inquiſitive man?

The benevolent perſuaſion, that no link in the chain of creation will ever be ſuffered to periſh, has induced certain authors of diſtinguiſhed merit,† to provide a reſidence for our Mammoth in the remote regions of the north. Some of the North American Indians alſo believe in the now exiſtence of this animal, and place him far beyond the lakes. But their belief reſts on mere tradition: for none of them will venture to declare they have ſeen the animal themſelves, or that their information concerning him, is drawn from any perſon who had. Their tradition is to this effect. "In ancient times," ſay they, "a herd of Mammoths "came to the Great-Bone Lick, and began a univerſal de-"ſtruction of the bears, deer, elks, buffaloes and other "animals. It ſo provoked the Great Man above to ſee the "havoc thus ſpread among creatures deſigned for the uſe "of his favourite Red Men, that he killed all the Mam-"moths except the big bull, who fled wounded beyond "the lakes, where he is living to this day."

There is little or no dependence to be placed on Indian traditions. They are ſo clouded with fable, as to obſcure any truths they may happen to contain. The above tradition, indeed, is not exactly of this deſcription, though it partakes largely of the fabulous: There is a truth in it, which my perſonal acquaintance with the Great-Bone Lick has

* The Megolicks of Paraguay: alſo certain large bones found in a nitrous cavern in Virginia, and preſented to our ſociety by its worthy Preſident.

† Pennant. Jefferſon.

has enabled me to deteƈt. As it will furniſh a corroborative preſumption, if not a proof, that the Mammoth was carnivorous, or partly ſo, at leaſt, I ſhall proceed to ſome obſervations on certain appearances at that ſaline, and which muſt have been familiar to the ſavages themſelves. —I mean colleƈtions of bones of the various animals mentioned in the tradition.

The Great-Bone Lick is a ſhallow ſtream of ſalt water flowing into the Ohio. Upon either margin of the ſtream there lies a *ſtratum*, extending a conſiderable diſtance, compoſed entirely of the bones of the buffalo and other ſmaller animals noticed in the tradition above. From the effeƈt of the mineral ſalt, theſe remains were in a ſtate of high preſervation—But, judge of my ſurprize, when attentively examining them, I diſcovered, that almoſt every bone of any length had received a fraƈture, occaſioned, moſt likely, by the teeth of the Mammoth, while in the aƈt of feeding on his prey.

It is well known that the buffalo, deer, elk and ſome other animals, are in the conſtant habit of making ſuch places their reſort; in order to drink the ſalt water and lick the impregnated earth. Now, may we not from theſe faƈts infer, that Nature had allotted to the Mammoth the beaſts of the foreſt for his food? How can we otherwiſe account for the numerous fraƈtures that every where mark theſe *ſtrata* of bones? May it not be inferred, too, that as the largeſt and ſwifteſt quadrupeds were appointed for his food, he neceſſarily was endowed with great ſtrength and aƈtivity?—that, as the immenſe volume of the creature would unfit him for courſing after his prey through thickets and woods, Nature had furniſhed him with the power of taking it by a mighty leap?—

leap?—That this power of ſpringing to a great diſtance was requiſite to the more effectual concealment of his bulky volume while lying in wait for prey? The Author of exiſtence is wiſe and juſt in all his works. He never confers an appetite without the power to gratify it.

With the agility and ferocity of the tiger; with a body of unequalled magnitude and ſtrength, it is poſſible the Mammoth may have been at once the terror of the foreſt and of man!—And may not the human race have made the extirpation of this terrific diſturber a common cauſe?

G. TURNER.

Philadelphia, July 20th, 1797.

Deſcription

No. LXXV.

Defcription of a Speedy Elevator. By the Inventor, NICHOLAS COLLIN, D. D. *with two drawings from a model, reprefenting it folded and wound up.*

Read before the Society, and the Model prefented, on the 2d December 1791; honoured with the Magellanian gold Medal in December 1795.

THE main body of the bafe is a rectangular folid floor. (Fig. F. W. in the plate.) To its corners are jointed four horizontal legs, of equal thicknefs with it, but half the length, having their nether fides even with its bottom. When the machine is ufed, thefe are difplayed fo that their ends form a rectangle; the diagonals of which may be on thofe of the main body, or vary from them in a pofition moft promotive of ftability.

The pillars A A ftand vertical on the long diameter of the bafe, equally diftant from its ends. Their feet enter into it, and are by the ftrongeft faftening incorporated with its body. Thefe pillars are pairs. Their form is a rectangular parallelipiped. Their inner fides have grooves from top to bottom: which terminate by offsets in cylindric fegments. Near the tops are central embrazures, whofe fides are fortified with iron plates that reach within the folid parts above and below. The pulleys are of metal, with fteel axes and brafs naves for eafy turning, and deep channels for fecuring the cords.

The pillars are joined by three pairs of ribs. Thefe are rectangular; wide, but comparatively thin; placed horizontally, between the tops and embrazures, about the middles, and near the feet. Their ends are clofely fitted within the pillars, and well faftened.

The piers B B are more flender than A A; with fhorter heads; but the length of their bodies is equal to the whole

whole of theſe.* They have ſimilar grooves, embrazures, pulleys, and joining ribs. Their faces are parallel rectangles. The backs of their bodies have tongues along the middles that fit the grooves of A A. Theſe are vertical rectangular ridges, and parts of the very pieces, formed by cutting down both ſides to a proper level. Theſe lower ſurfaces, being even and ſmooth, will thus move cloſe along the correſponding plain parts of AA while the tongues glide in contact with the ſides of the grooves.

The cords *a a* are well paired in length and texture. They ply over the pulleys of A A in the ſaid hollows behind the grooves; having their ends fixed under the feet of B B, and on the boxes of the windlaſs. This is well ſecured in the baſe, cloſe to and right beneath the pillars.

The cords *b b* are faſtened by one end on the heads of A A. They paſs over the pulleys of B B, and reach as far below them as *a a* reach below the pulleys of A A, which is the diſtance of theſe pulleys from the baſe. Their other ends are tacked a little above the bottoms of the piers C C.

Theſe piers are with their apparatus framed like B B; have leſs bulk and ſhorter heads. Their pulleys clear the tops of B B when the machine is down.

The cords *c c* have the ſame length with *b b*, below the pulleys of C C; plying over theſe; faſtened on the heads of B B and ſomewhat above the bottom of the pier D.

This is a ſingle piece. It has two backs to fit the grooves of C C, formed like the backs of the other pieces.

A frame is accurately fixed and poiſed on the top of D. In this the load L is placed, ſo that its centre of gravity is exactly or very nearly over the centre of the frame.

When the power begins to wind the cords *a a*, theſe raiſe the piers B B. As they riſe, their pulleys recede from the tops of A A, and by ſtretching the cords *b b* lift the piers C C. Theſe recede at the ſame time from the tops of

* I call the part about the embrazure *neck*, that above *head*, and that below *body*.

of B B, and lift the pier D. Thus while B B are wound up from the baſe to the height of the pulleys of A A, D riſes treble that height; and however great may be the number of moving pieces, it multiplies the celerity and acquired elevation of the uppermoſt by that of the firſt.

As the duration and celerity of all the movements is the ſame, the lengths of all the cords below the reſpective pulleys muſt be equal.

As the whole acquired elevation is by thoſe parts of the piers which are drawn out of their folds, theſe ought to have a very great propoition. Therefore B B reach the baſe when down; and the heads of A A are but long enough to keep them ſafe in their grooves, when drawn up. Again, as the ends of the cords *b b* will be above the baſe according to the length of the heads of A A, the heads of B B are ſhortened, and the bodies of C C are prolonged below thoſe ends, in order to ſave room, and yet afford theſe piers a ſecure depth in the grooves of B B, when drawn up. On the ſame principle the heads of C C are ſhortened, and a part of D left under the ends of *c c*. As theſe additions of ſaid pairs cannot increaſe the elevation, the cords ought to be fixed above them in order to ſhorten the bearings, and ſo far make the bodies firmer. In a longer ſeries of piers this ſhortening of the heads can only be continued to the limit of depth neceſſary to ſupport the ſtrains.

The pier D preſſes the cords *c c* by its own weight, and the load L. This preſſure cauſes an equal pulling and conſequential reſiſtance in the tops of the piers B B. The pulleys of the piers C C muſt bear this double preſſure = 2 L + 2 D. Theſe therefore preſs the cords *b b* with ſaid weight and their own = 2 L + 2 D + C C. This doubled = 4 L + 4 D + 2 C C is the preſſure on the pulleys of B B: But their heads are pulled up by L + D: The difference of theſe forces added to their own weight is the preſſure of theſe piers on the cords *a a*, $= 3\,L + 3\,D + 2\,CC + B\;B$. The power on the windlaſs muſt be equal to this.

In any ſeries the power muſt lift a weight equal to the firſt piers, double the ſecond, a id ſo forth, till the laſt pier and its load multiplied by the number of moving pieces.

The pulleys, cords, and ribs have ſome weight, and are to be counted as parts of their reſpective piers. A competent allowance is likewiſe required for the friction of the pulleys, which impedes the aſcent, though the deſcent is advantageouſly retarded by it.

The preſſure on the pulleys of the pillars A A is double the power. The ſtrain in their tops is half of the weight on the pulleys of B B. The difference of theſe forces, $= 4\,L + 4\,D + 3\,C\,C + 2\,B\,B$, added to their own weight is the preſſure on the baſe.

The ſtrain in the tops of any piers in a ſeries is equal to what the power would be, if the pair next above was the laſt. The weight on the pulleys of the ſame pair is double the ſtrain in the tops of the pair next below. The ſtrain on their feet is equal to the ſtrain in the tops of the piers two ranks below.

It is very neceſſary to compute the ſtrains and preſſures in order to ſecure all the parts, and to ſave needleſs bulk, which would be a great diſadvantage in the piers by the additional expenſe of power. The preſſure of vertical pieces by their own weight muſt be counted, though not as equal to the ſame quantity of external burden laid upon them: its operation is viſible in high maſſive beams, which bend without any load; but in ſhort though ſlender pieces it is not ſufficient to break the internal coheſion of the parts. The effect of external weight is according to its quantity, and to the height and ſlimneſs of the piers; but not in uniform proportions. Divers kinds of wood have alſo different degrees of weight, and of vertical firmneſs: ſome are both ſtiff and light to an admirable degree: piers made

made of theſe can under ſlender forms bear weights many times greater then their own. Theſe qualities are in their blended effects of different value in this machinery: the pillars are the moſt preſſed, but they cauſe no weight to the power, and therefore their bulk is the leſs detriment. B B being the heavieſt laden piers are the moſt ſolid, but they have only a ſimple moment: C C bearing leſs are lighter, but their moment is double: D has a treble moment, but the lighteſt burden, and thus the leaſt weight of its own. Theſe continually growing increments of ſolidity are neceſſary conſequences of the conſtant double bearings; but ought to be ſmall in compariſon to the preſſures thus produced, which become very great, when the load to be lifted, and the elevation are conſiderable. Lightneſs is then moſt beneficial in the upper ranks, and firmneſs in the lower, as *theſe muſt lift but thoſe be lifted* many times: accordingly different ſorts of wood may be choſen by their degrees of lightneſs and firmneſs; they being otherwiſe proper, eſpecially for cloſe and ſmooth folding.

On account of the grooves and tongues the pieces cannot have thoſe regular forms that give the greateſt ſolidity; nor can the pulleys be placed exactly over the line of central ſtrength. In practiſe theſe defects muſt not exceed neceſſary limits. Moreover, when the preſſures and ſtrains on the ſeveral parts of the pieces are eſtimated, hollows may be contrived in places that can bear it—Theſe niceties cannot be marked in a model.

When the load, the elevation, and quality of the wood are given, the lighteſt ſeries of piers is found by computing the reſults from different numbers.* A greater number muſt effect a greater proportion of the whole elevation than a ſmaller, becauſe the pillars, by becoming ſhorter, contribute leſs; this addition is a new expenſe of power. The

* In this the pairs are conſidered as one.

weight of the load is eſſential, as it muſt be multiplied by the whole number of piers; imparts the ſame moment to its own pier of competent bulk; and in conjunction with it thickens with continual increaſe all the others. On the other hand the firmneſs of piers increaſes greatly with the decreaſe of their height within certain limits. Some ſpecies of wood have alſo correſponding degrees of ſtrength. The co-operation of theſe advantages may therefore render a conſiderable number of ſhort piers light, and proportionally ſo in their reſpective multiple moments. The more numerous the piers are, the ſooner is the machine wound up, and let down, which is an advantage, ſo far as men can make greater exertions for a ſhort time.

The form of the windlaſs determines, in combination with the preceding, the ſpeed of operation, and the degree of power. It admits various modes: for example, one might be placed on either ſide of the pillars, with long handles on the winches; by which eight men can work together. This model is intended to ſhow conſiderable effects from an eaſy apparatus: accordingly two men lift another, and three tiers of piers: they are aided by a ſufficient projection of the winches beyond the ſemidiameter of the boxes: this has ſuch proportion to the height of the pulleys in the pillars, and the equal length of the cords *a a* below them, that the whole winding is done by a few turns. The dimenſions of the piers are not ſpecified, as my experiments are not ſufficient; but I eſtimate them ſo, that the elevation is at leaſt fifty feet. The power increaſes, though the velocity decreaſes by leſſening the width of the boxes; and this can be done while their length can ſo correſpond, that the rounds of the cords *a a* have ſufficient room, when the machine is wound up;

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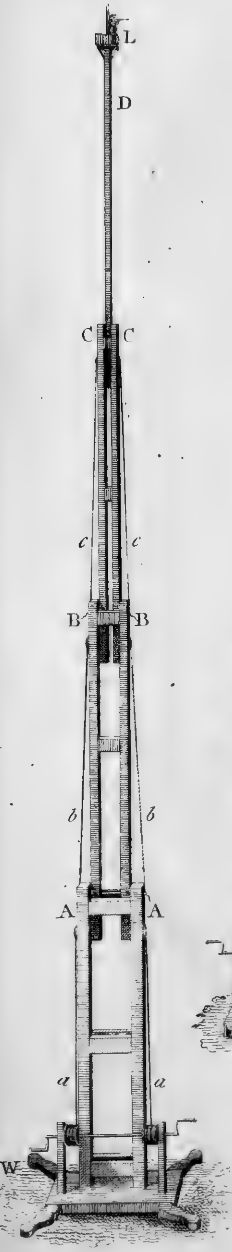
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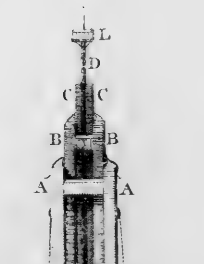
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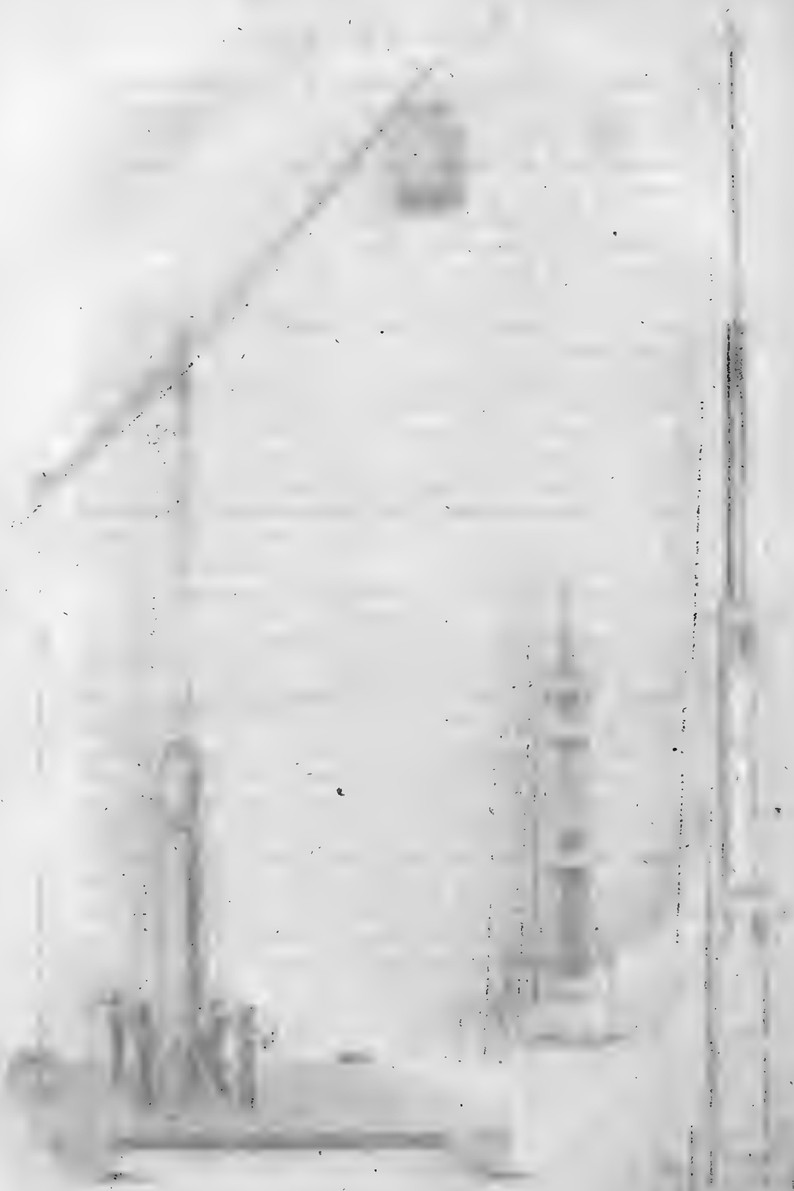

Oblique preſſures cannot ariſe from the principles of conſtruction; but happen from inevitable imperfection of materials and workmanſhip in a ſmall degree, which is not an object of exact calculation, but ſhould have full allowance for its effects on the machine. The obliquity will be the greater as the folding is ſhallow, and the fitting is looſe. The effect reſults jointly from the angle of declination, the length of the pier, and its moment of weight. The oblique bearings on the ends of the tongues, when the machine is wound up will be dangerous, if theſe have not a competent ſolidity.

All the piers with their moments of weight bear on the pillars; and the preſſure on their pulleys is the difference between double the power and the ſtrain of their heads, which balance is very great. This preſſure remains there when the machine is wound up, in every ſtage of the elevation, however great. The common centre of gravity of the pulleys thus preſſed, the pillars themſelves, and the baſe, is below the pulleys. Thus the machine has a great ſtability, and the baſe is accordingly not extenſive.

This machine combines theſe advantages: ready approach to heights otherwiſe not acceſſible without great trouble: ſpeedy aſcent and deſcent: convenient folding for keeping under cover, and for eaſy conveyance. It can be applied to ſeveral uſeful purpoſes:—Quick hoiſting and lowering of things on many occaſions; particularly ſaving of goods from upper ſtories in caſes of incend: High elevation and ſpeedy exchange of ſignals: theſe being light may be raiſed three hundred feet, and above interjacent hills: Elevation of a perſon for taking views, and quick deſcent when required; as on reconnoitring an enemy within ſhot: a machine calculated for lifting him at leaſt one hundred feet by eight men can be light enough for carrying on a waggon by two horſes.

No. LXXVI.

A Description of the Bones deposited, by the President, in the Museum of the Society, and represented in the annexed plates.* *By* C. WISTAR, M. D. Adjunct Professor of Anatomy, &c. in the University of Pennsylvania.

THE large bones are the ulna and radius of the left leg. And the plate, No. 1. contains two views of each.

The figure A exhibits the ulna with a view of its surface for articulation with the os humeri (No. 1,) connected with another smooth surface (No. 2) for supporting the upper end of the radius.

The ulna is remarkably thin for so broad a bone, being 2·8 inches in breadth,† and but 1·14 inches thick about the middle.

At the lower end is an oval surface for articulation with the carpus, about 1·8 inches in length, which is not represented in the figure. On the edge next to the radius is a protuberance (A. No. 3.—B. No. 5) which appears calculated to be received into that bone, but its surface, as well as the surface of a corresponding depression of the radius, has been so much abraded that they do not now seem calculated for articulation.

On the other edge of the bone, at the extremity, is a projection (B. No. 4.) analogous to the styloid process of the human ulna, but not proportionably long, with a smooth surface externally, about eight-tenths of an inch in length, which seems to indicate that one of the carpal bones must have lapped over, or extended beyond it.

The upper end of the radius is nearly oval, it is concave on the top for articulation with a condyle of the os humeri

* See page 246.

† The difference which may be observed between this statement and that of the President is owing to the different methods of measuring—he used a slip of paper whereas the dimensions above were taken with dividers.

meri (C. No. 1.) on one ſide of it is the ſmooth ſurface for articulation with the ulna (C. No. 2.—D. No. 4.) which is ſo ſmall that it does not appear calculated to admit much rotation, or pronation and ſupination of the paw; for the oval circumference of the upper end of the bone is 7·6 inches and this ſurface extends upon it but 1·7 Inches. It is in the ſame line of direction with the edge of the bone, and not with the flat ſide of it—When it is applied to the correſponding ſurface of the ulna the two edges of the bones are oppoſed, and as there is no rotation of the radius upon the ulna, they muſt be nearly parallel to each other, without much decuſſation, making the fore arm immenſely broad.

From this arrangment of the bones and their want of rotation and decuſſation, the palm of the paw would preſent inwards, and not downwards or backwards, unleſs the poſition of the os humeri, or the form of its lower extremity, were particularly calculated to prevent it. At the lower extremity of the radius, on the external ſurface, (C. No. 3), are ſeveral remarkable foſſæ or grooves, like thoſe on the human radius, for the tendons of the extenſor muſcles—The edge of the bone which preſents, or is oppoſed, to the ulna, becomes gradually broader as it extends to the lower extremity (D. No. 5) and there is a depreſſion in it correſponding to the protuberance of the ulna, but the ſurface is ſo abraded that no inference can be deduced from it reſpecting the connection of the bones at this place.

At the lower end of the radius is a deep oblong cavity for receiving the carpal bones, (D. No. 6), its longeſt diameter is 3·2 inches, its tranverſe is 2·37 inches, and its depth eight-tenths of an inch. When the ulna is in its natural ſituation, the cavity for receiving the carpus, formed by both bones taken together, is very near five inches in extent—the carpus was probably equally broad, and the hand or paw much broader. This breadth is not diſproportioned

to

to that of the fore arm, for when the radius and ulna are placed in their natural poſition, the breadth of the bones of the fore arm muſt be ſix inches, about the middle, and 6.48 inches, at the lower extremity.

The bones repreſented in plate No. 2, belonged to one of the paws.

The upper row conſiſts of four ſeparate pieces arranged in their natural order, one of which is ſuppoſed to belong to the metacarpus, and the other three to a claw or finger. Under the firſt bone of the row, is another of the ſame form, marked alſo No. 1, the lower bone is much ſmaller than the uppermoſt, although they appear to have joined each other in the ſame paw—At their upper extremities they reſemble metacarpal or metatarſal bones, as each of them has an articulating ſurface for connection with the carpus or tarſus, and another on each ſide for the other metacarpal bones—they alſo reſemble metacarpal bones, by approaching to the triangular form at this extremity, for the upper ſurface being broader than the lower, the ſides approach nearer below than above, and of courſe, when they are arranged in contact with each other, they form an arch, correſponding probably with the concavity of the carpus—Their lower extremities, inſtead of a round head or condyle, have a peculiar form, which the upper end of the ſmaller figure No. 1 repreſents imperfectly, for a high ridge of a ſemicircular form, and a vertical direction when the bone is in its natural poſition, projects from the articulating ſurface, and is received into a cavity of the next bone (No. 2. b)—Articulated with this end of the large bone No. 1, is No. 2, which reſembles neither the metatarſal bones nor thoſe of the phalanges, and is ſo ſhort that its length is leſs than its breadth.

The figures below, marked 2 a, 2 b, exhibit the articulating ſurfaces of this bone.

The

Engraved by James Akin, from Wash Drawings made by Mr. Titian R. Peale

Engraved by James Akin from Chalk Drawings made (the size of the Bones) by Doct.r W. S. Jacobs.

The ſurface No. 2. b. correſponds with the lower end of the metacarpal bone, having a deep groove to receive its projecting ridge, and on each ſide of the groove a ſmooth ſurface correſponding to the ſurfaces on each ſide of the ridge.

From theſe ſurfaces it appears that this bone muſt have moved conſiderably on the metacarpal bone, and that its motion was from above downwards in a circular direction.

The other ſurface of the bone No. 2. a. forms two eminences with a large depreſſion between them, which is well exhibited by the plate, and correſponds with the upper ſurface of the next bone No. 3.

The form of No. 3 is accurately repreſented in the plate, —the ſurface articulated with No. 2 has an eminence in the middle, with a depreſſion on each ſide of it; correſponding to the eminences and depreſſion of that bone,—the other extremity is flat on the ſides, and remarkably round, forming two-thirds of a circle—The articulating circular ſurface is divided by a very deep groove which is extremely narrow at the bottom to receive a ſharp ridge of the next bone.

I believe the poſition of this bone in the plate is inverted and that the upper ſide of the figure ought to be down.

The three figures, marked No. 4, will convey an accurate idea of the original ſtate of the bone of the laſt phalanx, the two largeſt bones are not entire, the bony caſe round the root of the unguis, as well as the point, being broken off, in each of them—in the lowermoſt figure both of theſe parts are entire, and from this an idea may be formed of the large claw bones before they were mutilated—The ſurface for articulating with the end of the other phalanx is beſt repreſented in the ſecond figure No. 4, the ridge which penetrates into the groove of that bone being very ſharp and deep, no motion but that of ſimple flexion and extenſion is practicable. The circular form of the ſurfaces evinces a great degree of flexion, and the claw could readily move ſo as to form a right angle with the other phalanx.

The bone repreſented by No. 5 has a ſtrong reſemblance to the metatarſal bone which ſupports the little toe, in the human ſubject—its baſe has an articulating ſurface for the tarſus—the internal ſide is ſmooth for articulation with the adjoining bone, but externally it projects outwards ſo as to reſemble greatly the bone above mentioned. The extremity connected with the toes has an oblong form, and its greateſt length is vertical, ſo as to be analogous to the ridge on the metacarpal bone No. 1. As this bone is evidently metatarſal, and very different in form and length from the others, I am induced to believe that the others are metacarpal.

From the ſhortneſs of the metacarpal bone, and the form and arrangement of the other bones of the paw, and alſo from the form of the ſolitary metatarſal bone, it ſeems probable that the animal did not walk on the toes, *it is alſo evident that the laſt phalanx was not retracted.* The particular form of No. 2, and its connection with the metatarſal bone, and with No. 3, muſt have produced a peculiar ſpecies of flexion in the toes, which, combined with the greater flexion of the laſt phalanx upon the ſecond, muſt have enabled the animal to turn the claws under the ſoal of his feet; from this view of the ſubject there ſeems to have been ſome analogy between the foot of this animal and thoſe of the bradypus—having no ſpecimens of that animal I derive this concluſion from the deſcription of its feet given by M. Daubenton.

Notwithſtanding a general reſemblance, they differ in ſome important points—In the ſloth the figure of the metacarpal bone was ſuch that M. Daubenton could not determine from it, whether the bone belonged to the metacarpus or the phalanges—but there could be no doubt as to theſe bones, for they are unequivocally metacarpal or metatarſal

metatarſal—The ſloth has but two phalanges in addition to the ſuppoſed metacarpal bone, whereas the animal in queſtion had bone No. 2 and two phalanges beſides. The relative ſize or proportions of the phalanges, muſt have differed greatly in the two animals, M. Daubenton deſcribes the firſt phalanx as very long, and the laſt, or claw bone, as very ſhort, in the ſloth, but the reverſe is the caſe with theſe bones—There is however an unguis deſcribed by M. Daubenton which is particularly intereſting, it was preſented by M. De la Condamine as belonging to a large ſpecies of ſloth, and although not entire, its length meaſured round the convexity, was half a foot, and its breadth, at the baſe, an inch and a half.

We are naturally led to inquire whether theſe bones are ſimilar to thoſe of the great ſkeleton found lately at Paraguay, but for want of a good plate, or a full deſcription we are unable at preſent to decide upon that ſubject—If however any credit be due to the repreſentation given in the Monthly Magazine for Sept. 1796 publiſhed in London, (the only plate I have ſeen) theſe bones could not have belonged to a ſkeleton of that animal—for according to that repreſentation, the lower end of the ulna is much larger, and articulated with a larger portion of the foot, in the megatherium than in the megalonix—The upper end of the radius alſo is much larger than the lower in that figure, whereas the reverſe is the caſe with the megalonix, and the difference in the claw bones is ſtill greater, as will appear to every one who compares the two.

END OF THE FOURTH VOLUME.

ERRATA IN THIS VOLUME.

P. v. l. 2 (from the bottom) for dies r. dyes.
P. xviii. l. 2 for Vaughn r. Vaughan.
P. xxii. l. 16 and 18 for Cherachie read Cerrachi.
P. xxix. l. 21 for Kananwa r. Kenhawa.
P. xxx. l. 4 for Dr. M' Kenzie r. Mr. M' Kenzie.
P. xxxii. l. 4 for *Friere* r. Freire.
Ib l. 5 (from the bottom) for fattin r. fatin paper.
P. xxvi. l. 2 for An Indian legging 'of buckfkin ornamented' r. Two Indian leggings ornamented, and add, after " quills," from a new-difcovered nation high up the Miffouri.
Ib. l. 1 (from the bottom) for Ingenhouze r. Ingenhoufz.
P. xxxiv. l. 17 for Sivermynt r. Silemynt.
P. 57. After finifhing what is there printed on *Aberation*, turn to p. 230 for the remainder.
P. 97 l. 2 (from the bottom) for fepents r. ferpents.
P. 142 l. 8 for haiving r. having.
P. 266 l. 26 for meafured, r. meafure.
P. 270 l. 18 for '*and* du feu' r. et du feu.
P. 290 l. 4 for greafey r. greafy.
Ib. l. 17 dele 'it.'
P. 230 l. 2 for No. VI. r. No. VII.
P. 254 l. 24 for fiezed r. feized.
P. 313 l. 3—p. 314 l. 21—p. 315 l. 27—and p. 350 l. 9 for refiftence r. refiftance.
P. 325 l. 7 for round r. around.
P. 362 after the title, MEMOIR ON AMPHIBIA, add thefe words, *By M. De Beauvois*.
P. 440 l. 3 for 'place the old volcanic' r. place *of*, &c.
P. 484 l. 1 of Note (from the bottom) for Stralenberg r. Strahlenberg.
P. 500 l. 1 for hanch, r. haunch.
P. 506 l. 6 (in note from the bottom) for relugar r. regular.
P. 509 l. 13 (from the bottom) for New r. *New*—l. 14 (do.) for Syrac r. Syriac.
P. 516 l. 1 (in note) for Megolicks r. Megatherium.
P. 481, note l. 7—p. 485—p. 486, laft line—p. 491 l. 14—p. 492 l. 22—p. 494 l. 12, before *valkeus*, for F. r. Fi.
P. 485—p. 494 l. 6 and 10—p. 496 l. 16 for H. r. Hu.
P. 492 l. 9 for I. r. Ir.
P. 489 l. 3 for that r. on.
P. 495 l. 16 after fpell infert Is. *fpialla*.
P. 506 for Yale-College, &c. r. Univerfity of Cambridge in Maffachufetts.

P. 56 under Conftant Log. l. 2 for 20 read *2.o*.
4 — *or* — *ov*.
6 — *10r* — *10z*.
P. 60 col. 1 the words *Multiply by* fhould be on the fame parallel with the character ——× in lines 7, 11, 15, 19, 23.
P. 66 left hand fide lines 5 for *northern* read fouthern.
7 — *fouthern* — northern. The fame on right hand fide of the argument.
—— in the middle column of figures in argument l. 2. under + infert — and under — infert +.
P. 69. line 5 for 15 read 25.
6 — C — S.

CORRIGENDA of Errors unnoticed before.

VOL. I.

Remove plate I. to page 37 of Appendix.
Page 90, line 27, for ſtatue, read ſtatute.
P. 124 l. 1 and 2 (from the bottom) for morter, r. mortar.
In the paging at top, for 217 r. 116.
P. 99 l. 2 (from the bottom) for perpendicalar r. perpendicular.
In the marginal note, p. 144. and in that of p. 146 for triming r. trimming.
P. 146 l. 9 and 12 for trimed r. trimmed.
Ibid. l. 29 for jelley r. jelly.
P. 159 l. 18 for thining r. thinning.
In note, p. 172 for vine r. wine.
P. 198 l. 17 for Antil r. Antill.
P. 250 l. 3 (from the bottom) for veneral r. venereal.
P. 259 l. 29 (in the 3d column) for kid-bean r. kidney-bean.
P. 264 l. 27 (col. 3d) for Tumerick r. Turmeric.
P. 272 l. 19 for tropies r. tropics.
P. 276 l. 16 for Auguſtine r. Auguſtan.

VOL. II.

P. 223 l. 13 for Italian r. Halleian.

VOL. III.

P. xiii. l. 11. Before *Article* insert 3.
P. xxxvi. l. 13 for Bodoin r. Bowdoin.
P. xxxviii. l. 18 and p. 222 l. 23 for dies r. dyes.
P. 35 l. 4 (from the bottom) for 1783 r. 1683.
P. 38 l. 10. *dele* the s in ‘ years.’
P. 71 l. 10 for trout r. crout.
P. 145 l. 26 for imping r. impinging.
Ib. l. 33 for overtates r. overtakes.
P. 192 l. 15 for $a = \overset{A}{\overline{8}}.\ \overset{V}{\overline{924}}\sqrt{h}$ r. $2\,a = \overset{A}{\overline{18}}.\ \overset{V}{\overline{47}}\sqrt{h}$.
P. 193 l. 14 for volicity r. velocity.
P. 218 l. 13 and p. 219 l. 26 for praires r. prairies.
P. 226 l. 20 for plow r. plough.
P. 250 l. 16 for extenling r. extending.
P. 324 l. 15 for whither r. whether.

GENERAL

NOTE; that as a second edition of Vol. I. has been published and paged differently from the first, it is necessary to remark that the following Index refers to the first edition alone.

GENERAL INDEX

TO THE FIRST FOUR VOLUMES.

A

ABERRATION of the Stars, how to calculate it, Vol. IV. p. 51.
Accents, observations on, Vol. III. page 301.
Acer saccharinum described, Vol. III. p 64—its sap a pleasant drink, 68, 69—method of making sugar from it, 67, 69—and compared with that of the West Indies, 71—virtues and various uses of the maple sugar and sap, 73. See *Sugar*.
Acid (vegetable) combined with marine salt, antiseptic virtues of, Vol. II. p. 284.
—— *(carbonic)* See *carbonic* acid.
—— *(nitric)* See *Nitric* acid.
Acids (sulphuric and muriatic) of the solution of iron in, Vol. IV. p. 457
Act incorporating the A. Philo. Soc. Vol. II. p. xi.
Advertisements of the A. P. S. Vols. I. and IV. p. iii.
Affinities of letters, Vol. III. p. 292.
Ages of persons in America and Europe compared together, Vol. III. p. 43, 54, 56.
—— remarkable of certain persons, Vol III. p. 44.
Agricultural essays, Vol. I. p. 118 to 224, 241, 255.
Agriculture the source of American prosperity, Vol. III. p. vii.
——— certain pursuits in, recommended, Vol. III. p. viii.
Air lighter than smoke, Vol II. p. 2—how best conducted into chimnies, 8—advisable way of admitting it into rooms, 9—vulgar prejudices against, 21—whether moist or dry, its free circulation recommended, 21—night air not unwholesome, ibid.—unhealthy only when charged with putrid vapours, 22—curious current of in chimnies explained, and its uses suggested, 30—proved to be more humid in France and England than America, 53 to 55—purified by the willow tree, 209—resistence of, 296—its capability of extreme density, Vol. III. p. 64—on the evaporation of, 126—how affected by heat, Vol. I. p. 273—application of a machine for preserving its due temperature in hospitals, &c. 289—the seclusion of *air* requisite in preserving wine and cider, 184—Priestley's experiments and observations relating

relating to the analyfis of atmofpherical air, Vol. IV. p. 1 to 11—experiments on the generation of air from water, Ibid. to p. 20.
—— not lefs pure in towns than country, Vol. IV. p. 264.
—— (fea and land) experiments on, Vol. IV. p. 262, 266, 269.
—— *(rarified)* explained, Vol. II. p. 2.
—— *(electric)* its capacity leffened by condenfation, but increafed by heat, Vol. II. p. 79.
Air (phlogifticated). See *Phlogifticated.*
—— *(dephlogifticated).* See *Dephlogifticated.*
—— *(foul)* how to expel it from wells, Vol. III. p. 324.
—— *(fea)* thermometrical ftate of, Vol. III. p. 90, 200.
—— *(fixed)* See *Carbonic* acid.
—— *(atmofpherical)* experiments and obfervations relating to the analyfis of, Vol. IV. p. 1.
—— *(nitrous).* See *Nitrous.*
—— *(inflammable).* See *Inflammable.*
Airs, Experiments and obfervations on various, Vol. IV. p. 1 to 11, 128, 417, 452. See the *Gafes.*
Aldebaran (occultation of*)* as obferved in the city of Wafhington in 1793, Vol. IV. p. 48.
Allegany mountain (weft of) the ftrata of ftone lie horizontally, Vol. IV. p. 227.
———— barometrical meafurement of it, Vol. IV. p. 216.
—— and Ohio rivers, &c. their fogs remarkably denfe but not unwholefome, Vol. IV. p. 225.
Alphabet, how to form a univerfal one, Vol. III. p. 263—the imperfection of exifting alphabets, 263, 266—a particular one conftructed and recommended, 277.
Alder tree, agreeable food for filk worms, Vol. I. p. 226.
America (North) qualities of its winds, Vol. I. *Preface* p. xviii.—compared in places with China, Kamtfchatka, &c. ibid.—obfervations on its vegetable kingdom, *pref.* ibid. p. 250—its grapes 192—its minerals, &c. *pref.* xxii. p. 251—the three bread colonies become very dry, 203—its general climate proper for the grape and making of wine, 117, 120—directions for the culture of the grape, 157—medicinal productions of 235, 239, 250, 257, 258, 260, 262, 263, 264, 265, 266. [See *Medicinal Productions*] change of climate in the middle colonies, 272—Indian population below the falls, Vol. IV. p. 252.
—— antiquities of, Vol. III. p. 214—Vol. IV. p. 179, 181, 185, 196, 198, 203.
——, memoir on the difcovery of, Vol. II. p. 263.
—— bodily diforders common in, Vol. III. p. iii.—on the duration of human life in, and the population of, 25—two births to one death there, 38—inftances of longevity, 44—obfervations on the Weftern country of, 214—and on certain plants, and animals, 241.
American Philofoph. Society, its rules, Vols. I and IV. p. iii—laws and regulations, Vols. I and II. p. v. Vol. IV. p. iii—names of officers and members, Vol. I. p. xiii—Vol. II. p. xxiii—Vol. III. p. xxviii—Vol. IV. p. xiii—objects of the inftitution, *pref.* xiv.—act of incorporation,
Vol.

Vol. II. p. xi—depofits and annual fubfcriptions of its members increafed by law, xvii—extracts concerning Magellan's donation, xix and Vol. IV. p. vii—donations received, Vol. II. p. 398—Vol. III. p. 351—Vol. IV. p. xvii—meafures for improving inland navigation, Vol. II. p. 293, 295—reports made thereon, 294-5-6—premiums offered from out of the fociety's funds, Vol. IV. p. iv.—circular letter of the permanent committee for inquiring into the antiquities, &c. of North America, p. xxxvii.

American Grapes, obfervations on, Vol. I. p. 192—and on filk worms, p. 224.

American Indians long lived, Vol. III. p. 50—their ancient works, ib. p. 214—Vol. IV. p. 179, 181—their traditions, Vol. III. p. 216, 220 Vol. IV. p. 207, 209, 516—their dyes, Vol. III. p. 222—languages, cuftoms, manners and genius, ib. p. 220—fome account of former tribes, Vol. IV. p. 182.

Americans, their popular diet occafions certain difeafes, Vol. III. p. vi.

Ammoniacal gas, a portion of it in the atmofphere, Vol. IV. p. 128.

Amphibiæ, memoir on, Vol IV. p. 362.

Amfterdam, proportion of deaths there, Vol. III. p. 41, 59.

Analyfis of the chalybeate waters of Briftol (Penn.) Vol. I. p. 303—their virtues, 306, 310, 313.

—— *of atmofpherical air*, experiments and obfervations relating thereto, Vol. IV. p. 1.

Anatomy, its utility, Vol. II. p. 366.

Anatomical preparations, the art of making them by injection and corrofion, Vol. II. p. 366—how prepared with wax, 381.

Anchor (floating) defcribed and propofed to be ufed on a lee fhore, Vol. II. p. 311.

Anderfon (Dr. James) his difquifition on wool-bearing animals, Vol IV. p. 149—his communication refpecting a fheep brought from Jamaica, ib. p. 153.

Anecdote of a Roman cultivator of the grape, Vol. I. p. 121.

Animal remains difcovered. See *Bones,—Mammoth,—Megatherium.*

Animals larger in the new than the old world, Vol. IV. p. 258.

—— on the early condition of, Vol IV p. 503.

Animate bodies counteract atmofpheric heat, Vol. II. p. 143.

Anomaly (eccentric) of the planets, mode of calculating it, Vol. IV. p. 67.

Antill, Efq (Edwd) his effay on the cultivation of the grape, and the making and preferving of wine in N. America, Vol. I. p. 117—his method of curing grapes for raifins, 193—of curing figs, 198—his obfervations on the raifing and dreffing of hemp, ib

Antiquities of America (Indian) Vol. III. p. 214—Vol. IV. p. 179, 181, 184, 198.

Apparatus defcribed for exhibiting the operations of fmoke and rarefied air, Vol. II. p. 22.

Apparent diameter of an object, how to find it, Vol. I. p. 98—curious experiments attending it, ib.

Appendix to aſtronomical and mathematical papers, Vol. I. commencing after p. 116, to p. 72 of this appendix.
Apis Mellifica, or honey-bee, whether a native of America? Vol. III. p. 241.
Architects erroneous in their conſtruction of fire places, Vol. II. p. 10.
Arcturus ſtar, his great celerity of motion weſtward, Vol. II. p. 224.
Arſenic, an infallible teſt for detecting its preſence, Vol. II. p. 214—the baſis of Martin's cancer-powder, ib.—its efficacy in ulcerous caſes, 215.
Aſia (eaſtern ſide of) like the ſame latitudes in North America, Vol. I. *pref*. iii.
Aſtronomical and mathematical papers, Vol. I. from p. 1. to 116, with an appendix following.
——— calculations and corrections of, Vol. I. p. 39, 102.
——— inſtruments uſed in obſerving the tranſit of Venus at different places in 1769, Vol. I. p. 10, 13, 97, 106, 110, 114.
——— obſervations, Vol. II. p. 260—Vol. III. p. 150, 261—Vol. IV. p. 21, 32, 67.
Atmoſphere, Venus ſurrounded by a denſe one, Vol. I. p. 49—prodigious height of the Cometarian, and its ſuppoſed uſes, appendix p. 35.
——— conſidered as to heat, Vol. I. p. 278—more moiſt in England and France than America, Vol. II. p. 53 to 55—denſity of it, 96—very moiſt and denſe at the lakes, Vol. IV. p. 225—and charged with heavy but innoxious dews in interior America, ib.—queries as to its electrical ſtate, Vol. I. p. 100—of Philadelphia, exceſſive heat of in 1750, with a curious fact from Dr. Franklin, 143—component parts of, Vol. IV. p. 266, 327, 417—changes in peculiar to America, 217.
——— (a *ſalubrious*) how compounded, Vol. IV. p. 128, 417.
——— at ſea, the ſtate of it, Vol. II. p. 90, 200—experiments on it, Vol. IV. p. 262, 269, 272—more pure than land air, 269.
——— of marſhes, experiments and obſervations, Vol. IV. p. 417.
Atmoſpheric heat counteracted by animate bodies, Vol. II. p. 143.
Atmoſpherical air, Prieſtley's experiments and obſervations on the analyſis of, Vol. IV. p. 1 to 11—whence compounded, 17—417, 424. See *Air*.
Attendant ſtars, numbers of diſcovered by Mayer, Vol. II. p. 222—uſeful for determining the motion of fixed ſtars, ib.
Attrition, why it cauſes heat, Vol. I. appendix, p. 32.
Aurora boreales, queries concerning, Vol. II. p. 100—an uncommon one, 136—always ſucceeded by a fall of the barometer, and generally by rain, 142—account of one, Vol. III. p. 62.
——— obſervations on, Vol. II. p. 196.
Axis of the earth, how to calculate its nutation, Vol. IV. p. 51.
Azote, or phlogiſticated air, proportion between it and oxygen, as ſtated by ſome, Vol. IV. p. 1—not a ſimple ſubſtance—experiments and obſervations on, ib. p. 7, 11 and 382.
———, or nitrogen gas, what proportion of it in the atmoſphere, Vol. IV. p. 128, 417.
Azotic gas, a component part of the atmoſphere, Vol. IV. p. 266, 417.

B

Bailey (Joel) and others, their account of Venus's tranſit over the ſun in 1769, Vol. I. p. 89.
Bark (red) hiſtory of, Vol. II. p. 289.
Barker's mill improved, Vol. III. p. 144.
——— as improved by James Rumſey, inveſtigated as to its powers, p. 185.
Barometrical meaſurement of the Blue Ridge, Warm Spring, and Allegheny mountains in Virginia, Vol. IV. p. 216.
Barton (Wm.) on the duration of human life, and the progreſſive population of the United States, Vol. III. p. 25—his poſtſcript to the foregoing, 134.
—— *(Dr. Benj. Smith)* on the bite of the rattle ſnake, and the cure, Vol. III. p. 100—his inquiry, whether the true honey-bee is a native of America? 241—his botanical deſcription of the podophyllum diphyllum of Linnæus, 334—his memoir concerning the faſcinating faculty which has been aſcribed to the rattle ſnake and other American ſerpents, Vol. IV. p. 74—his account of an American ſpecies of dipus, or jerboa, ib. p. 114—his obſervations and conjectures concerning certain articles which were taken out of an ancient tumulus or grave, at Cincinnati, in the county of Hamilton, and Territory of the U. S. north-weſt of the river Ohio, p. 181—his hints relative to the ſtimulant effects of camphor on vegetables, p. 232.
Bartram (Moſes) his obſervations on the native ſilk-worm of N. America, Vol. I. p. 244.
——— *(Iſaac)* his memoir on the diſtillation of perſimons, Vol. I. p. 231,
Baſaltes (cryſtallized) found in Pennſylvania, Vol. IV. p. 445.
Battle of the Kegs. See *Submarine veſſel.*
Bays of Spirito Sancto (E. F.) of Mobile, St. Joſeph, Penſacola, Sta. Roſa, Spirito Sancto and St. Andrew (W. F.) deſcribed, Vol. I. p. 253.
Bee (the honey) obſervations on, Vol. III. p. 241—various ſpecies of, 245, originally from England, 256.
——*hive*, but one in Weſt-Florida, Vol. III. p. 249.
Bean, a remarkable one uſed in the improvement of lands, Vol. III. p 226.
Bear, a deſcription from Indian tradition of a ſuppoſed huge terrific one, called the big naked bear, Vol. IV. p. 260.
Beauvois (De) his obſervations on plants called cryptogamic, Vol. III. p. 202.—His memoir on the ſubject of a new plant growing in Pennſylvania, Vol. IV. p. 173.—His memoir on a new ſpecies of ſiren, 277.—His memoir on amphibia, 362.
Beccaria and *Prieſtley*, their ideas concerning electricity, Vol. II. p. 76.
Beer (excellent) of perſimons, Vol. I. p. 234.
Belknap, (Jeremy) his deſcription of the white mountains in N. Hampſhire, Vol. II. p. 42—his obſervations on the aurora borealis, as accompanied with a faint ruſtling noiſe, 196—his method to preſerve parſnips, 199.
Belly-ach, how cured, Vol. II. p. 287.
Benni-ſeed oil, account of, Vol. I. p. 239—equal to Florence oil, ib.

Berlin, duration of human life at, Vol. III. p. 59.
Biddle (Owen) and others, their account of the tranſit of Venus in 1769, Vol. I. p. 89.
Billings (Capt.) thermometrical journal of the temperature of the air and water at ſea, Vol. III. p. 200.
Billious and *intermitting* fevers of Pennſylvania, inquiry into the cauſe of, with hints for preventing them, Vol. II. p. 206.
Births and deaths in certain places, proportion of, Vol. III. p. 30, 43, 54, 56, 59—proportion of, as to marriages, ib.
—— in France and England compared, 35.
Biſcuit, (ſignifying twice baked) the original name for ruſk, Vol. II. p. 322.
Black-lead, a large bed of, Vol I. *pref.* xii—the beſt point for metallic conductors, Vol. III. p 322.
Blaize-cape, W. Florida, latitude of, Vol. I. p. 253.
Block, deſcription of a newly-invented ſpring-block, for aſſiſting veſſels in ſailing, Vol. III. p. 331.
Blue boy, ſome obſervations on a, Vol. IV. 138.
Blue-ridge in Virginia, barometrical meaſurement of it, Vol. IV. p. 216—formation of the country beyond, 246—nitrous caverns there, ib.
Boats, various modes of propelling, Vol. II. p. 307.
—— Chineſe, how rowed, ib.
—— a new conſtruction propoſed for, 309—another deſigned for American inland navigation, Vol. IV. p. 298, 301.
Boiquira [crotalus horridus] See *Rattle ſnake*.
Bones of a non-deſcript great clawed animal diſcovered in a nitrous cave in Virginia, Vol. IV. p. 246—of another at La Plata in Paraguay, p. 259—others found in Virginia, 439—obſervations on thoſe of the Mammoth, 510—See *Mammoth* bones—Anatomical deſcription of, p. 526.
—— (Shark's) abundance of found foſſil in Virginia, Vol. IV. p 439.
β *Bootes* and the pole ſtar, right aſcenſion and declination of, Vol. III. p. 116.
Botanical deſcription of cryptogamic plants, Vol. III. p. 202.
—— ———— of the caſſia chamæcriſta, Vol. III. p. 228.
—————— of a poiſonous plant, Vol. III. p. 234.
—————— of the podophyllum diphyllum of Linnæus, Vol. III. p. 334.
Botham on the making of E. India ſugar, Vol. III. p. 79.
Bradley (John) his account of the tranſit of Venus and eclipſe of the ſun, as obſerved in 1769 at Lizard point (G. B.), Vol. I. p. 114.
Braſs and iron ſooner tarniſh and ruſt in woody than cleared land, Vol. IV. p. 226.
Breaſt, ſugar uſeful in diſorders of the, Vol. III. p. 75.
Breſlaw, calculations of life there, Vol. III. p. 57.
Bryant (William) his account of an electrical eel, the torpedo of Surinam, Vol. II. p. 166.
Briſtol (Penſ.) chalybeate waters of, Vol. I. p. 303—diſeaſes cured or relieved thereby, 310.

Britain, why ſo named, Vol. IV. p. 506.
Britt (Daniel) his and Mr. Turner's thermometrical obſervations made at Fort Waſhington, Vol. IV. p. 330.
Buck-wheat bloſſoms coveted by bees, Vol. III. p. 258.
Buffon, his calculations on the duration of human life, Vol. III. p. 56—his ſuppoſed origin of planets, Vol. IV. p. 256—his opinion reſpecting the mammoth and its remains, 257.
Bugs, how to deſtroy them, and fleas and flies, Vol. I. p. 222.
Bull, of the zebu kind, deſcribed, Vol IV. p. 151.
Burgundy wine, what grapes preferable for making it in N. America, Vol. I. p. 126.
Burnt-bones, ſmoke of deſtructive to inſects, Vol. I. p. 222.
Buſhnell (D.) his general principles and conſtruction of a ſubmarine veſſel, invented by himſelf and communicated to Mr. Jefferſon, while miniſter at Paris, Vol. IV. p. 303.
Butter, method of preſerving, Vol. II. p. 31.

C

Cables and rigging of ſhips, propoſed improvement in, Vol. II. p. 297.
Cadmus; or a treatiſe on the elements of written language, &c. with an eſſay on the mode of teaching the dumb to ſpeak, Vol. III. p. 262.
Calculation of the tranſit of Venus over the ſun in 1769, in N. lat. 40° and at Philadelphia, Vol. I. p. 5.
Campbell's-town (Pennſ.) baſaltes diſcovered there, Vol. IV. p. 445.
Camphor, triturated with water, its ſtimulant effect on vegetables, Vol. IV. p. 232, 234—a more wholeſome ſtimulant to plants than nitre, 235.
Canal propoſed between Cheſapeak bay and the Delaware, Vol. I. p. 293.
—— navigation, the enclined plane recommended, Vol. IV. p. 299—deſcription of a new boat to be uſed in, 301.
Cancer, Dr. Martin's cure for, Vol. II. p. 212.
Cape-Henlopen, obſervations near it on the tranſit of Venus 1769, Vol. I. p. 89—lat. and lon of the obſervatory and light-houſe there, 92, 96—courſes and diſtance between them, 93.
—— *Cod*, its water cold, Vol. III. p. 84—extent and ſoundings of, 98.
—— *Blaize*, W. Florida, lat. of, Vol. I. p. 253.
—— *Henry* (V.) memoir on the ſand-hills of, Vol. IV. p. 439—prevailing winds there, 440—its light-houſe deſcribed 442.
Carbonic acid gas, [fixed air] what proportion of it in the atmoſphere, Vol. IV. p. 128, 266, 417—obſervations and experiments on its production, 469.
Carter (Landon) his obſervations concerning the fly weevil ſo deſtructive to wheat, Vol. I. p. 205.
Caſcade, a curious one, Vol. II. p. 50.
Caſſia Chamæcriſta, beneficial to worn-out, or poor lands, Vol. III. p. 226—botanical deſcription of it, 228.
Caſtor-oil plants recommended for culture, Vol. I. p. 239.

Catalogue of foreign plants deserving attention in N. America, Vol. I. p. 325.
Cataract of Niagara described, Vol. III. p. 17—Vol. IV. p. 227 its true height, Vol. III. p. 23—and Vol. IV. p. 227—a singular appearance there explained, 229.
Cattle and horses fatten on sugar, Vol. III. p. 75.
Cepede (De la) his distribution of serpents into genera, Vol. IV. p.379.
Chalybeate waters, analysis of the Bristol, Vol. I. p. 303—in what diseases recommended, 306, 310, 313.
Charcoal, a good conductor of electricity, &c. Vol. III. p. 324.
———, spirits of, its deadly effects proposed as a remedy against insects, Vol. I. p. 222.
Chemical admixtures, their effects in producing heat and cold, and the cause, appendix, Vol. I. p. 32.
Chesapeak-bay. See *Lewes*.
Chimney (Staffordshire) described, Vol. II. p. 25.
Chimnies, on the construction of, Vol. II. p. 1. causes of their smoking, 5, 10, 12, 13, 14, 15, 16, 17, 232—how to remedy it, 6, 11, 12, 14, 15, 16, 17, 18, 23, 232—a new mode of admitting air into them, 8—how to form their opening, 10, 11, 29—proportions of the funnels, 10, 11, 12—were not formerly used in England, 19—how constructed at Venice, 18—miscellaneous observations on, 35—sliding plates to, recommended, 13—and described, 25, 29—a curious current of air in them explained, Vol. II. p. 31—its uses, 30, 31, 32.
China resembles N. America, *preface*, Vol. I. p. iii.
Chinese economy as to fuel, Vol. II. p. 32—mode of warming the ground floors, 33.
Cholic, a cure for, Vol. I. p. 235, 312.
Cicuta Venensa described, Vol. III. p. 235—its deleterious quality, 237.
Cinchona Caribæa Santæ Luciæ described, Vol. II. p. 291—its virtues surpass Peruv. bark, ib.—another species described, 292.
Cincinnati, curious discoveries made in an Indian tumulus at, Vol. IV. p. 179—observations and conjectures concerning them, ib. p. 181—thermometrical observations made there, 330.
Clays in America, some account of valuable ones, *pref.* Vol. I. p. xi—a fine paint obtained from one, xii.
Clearing and cultivating a country, their opposite effects on the inhabitants, Vol. II. p. 207—the one to keep pace with the other, 209.
Climate of Ireland, its temperature, Vol. IV. p. 244—yearly quantity of rain there, ib.
Climates of N. America, observations on, Vol I. p. 251, 272—Vol. III. p. vi. 43, 47, 51, 272—Vol. IV. p. 225—how they might be improved, 246,
Clothing worn by Americans defective in fashion and affects their health, Vol. III. p. vi.
Clouds (electric) how formed and operating, Vol. II. p. 88—their general course in America, 99.
Coal (pit) when first used in England, Vol. II. p. 19.

—— neglected by the French, 19, 20—used by the Chinese, 33—its smoke thought to be salubrious, 20—in kindling discharges much moisture, 54.
Cocoons of America larger than the Italian, and the silk stronger, but less fine, *preface* Vol. I. p. vi.
Cold, produced by admixture of certain bodies, Vol. I. *appendix* p. 32.
Collin (Doctor Nicholas) his essay, by way of introduction, on natural philosophy, Vol. III. page iii—his description of a machine invented by himself for saving persons from the upper stories of a house on fire, Vol. IV. p. 143—his philological view of some very ancient words in several languages, 476—his description of a speedy elevator 519.
Comet of June and July 1770, account of it by Mr. Rittenhouse, Vol. I. *appendix* p. 37—its uncommon velocity, ib. 46—smallness of size and short time visible, 37 to 46—its elements and motion, 38—method of computing its visible place and distance from the sun, ib. et 39—curious conjecture concerning comets, ib.—M. Messier discovers this comet in France, 41—his observations on it, ib.—discovered in England by Mr. Six, 44, with his observations on it, ib.—Lord Stirling's account of it, 45—observations on one lately discovered, Vol II. p. 195—account of another, Vol. III. p. 261.
Comets, an essay on the use of, with conjectures on the origin of heat, Vol. I. *appendix* p. 27—have no light of their own, p. 28—their luminous trains accounted for, 29—their presumable temperature, ib.—Newton's opinion of comets, ib.—conceived the residence of intelligent beings, 30—immense height of their atmosphere, and its supposed use, 35.
Committee of husbandry's observations on the fly weevil, Vol. I. p. 218.
Committees appointed by the A. P. S. for observing the transit of Venus over the sun, Vol. I. p. 8, 44, 82, 89—of Mercury, 82.
—— ——for reporting on the practicability of a canal between Delaware and Chesapeak, Vol. I. p. 293—their reports, 294, 295, 296.
Compass (mariner's) an easy method of finding its variation, Vol. II. p. 251—a proposed make-shift for the, 318—improvements on it, 396.
Condensation of electric air lessens its capacity, Vol. II. p. 79.
Conductors of lightning, an addition proposed to, Vol. III. p. 125—improvement on, 322.
Conewaga hills (Pennf.) basaltes and granite found thereon, Vol. IV. p. 445.
Connecticut, longevity of its people, Vol. III. p. 39—deaths at Milford in, ib.
Contents of Vol. I. p. xxiii—of Vol. II. p. xxix—of Vol. III. p. xxxvi —of Vol. IV. p. xl.
Copper (hardened) in use among the Mexicans, the Greeks and Romans, Vol. IV. p. 200, 202—also the N. American Indians, 203.
Country compared with towns, as to number of deaths, Vol. III. p. 41.
Cooke (John) his description of a new standard for weights and measures, Vol. III. p. 328.
Coombe (Thomas) his meteorological observations made by him at Phila. 1770–1, Vol. I. *appendix* p. 70.

Corrections of astronomical calculations, Vol. I. p. 39, 102—of a mistake in the theory of mills, Vol. III. p. 144, and its importance exemplified, 147.
Cortex ruber, medical history of, Vol II. p. 289—more efficacious than the common bark, 290—but inferior to the cinchona, 291.
Cosmogony of Buffon, Vol. IV. p. 256.
Cotton-seed oil, the quantity obtainable, Vol. I. p. 235—good for the cholic, ib.
Courses and distances from Norriton to Philadelphia observatory, and thence to Mason and Dixon's observ.—with their difference of lat. and long. Vol. I. *appendix* p. 6.
—— ——between Cape Henlopen and the lighthouse Vol. I. p. 93.
Coxe (Dr. John Redman) his enquiry into the comparative effects of the opium officinarum extracted from the papaver somniferum, or white poppy, of Linnæus; and of that procured from the lactuca sativa, or common cultivated lettuce of the same author, Vol. IV. p. 388.
Crotalus horridus, or rattle-snake, on means to prevent the consequences of the bite of, Vol. III. p. 100—its poison counteracted, 102, 104, 109—reputed remedies against the bite of it, 114. Its fascinating power attempted to be disproved, Vol. IV. p. 74.—See *Rattle snake*.
—— *miliaris* [ground rattle-snake] some account of, Vol. IV. p. 367.
Cryptogamick plants, observations on, Vol. III. p. 202.
Curiosities found at Cincinnati, Vol. IV. p. 179, 181.
Currant-wine, receipt for making, Vol. I. p. 248.
Currie (Dr. Wm.) his inquiry into the causes of the insalubrity of flat and marshy situations; and directions for preventing or correcting the effects thereof, Vol. IV. p. 127.

D

D'Aboville, his account of a partridge with two hearts, Vol. II. p. 330.
Dampness in air or linnen not unwholesome, Vol. II. p. 21.
Dan river (N. C.) remains of a volcano there, Vol. III. p. 231.
Data respecting population, Vol. III. p. 28.
Davidson (Dr. Geo.) on the virtues of cinchona, Vol. II. p. 290.
Dart-points, vulgarly so called. See *Shark's* teeth.
Deaths and births in certain places compared, Vol. III. p. 30.
Declination and right ascension of β Bootes and the pole star, Vol. III. p. 116.
Deflogisticated air, how produced, Vol. II. p. 209—in what case absorbed, Vol. IV. p. 1. See *Air*.
Degeneracy of certain vegetable productions when transplanted into different situations, Vol. I. p. 223
De Grauchain (Gen.) on a solar and lunar eclipse in 1780, Vol. II. p. 239.
Degree of latitude, Mason and Dixon's measurement of a, Vol. I. p. 96.

Deflogisticated air, or *oxygen*, how produced, Vol. II. p. 209—Vol. IV. p. 2. when absorbed, Vol. IV. p. 1—observations and experiments concerning, Vol. IV. p. 1 to 11—proportion of it in the atmosphere, p. 417.
Delineations and projections of the transit of Venus in 1769, Vol. I. p. 5, 27, 36, 59, 66.
——— of Mercury, Vol. I. p. 8.
Density of the atmosphere, Vol. II. p. 96—description of at L. Erie, &c. Vol. IV. p. 225.
Deveze (Dr. J.) his essay on a new method of treating the effusion which collects under the scull after fractures of the head, Vol. IV. p. 433.
Dews in the Western country very heavy, but innoxious, Vol. IV. p. 225.
Diameter of the sun, Vol. I. p. 52, 79, 84, 101—*appendix* iv.—of Venus, ib.—of Mercury, *appendix*, 52.
——— (apparent) of an object, how to ascertain it, Vol. I. p. 98—interesting experiments attending it, ib.
Diabetes with fever, how cured, Vol. II. p. 286.
Diet commonly used in the United States the cause of certain diseases, Vol. III. p. vi.
Dipus, or Jerboa, account of an American species of, Vol. IV. p. 114.
Diseases prevalent in N. America, Vol. III. p. iii.
——— general causes of, Vol. III. p. vi.
——— of marshy places, observations on, Vol. IV. p. 136.
Distillation of persimons, memoir on the, Vol. I. p. 231.
Donations to the A. P. Soc. list of, Vol. II. p. 398—Vol. III. p. 351—Vol. IV. p. xvii.
Dogs, certain ones yield fine fleeces, Vol. IV. p. 151.
Dress (American) unappropriate to the climate, Vol. III. p. vi.
Dublin, proportion of deaths there, Vol. III. p. 59.
Dumb, mode of teaching such to speak, Vol. III. p. 310.
Dyes of the N. American Indians, account of some of the principal, Vol. III. p. 222—farther observations on, Vol. I. p. 255, 256, 261, 263.
Dymond and Wales observe the transit of Venus in 1769 at Hudson's bay, Vol. I. *appendix* p. iv.
Dysentery, a cure for, Vol. II. p. 285, 291.

E

Earth her distance from the sun, Vol. I. p. 62, 103—the Parisian hypothesis of the excentricity of her orb and distance from the sun, 103—Dr. Franklin's conjectures on the formation of, Vol. III. p. 1—his queries and conjectures relating to the theory of, ib. p. 10—how to calculate the nutation of her axis, Vol. IV. p. 32.
——, reflections on the early condition of the, Vol. IV. p. 503.
Earthquakes, Vol. I. p. 282, 285.
Earths (American) described, Vol. I. *pref.* p. xi.
Earthy substance, a curious one found at Niagara falls, Vol. III. p. 17.
Easterly winds in hot weather, their cause suggested, Vol. II. p. 91.
Eclipse, observations on a solar and lunar, Vol. II. p. 239.

Eclipses of Jupiter's satellites, Vol. I. p. 21, 55.
——— of the sun, Vol. I. p. 114—Vol. II. p. 250—of the moon, Vol. III. p. 150.
Earth's axis, how to calculate its nutation, Vol. IV. p. 51.
Eccentric anomaly of the planets, how to calculate it, Vol. IV. p. 67.
Edinburgh, proportion of deaths in, Vol. III. p. 40, 59.
Eel (electrical) or torpedo of Surinam described, Vol. II. p. 166.
— *(torporific)* or numb-fish described, Vol. II. p. 170.
Effluvia, observations on, Vol. IV. p. 138.
Electricity, Priestly and Beccaria's ideas of, Vol. II. p. 76—cause of rain, 100.
Electric capacity, of air lessened by condensation, and enlarged by heat, Vol. II. p. 79.
——— clouds, how generated, formed and operating, Vol. II. p. 88.
Electric fluid, how accumulated and discharged, Vol. II. p. 75.
——— —— in connection with water, compared by Dr. Franklin to water applied to a sponge, ib.—its effects on certain houses, Vol. III. p. 119—whether conducted by heat or its rarefying effect? p. 121—its effect on a house with conductors, p. 122—modes of conducting it, p. 125, 321—conducted by black-lead and charcoal, 323.
Electrical battery of Dr. Priestley described, Vol. II. p. 93.
——— *Eel*, or torpedo of Surinam described, Vol. II. p. 166.
Electricity, the cause of rain, Vol. II. p. 100.
Elevator (a speedy) for taking views, &c. a new invented machine, described, Vol. VI. p. 519 [*gained the Magellanic prize medal.*]
Ellicot (Andrew) on looming, Vol. III. p. 62.
—— ———, his letter on the right ascensions and declinations of β Bootes and the pole-star, p. 116—his letter on astronomical observations, and the theory and method of calculating the aberration of the stars, the nutation of the earth's axis, and the semi-annual equation, Vol. IV. p. 32—his method of calculating the eccentric anomaly of the planets, p. 67—his miscellaneous observations relative to the western parts of Pennsylvania, particularly those in the neighbourhood of L. Erié, p. 224—his observations made on the old French fort at Presqu'isle to determine the latitude of the town of Erié, p. 231—his observations for determining the lat. and lon. of the town of Natchez, p. 447.
Elliptical orbit, how to determine the true place of a planet in an, Vol. IV. p. 21.
Emeralds, very common among the Mexicans, Vol. IV. p. 199.
Emphasis, observations on, Vol. III. p. 304.
England, formerly without chimnies, Vol. II. p. 19.
———, the use of pit-coal introduced into, ib.—her atmosphere moister than that of N. America, p. 55—and cabinet works there unsuitable to American air, ib.—proportion of births in, Vol. III. p. 35, 57, 59—climate of, p. 52.
English language, a table of all its distinct sounds, Vol. III. p. 292.
Equation (semi-annual) how to calculate it, Vol. IV. p. 51.

Erié (Lake) miſcellaneous obſervations concerning it and parts adjacent, Vol. IV. p. 224—various phenomena attending it, ib.—the winds why like W. India breezes, ib.—the flux and reflux of water in the lake accounted for, and the idea of a tide refuted, 225—heavy dews and fogs there, ib.—the Southern ſhores deſcribed, 226—romantic caſcades there; and ſlate and excellent free-ſtone; all the ſtrata of ſtone horizontally diſpoſed, ib.—an erroneous opinion refuted concerning this diſpoſition, 227—ſingular appearance of the lake rocks; the falls of Niagara deſcribed, ib.
—— town of, its lat. determined, Vol. IV. p. 231—magnetic variation there in 1795, p. 232.
Errata (for tables of) Vol. I. ſee the page before "The contents," and alſo p. 39, 81, 116, and the head of this index—of Vol. III. ib. and p. xl.—of Wm. Barton's paper [p. 25,] on the duration of human life, Vol. III. p. 134.
Eſpaliers, method of managing vines for. See *Vines*.
Eſſay on thoſe enquiries in natural philoſophy which at preſent are moſt beneficial to the U. States, Vol. III. p. iii.
—— tending to improve intelligible ſignals and to diſcover a univerſal language, Vol. IV. p. 162.
Europe, proportion of inhabitants there under 16 years, Vol. III. p. 43—all the obſervations made there on the tranſit of Venus collected into one view, *appendix* Vol. I. p. 12.
Evaporation, experiments on, Vol. II. p. 118—do. Vol. IV. p. 72, 246.
—— the beſt method of drying up marſhes Vol. IV. 243—remarkable inſtance of it in England, 245.
—— (ſpontaneous) of ſugar-maple ſap, produces ſugar, Vol. III. p. 69.
—— in cold air, experiments on, Vol. III. p. 125.
Ewing (Revd. John) his projection of the tranſit of Venus, Vol. I. p. 5—his account of the obſervations on it, p. 42, 66, and of the tranſit of Mercury, 82—his improvement of Godfrey's quadrant, *appendix* p. 126.
Experiments on evaporation in cold air, Vol. III. p. 125.
—— and obſervations relating to the analyſis of atmoſpherical air, Vol. IV. p. 1, 417.
Eye, worm in a horſe's, Vol. II. p. 183.
——ſnake in a horſe's, Vol. II. p. 385.

F

Falls of Niagara account of, Vol. III. p. 17—Vol. IV. p. 227—their height, Vol. III. p. 23 and Vol. IV. p. 229—a ſingular appearance there now firſt explained, ib.
—— of rivers in the Atlantic ſtates, the ground below them of Neptunian origin, Vol. IV. p. 439, 444.
Faſcinating faculty of ſerpents, attempted to be diſproved, Vol. IV. p. 74—obſervations on the, p. 363, 370. See *Rattle-ſnake*.
Fenwick's Iſle. See *Lewes*.
Fermentation, why it excites heat, *appendix* Vol. I. p. 34.
—— of wine, obſervations on it, Vol. I. p. 174, &c.

Fevers in Pennſylvania, bilious and intermitting, with hints for preventing them, Vol. II. p. 206—how cured, 290.
Figs, method of curing, Vol. I. p. 198.
Files, deſcription of a machine for cutting, Vol. I. p. 300—its ſuperiority, 302.
Finery cinder [ſcales of iron] obſervations on, Vol. IV. p. 466.
Fire, a machine to ſave perſons from, Vol. IV. p. 143—others, p. 143, 519.
Fire-places, how to be conſtructed, Vol. II. p. 10. See *Stoves*.
—— ——(*Staffordſhire*) deſcribed, Vol. II. p. 25—miſchief of large ones, 35—a moſt ingenious one for warming ſeveral rooms deſcribed, 36.
Fiſh, their characters as given by Vicq, D'Azir and D'Aubenton, Vol. IV. p. 280.
Fiſhes falling with rain, accounted for, Vol. II. p. 117.
—— experiments on the animal heat of, Vol. III. p. 98.
Fixed air. See *Carbonic acid gas*.
Flagg (Henry Collins) his obſervations on the numb fiſh or torporific eel, Vol. II. p. 170.
Floating anchor recommended againſt a lee ſhore, Vol. II. p. 311.
Florida (Weſt) letter concerning, Vol. I. p. 250—natural products, ib.—account of its climate, 251, Vol. III. p. 48—gulf of Mexico, 252.
—— *Indians* acquainted with tin, Vol. IV. p. 202.
—— *(Eaſt)* ſome account of its climate, Vol. III. p. 48.
Fluids, nature and properties of, Vol. II. p. 104.
Fly-wevil deſtructive to wheat, obſervations on it, Vol. I. p. 205—further obſervations, 218.
Fogs remarkably denſe but innoxious on the rivers Ohio and Allegany, Vol. IV. p. 225.
Foreign plants deſerving attention in N. America, catalogue of, Vol. I. p. 255.
Fortifications (remains of Indian) Vol. III. p. 215, Vol. IV. p. 178.
Foſſil ſhells and bones, numerous in the weſtern country, Vol. III. p. 218—and elſewhere in America, Vol. IV. p. 439, 444. See *Bones*, *Oiſter*, *Shark*.
Fractures of the ſcull, a new method of treating them, Vol. IV. p. 439.
France, air of, moiſter than that of America, Vol. II. p. 55—population, &c. of, Vol. III. p. 32—proportion of births and deaths in, 36, 41, 59.
Franklin (Dr. Benj.) letter from him on chimnies, Vol. II. p. 1—his love of freſh air; ſleeps with open window; holds night air, moiſt air, and damp linnen as not unwholeſome, 21—propoſes an apparatus for lecturing on ſmoke and rarefied air, 22—makes ſmoke deſcend, and deſcribes a ſtove of his invention, 24—diſcovers a curious property of chimnies in ſummer; its utility and cauſe explained, 30—his letter to Mr. Nairne on hygrometers; with intereſting facts concerning the ſtate of the atmoſphere in England, France and N. America, 51—his deſcription of a new ſtove that conſumes its ſmoke, 57—his intereſting letter to Alphonſus Le Roy, concerning maritime obſervations, &c. 294—his obſervations on the warmth of ſea-water in the gulph ſtream, &c. 325, 326.

326, 327—his journal of a voyage from France to America, 328—offers a new conftruction for boats, 309—on the formation of the earth, Vol. III. p. 1—his new theory of light and heat, 5—his communication of the Chinefe manner of making certain paper, 8—on magnetifm and the theory of the earth, 10—defcribes a curious phenomenon, 14.

Free-ftone (excellent) on L. Erie, Vol. IV. p. 226.

Frogs, falling with rain, accounted for, Vol. II. p. 117—experiments with opium upon, Vol. IV. p. 398.

Froft and fnow lefs in America than formerly, Vol. I. p. 275—its frequency in Pennfylvania, Vol. III. p. 52—its action applied to making maple fugar, 69.

Fructification of plants, organs of, Vol. III. p. 209. See *Plants*.

Fruits and pulfe, their nature changed by tranfplantation, Vol. I. p. 224.

Fuel, advantages of, when plentiful and cheap, Vol. II. p. 26—means of faving it, 29, 33, 34, 35, 36, 67.

Furnaces, a machine to regulate the heat of, Vol. I. p. 286.

G

Gas (azotic)
—— (carbonic acid)
—— (nitrous)
—— (hydrogen)
—— (ammoniacal)
} Experiments and obfervations on, Vol. IV. p. 1, 17, 417, to 428.

Gaffendus firft difcovers Mercury on the fun's difc, Vol. I. *appendix* p. 53.

Georgia, its climate, Vol. III. p. 49—proportion of deaths at Savannah, 59.

Germany, proportion of deaths in, Vol. III. p. 41. 59.

Gilpin (John) on the annual paffage of herrings, Vol. II. p. 236.

Glaffes, (fmoked) beft to defend the fight, Vol. I. p. 106.

Goats yielding fleeces of fine wool, Vol. IV. p. 151.

Godfrey's quadrant, an improvement of, Vol. I. *appendix* p. 21.

Golden caffia defcribed, Vol. III. p. 228.

Granite (cryftallized) found on the Conewago hills (Pennf.) Vol. IV. p. 445.

Grapes (wild) abundant in N. America, *pref.* ix—their culture recommended, ib.—different kinds defcribed, 192—anecdote of a Roman cultivator of, Vol. I. p. 121—how to obtain the beft wine from, 125—the kinds fuitable to N. America, 126—American forts defcribed, 191—how to gather and make wine of them, 171—white do. 172—red do. 175—how to make raifins, 193—curious inftances of the effects of tranfplanting them, 222. See *Vines*.

Grafshoppers, artfully made ufe of by the nine-killer, as a bait to entrap fmall birds as prey, Vol. IV. p. 124.

Graves (Indian) accounts of, Vol. IV. p. 179, 181, 203, 205.

Great-Britain, obfervations there on the tranfit of Venus, Vol. I. p. 105, 114.

Great-bone-lick in Kentucky, remarkable appearances at, Vol. IV. p. 517.

Great claw, or megalonyx. See *Bones*.

Greenwich obfervatory (G. B) obfervations there on the tranfit of Venus in 1769, Vol. I. p. 105.

Greenway (Dr.) his account of the beneficial effects of caſſia chamæcriſta on worn-out or poor lands, Vol. III. p. 226—his communication on a ſuppoſed volcano in N. Carolina, 223—his account of a poiſonous plant in Virginia, 234.

Griſt-mills, obſervations on, Vol. IV. p. 350.

Grauchain (De) on a ſolar and lunar eclipſe obſerved in 1780, Vol. II. p. 239.

Grotto at Swatara in Pennſ. deſcribed, Vol. II. p. 177.

Ground-rattle-ſnake, ſome account of the, Vol. IV. p. 367.

Grub-worm, how to preſerve the vine from it, Vol. I. p. 164.

Gulf ſtream, obſervations on it, Vol. II. p. 314—its warmth, 325—Doctor Franklin's obſervations on it confirmed, Vol. III. p. 83. Further remarks on that ſtream, 86, 96, 194.

Gum copal, ſuppoſed to exude from a ſpecies of American ſumach, Vol. I. *pref.* ix.

Gums (viz.) perſimon, Vol. I. p. 234.—maſtick, 257—ſtorax, ib.—ſcammony, ib.—dragon, 259, 264—copal, *pref.* ix, 262—ſenegal, 263—arabic, ib.—labdanum, 264—galbanum, ib.—oppoponax, ib.—myrrh, 265—benjamin, ib.—ammoniacum, ib.—aſſafœtida, 266.

H

Hadley's quadrant, an eaſy and accurate way to adjuſt the glaſſes of it, on land, for the back obſervation, Vol. IV. p. 154.

Halley (Dr.) errors in his tables, Vol. I. p. 102—obſerves a remarkable period in the planet Mercury's motion, *appendix* 53—firſt conceived the method of aſcertaining the ſun's parallax, 54.

Hare and Skinner's natural hiſtory of the ſilk-worm from the egg to the cocoon; with the mode of obtaining the ſilk, &c. Vol. II. p. 347.

Harpſichords, an improved method of quilling, Vol. II. p. 185, 190.

Heart (Jonathan) on American Indians and ſome ancient works, Vol. III. p. 214.

Hearts, two found in a partridge, Vol. II. p. 330.

Heat, concerning the origin of, Vol. I. *appendix* p. 32—different modes of exciting it, ib.—the effect of vibratory motion, ib.—its proportion regulated by the face of country, p. 273—the cauſe of moſt winds, ib.—whence beſt reflected, 275—deſcription of a new machine for preſerving an equal degree of, 286—its expanſion of wood, Vol. IV. p. 29—enlarges the capacity of electric air, Vol. II. p. 79—the ſtrongeſt attraction between it and water, 144—deſtroys magnetiſm, 180.

—— and light, a curious theory of, Vol. III. p. 5—both deemed ſubſtances, Vol. IV. p. 11.

Hebrew language, obſervations on it, Vol. III. p. 264, 276.

Heckewelder (Rev. John) his account of the remarkable inſtinct of a bird called the nine-killer *(lanius excubitor)* Vol. IV. p. 124—his account of a ſuppoſed animal called by the Indians the big-naked-bear, Vol. IV. p. 260.

Hemp

Hemp (Indian) how useful, Vol. I. *pref.* p. x—observations on the raising and dressing it, p. 198—wind and air injurious to hemp, 204—best method of preparing it, 203.
Henlopen (Cape) observations near it on the transit of Venus in 1769, Vol. I. p. 89, lat. and lon. of the Cape, 92, 96—courses and distance between the lighthouse and observatory there, 93.
Henry (Wm.) of Lancaster's description of a self-moving register for preserving an equal degree of heat, Vol. I. p. 286.
—— *(Cape)* memoir on the sand-hills of, Vol. IV. p. 439—lighthouse described, 442.
Herrings, their annual passage, Vol. II. p. 236.
———— the same in England and America, ib.
Heterandra reneformis, a new plant, described, Vol. IV. p. 173.
Hieroglyphics, supposed ones on the Ohio, &c. Vol. IV. p. 195.
Higgins (Jesse) his manner of draining ponds in level grounds, Vol. III. p. 325.
Hog's lard and tar, a preservative to grape vines, Vol. I. p. 164.
Hollingsworth (Henry) his method of destroying wild garlic, Vol. I. p. 241.
Honey paid in tribute to the Mexican sovereigns, Vol. III. p. 245.
—— bee, an enquiry if originally a native of America, Vol. III. p. 241—various kinds of, 245.
Hopkinson (Francis) his description of a new machine for measuring a ship's way, Vol. III. p. 159—his account of a worm in a horse's eye, 183—his improved method of quilling harpsichords, 185, 190—his description of a machine for measuring a ship's way, 239—his new invented spring-block described, 331 [*gained the Magellannic prize*]—his and Mr. Rittenhouse's account of the effects of lightning on a house with conductors, 122.
Horse's eye, a worm in it, Vol. II. p. 183—a living snake in it, 385.
Horses and cattle fattened on sugar, &c. Vol. III. p. 75.
Hospitals, a machine proposed for regulating the due temperature of their air, Vol. I. p. 289—on the construction of, Vol. III. p. 348.
Hot-house, a new way to heat it, Vol. II. p. 74.
Houses struck with lightning, account of, Vol. III. p. 119, 122—mode for their greater security suggested, 125.
Hudson's bay, observations on the transit of Venus there in 1769, Vol I. *appendix* p. 1.
Human life in America, on the probable duration of, Vol. III. p. 25. 134.
Humidity of the air in England and France, Vol. II. p. 55.
Hurricane, description of a remarkable one in N. England, 1773, Vol. II. p. 136—conjectures as to its cause and origin, 139.
————*s*, theory of, Vol. II. p. 343—of the same nature with tornadoes and windspouts, 345.
———— common at lake Erie, Vol. III. p. 63.
Hutchins (Thomas) his description of a remarkable rock and cascade near Youghiogeny river, Pennsylvania, Vol. II. p. 50.
Hydrargyrus vitriolatus flavus. See *Turbith* mineral.
Hydrogen gas, or inflammable air, proportion of, in the atmosphere, Vol. IV. p. 128,

p. 128, 417—its properties, ib.—origin of it in marſh air, 420—quantity of it afforded by charcoal and finery cinder, 464—from what other ſubſtances it may be obtained, 465.
Hydrophobia cured in a young woman, Vol. II. p. 231.
Hygrometer, a new conſtruction for it, Vol. II. p. 56.
Hypotheſis (Pariſian) of the eccentricity of the earth's orb and its diſtance from the ſun, Vol. I. p. 103.

I

Icelanders' manner of warming their houſes, Vol. II. p. 34.
Inclined planes, a capital improvement in canal navigation, and far ſuperior to locks, Vol. IV. p. 299.
Index floræ Lancaſtrienſis, Vol. III. p. 157.
Indian antiquities of America deſcribed, Vol. III p. 214—Vol. IV. p. 179, 181, 184, 198.
——— traditions, Vol. III. p. 216, 220, Vol. IV. p. 188, 260—dyes, Vol. III. p. 222—languages, cuſtoms, manners and genius, 220, Vol. IV. p. 190, 205—ancient monuments, Vol. III. p. 214, Vol. IV. p. 179, 181.
——— *corn*, originally the only grain of America, Vol. I. *pref.* p. iii.
——— dart-points. See *Shark's teeth.*
——— *hemp* deſerving attention, Vol. I. *pref.* p. x.
Indians (American) long lived, Vol. III. p. 50, 51—traditions, 216, 220—languages, cuſtoms, manners and genius, ib.—ſome account of former tribes, Vol. IV. p. 183—ſuperſtitious veneration for certain birds, &c. p. 206.
———population of, below the falls of rivers on the ſettlement of America, Vol. IV. p. 252.
———ſculpture, on a rock at Kenhawa, Vol. IV. p. 253,
Inflammable air, or phlogiſton, obſervations on, Vol. IV. p. 2, &c.—may be produced from ſpirits of wine, ib. p. 19—its properties, ib. p. 130—proportion of it in the atmoſphere, 7, 128, 417—whence formed, 420.
——— fluids, deſcription of a propoſed veſſel for boiling them, Vol. IV. p. 431.
Inflammation, why an exciter of heat, Vol. I. *appendix* p. 34.
Inguana [ſiren] an intermediate claſs connecting the amphibiæ with fiſh, Vol. IV. p. 277—a new ſpecies of it detected in Peale's muſeum, ib.—a ſcientific deſcription of it, 279.
Inland navigation, propoſed improvements in, Vol. I. p. 293.
Inſcription (curious) near Naples, Vol. I. p. 285.
Inſects, how to protect pulſe and grain againſt, Vol. I. p. 222.
Inſtruments uſed in Pennſylvania for obſerving the tranſit of Venus at different places in 1769, Vol. I. p. 10, 13.
Introduction, being an eſſay on thoſe enquiries in natural philoſophy, which at preſent are moſt beneficial to the U. States, Vol. III. p. iii.
Ireland, its temperature of climate equable ; yet ſeldom affords three months ſummer weather, Vol. IV. p. 244—quantity of rain ſometimes there, ib.—its eaſt winds produce epidemic inflammatory diſorders, ib.

Iron very ſuſceptible of ruſt, and braſs ſoon tarniſhes in the weſtern country—and why, Vol. IV. p. 226—a portion contained in air, 327—of its ſolution in the diluted ſulphuric and muriatic acids, 457—obſervations on finery cinder, or ſcales of, 466.
Italy, &c. why formerly colder than now, Vol. I. p. 276.

J

Jaw-fall of infants, ſpecies of tetanus, Vol. II. p. 230—how to prevent it, ib.
Jefferſon (Thomas) his letter in anſwer to the A. P. Society's, on being choſen their preſident, Vol. IV. p. xii.—his letter on the height of certain mountains in Virginia, ib. p. 222—his memoir on the diſcovery of certain bones of a quadruped of the clawed kind in the weſtern parts of Virginia, ib. p. 246; with a P. S. on the ſkeleton of the Paraguay megatherium, 259—his deſcription of a mould-board of the leaſt reſiſtance, and of the eaſieſt and moſt certain conſtruction; taken from a letter to Sir John St. Clair, preſident of the Board of Agriculture at London, 313.
Jefferſonia binata, a new plant, Vol. III. p. 342.
Jerboa, account of an American, Vol. IV. p. 114.
Johnſon (Dr.) his rule for pronunciation erroneous, Vol. III. p. 270.
Jones (Dr.) and D. Rittenhouſe's account of houſes ſtruck with lightning, Vol. III. p. 119.
Journal of a voyage from France to America by Dr. Franklin, Vol. II. p. 328.
——*s (thermometrical)* at ſea, Vol. III. p. 90, 194.
Jupiter, eclipſes of his ſatellites obſerved at Norriton, Vol. I. p. 21—at Philadelphia, 55.
—— his diſtance from the ſun, Vol. I. p. 62.
—— *(ſatellites of)* their immerſions and emerſions as obſerved at Wilmington on the Delaware, and at the weſtern obſervatory, for determining the weſtern bounds of Pennſylvania in 1784, Vol. IV. p. 32—at Baltimore in 1788, ib. p. 48.

K

Kamtſchatka, ſimilar to Labrador, but different from ſame latitudes in Europe, Vol. I. *pref.* p. iv.
Kenhawa river, a curiouſly ſculptured rock there, Vol. IV. p. 252.
Kentucky, remarkable appearances at the Great-bone lick there, Vol. IV. p. 517.
Ketchup (India) how to make, Vol. I. p. 259.
King (Nicholas) his improvements in boats, &c. for river navigation, Vol. IV. p. 298.

L

Lactuca sativa [garden lettuce] yields good opium, Vol. IV. p. 391. See *Opium*.
Lake Erié. See *Erié*.
Lakes, subject to tempests, Vol. III. p. 63.
Land, when and why colder or warmer than water, Vol. I. p. 274.
—— *spouts*, account of two, Vol. II. p. 112—of another formed first on the water, 115—curious effects of it, 116.
—— *air*, experiments on, Vol. IV. p. 262, 266—less pure than sea-air, 269.
Lands (poor) good effects of cassia chamæchrista thereon, Vol. III. p. 226.
Language (written) an essay on, Vol. III. p. 262.
—— *(a universal)* proposed, Vol. IV. p. 167.
Languages, two different ones used by the Natchez Indians, Vol. IV. p. 193—Dr. Collin's philological view of some very ancient words in several, Vol. IV. p. 476—mines of human antiquities, 478—comparative specimens of various languages, 485.
Lanius excubitor, or great shrike, some account of, Vol. IV. p. 124.
Latitude of Norriton observatory, Vol. I. p. 22.
—— of Philadelphia, 54.
—— of Lewes, 92.
—— of Providence (N. E.) 103.
—— of Newcastle court-house, 93.
—— of lighthouse on Cape Henlopen, 93.
—— of Cape Blaize (W. Flor.) 253.
—— of Mobile bay, ib.
—— of Santa Rosa, ib.
—— of town of Erié, Vol. IV. p. 231.
—— of the town of Natchez, Vol. IV. p. 449.
La Plata, account of a huge skeleton found there, Vol. IV. p. 259.
Latrobe (B. Henry) his memoir on the sand-hills of Cape Henry (Vir.) Vol. IV. p. 439—supplement thereto, 444.
Lava erupted from mount Vesuvius, Vol. I. p. 283, 285.
Laws and regulations of the Amer. Philos. Soc. Vol. I. p. v.—Vol. II. p. v.
Law to encrease the annual subscriptions and deposits of members of the A. P. S. Vol. II. p. xvii.
Lee-shore, a proposed contrivance to avoid it, Vol. II. p. 311.
Lenses (two convex) invert objects, and why, Vol. II. p. 37—other curious appearances produced by them explained, 37 to 42.
Leprosy assigned as the cause of the dark complexion of Negroes, Vol. IV. p. 289.
—— various kinds described, Vol. IV. p. 289.
Le Roy on the construction of hospitals, Vol. III. p. 348.
Letters, new ones proposed, Vol. III. p. 277—their affinities, 292.
Lettuce [lactuca sativa] yields good opium. See *Opium*.
Lewes observatory, transit of Venus as observed there in 1769, Vol. I. p. 89—courses and distances from it to the provincial west line between

Fenwick's

Fenwick's iſle and Cheſapeak bay, 91—alſo to lighthouſe on Cape Henlopen, 93—its lat. and lon. 92, 96.
Life (human) on the probable duration of, Vol. III. p. 25, 134.
Light and *heat*, Franklin's new and curious theory of, Vol. III. p. 5—both are ſubſtances, Vol. IV. p. 11.
Light of *comets* proceeds from the ſun, Vol. I. *appendix* p. 28.
—— -- —— aſtoniſhing velocity of, ib. *appendix* 36.
Light-houſe on Cape Henlopen, its lat. and lon. Vol. I. p. 92, 96.
———— at Cape Henry (Vir.) deſcribed, Vol. IV. p. 442—its ſtair-caſe faulty, ib.
Lightning and thunder ſtorms, theory of, Vol. II. p. 74.
———— account of houſes ſtruck by, Vol. III. p. 119, 122—Q. whether conducted by heat, or its rarefying effect? 121—how to guard ſhips againſt, Vol. II. p. 302.
———— *rods*, or conductors, an improvement propoſed in the conſtruction of, Vol. III. p. 125—another improvement, 321.
Limeſtone, intermixed with flint and marine petrifactions on L. Erie, Vol. IV. p. 226.
Lion, dimenſions of a large one, as taken by M. de Buffon, Vol. IV. p. 248—a larger one ſtill, preſumed to be a native of America, 252—its figure ſuppoſed to be cut on a rock upon the Kenhawa, 253—preſumptive proofs of his exiſtence, ib.—curious particulars concerning the lion, 254.
Liriodendron tulipifera [tulip-tree] how affected by camphor infuſed in water, Vol. IV. p. 232.
Liſt of the officers and members of A. P. S. Vol. I. p. xii.—Vol. II. p. xxiv.—Vol. III. p. xxviii.—Vol. IV. p. xiv.
Lizard point (G. B.) account of the tranſit of Venus and eclipſe of the ſun, as obſerved there in 1769, Vol. I. p. 114—longitude of, 116.
Locked jaw, preventive for, Vol. II. p. 228.
Logarithm of any number *(common)* Rittenhouſe's mode of raiſing it immediately, Vol. IV. p. 69.
London, proportion of deaths in, Vol. III. p. 40—table of the duration of human life there, 57.
Longevity, comparative degrees of, Vol. III. p. 38—particular inſtances of in the United States, 44—inſtances elſewhere, 50.
Longitude of Philadelphia and Norriton obſervatories, Vol. I. p. 5, 59, 93, 96.
———— of Lewes, Vol. I. p. 92, 96.
———— of Providence (N. Eng.) and Cambridge (do.) Vol. I. p. 103.
———— of the Lizard point (G. B.) Vol. I. p. 116.
———— and latitude, meaſurement of the difference of, between Philadelphia and Norriton obſervatories, Vol. I. *appendix* p. 5.
———— of the town of Natchez, Vol. IV. p. 451.
Looming, obſervations on the phenomenon of, Vol. III. p. 62.
Lorimer (Dr.) his obſervations on W. Florida, Vol. I. p. 250.
Lukens (John) his account of the contacts in the tranſit of Venus over the ſun, Vol. I. p. 28.
Lunar eclipſe, obſervations on one, Nov. 2d 1789, Vol. III. p. 150.

M

Machine for regulating the heat of furnaces and temperature of the air, Vol. I. p. 286—for pumping veſſels at ſea, without manual labour 289—for cutting files, 300.
——— for meaſuring a ſhip's way, invented and deſcribed, Vol. III. p. 239.
——— for ſaving perſons from the upper ſtories of a houſe on fire, Vol. IV. p. 143.
Madagaſcar ſheep deſcribed, Vol. IV. p. 149.
Maiz, [zea] the only grain found by the firſt ſettlers in N. America, Vol. I. *pref.* p. iii.
M'Cauſlin (Rob.) his account of an earthy ſubſtance at Niagara; with remarks on the cataract there, Vol. III. p. 17.
Madeira, proportion of deaths in, Vol. III. p. 41, 59—encreaſing population of, 43.
Madiſon (Rev. J.) his letter to D. Rittenhouſe containing obſervations on the climate at Williamſburgh, Virginia, Vol. II. p. 141—his experiments and obſervations on the ſweet-ſprings of Virginia, 197—his obſervations on a lunar eclipſe and the tranſit of mercury over the ſun, Nov. 1789, Vol. III. p. 150—his experiments on magnetiſm, Vol. IV. p. 323.
Magellan (I. H. de) his donation to the American Philoſophical Society, Vol. II. p. xix.
Magellanic prize, how and in what caſes to be awarded, Vol. II. p. xix—Vol. III. p. 336—on whom beſtowed, p. 262, 321, 331. Vol. IV. p. 519.
Magnetic variation at the town of Erie in 1795, Vol. IV. p. 232.
——— *needle (univerſal)* Vol. I. p. 54.
Magnetical obſervations much wanted in America, Vol. I. p. 254.
——————— made at Cambridge (Maſſachuſetts) Vol. III. p. 115.
Magnetiſm, experiments on, Vol. II. p. 178—Vol. IV. p. 324—deſtroyed by heat, 180—produced by friction of two pieces of ſteel againſt each other, ib.—how communicated to a ſoft ſteel ramrod, ib.—curious thoughts on, Vol. III. p. 3.
——— and the theory of the earth, queries and conjectures concerning, Vol. III. p. 10.
Magnolia and ſpice-wood excellent in ſome diſeaſes, Vol. I. *pref.* p. viii.
Magotty-bay-bean (caſſia chamæchriſta) an excellent manure, Vol. III. p. 226—botanical deſcription of it, 228.
Mammoth-bones, Buffon's opinion of them, Vol. IV. p. 257—memoir concerning them, 510—whence their name, ib.—abundant beyond the mountains, and why, 511—a popular opinion concerning the animal rejected, ib.—the remains of more than one ſpecies of a huge non-deſcript, 512—obſervations as to the probable volume of the mammoth, 511—arguments againſt his now-exiſtence, 516—Indian tradition reſpecting him, ib.—remarkable appearances at the great-bone-lick in Kentucky, 517—conjectures as to his nature and purſuits, ib.

Mankind, reflexions on the early ſtate of, Vol. IV. p. 481.
Manure requiſite to vineyards, Vol. I. p. 121—what kinds proper, 124, 128 —how diſpenſed with by the poor, 124—how applied to vineyards, 160 —a rich one, 161.
Maple tree (Sugar) ſuppoſed to be the arrack tree, Vol. I. *pref.* p. v—its ſap yields a fine ſpirit, ib.—obſervations on, Vol. III. p. 64—its produce in ſugar, 66, 69, 71, 73, 76—its ſap a pleaſant drink, 68—how uſeful otherwiſe, 73—how to make the ſugar, 67—ſuperior to that of the W. Indies, 71—virtues and uſes, 74.
Marble, (curious) Vol. II. p. 50.
Marine ſalt and vegetable acid combined, their virtues in putrid diſorders, Vol. II. p. 284.
Mariner's compaſs improved, Vol. II. p. 396.
Maritime obſervations, Vol. II. p. 294—Vol. III. p. 90.
Marriages as to births, proportion of, Vol. III. p. 30.
Mars, his diſtance from the ſun, Vol. I. p. 62.
Marſeilles, a poiſon kept there, Vol. III. p. 238.
Marſhy-ſituations, their inſalubrity inquired into, Vol. IV. p. 127—their quality and properties, ib. p. 128, 428.
Marſh-air, experiments and obſervations on, Vol. IV. p. 415 to 430.
Marſhes, mode of drying them up, Vol. IV. p. 244.
———, ſalubrious in their general effects, Vol. IV. p. 429.
Martin (Dr. Hugh) on certain Indian dyes, Vol. III. p. 222.
Maryland and *Pennſ.* propoſals for improving their inland navigation, Vol. I. p. 293.
Maſkelyne (Nevil) his letter to Mr. Penn, Vol. I. p. 40—his obſervations on the tranſit of Venus at Greenwich (G. B) in 1769, p. 105—his account of the northern obſervations, *appendix*, p. 1.
Maſon and *Dixon's* meaſurement of a degree of latitude, Vol. I. p. 96.
Maſſachuſetts, proportion of perſons to each family and houſe in, Vol. III. p. 31—marriages and births there, ib.—male inhabitants, 35—number of births and deaths at Salem, 38, 56—do. at Kingham, 56—longevity of the inhabitants, ib.
Mathematical and aſtronomical papers, Vol. I. p. 1 to 116—*appendix* thereto following, p. 116.
Mayer's celeſtial diſcoveries, Vol. II. p. 222.
Meantes, an order of Linnæus improperly ſuppreſſed, Vol. IV. p. 281.
Meaſures and weights, a new ſtandard for, Vol. III. p. 328.
Meaſurement by Maſon and Dixon of a degree of latitude, correſponds with another ſince made, Vol. I. p. 96.
——— *(terreſtrial)* of the difference of longitude and latitude between Norriton and Philadelphia obſervatories, Vol. I. *appendix* p. 5.
Meat, how to preſerve, Vol. II. p. 30, 31.
Medical hiſtory of the *cortex ruber*, Vol. II. p. 289.
——— *papers*, Vol. I. p. 368 to 404.
——— *enquiries*, Vol. III. p. iii.
Medicinal productions, Vol. I. p. 235, 239, 250, 257, 258, 260, 262, 263, 264, 265, 266.———Vol. III. p. 102, 104, 114, 345.

Megalonyx, or great claw. See *Bones*.
Megatherium of Paraguay, Jefferson's observations on its skeleton, Vol. IV. p. 259.
Members of the A. P. S. a list of,—Vol. I. p. xii.—Vol. II. p. xxiv.—Vol. III. p. xxviii.—Vol. IV. p. xiv.
Memoir on the discovery of America, Vol. II. p. 263.
——— concerning the fascinating faculty which has been ascribed to the rattle-snake and other American serpents, Vol. IV. p. 74.
———on the subject of a new plant, growing near Philadelphia, Vol. IV. p. 173.
———on a new species of siren, Vol. IV. p. 277.
——— on the discovery of the remains of a new non-descript quadruped, Vol. IV. p. 246—with observations on the megatherium of Paraguay, 259.
——— on the extraneous fossils denominated mammoth bones; principally designed to shew, that they are the remains of more than one species of non-descript animal, Vol. IV. p. 510.
——— on the distillation of persimons, Vol. I. p. 231.
——— on amphibia, Vol. IV. p. 362.
——— on the sand hills of Cape Henry (V.) Vol. IV. p. 439—supplement thereto, 444.
Mercury precipitated from oxygen gas, Vol. IV. p. 416.
Mercury, his distance from the sun, Vol. I. p. 62—account of his transit over the sun in 1769, p. 82, *appendix* 50—Vol. II. p. 249—in 1782, p. 260.
——— — ———affords with Venus's the best opportunity for determining the longitude, Vol. I. p. 82—his horary movement, 86—projection of his transit, 88—his diameter ascertained, *appendix*, 52—a remarkable period in this planet's motion, *appendix* 53—when first observed on the sun's disc, ib.
Meridian mark, a new method of placing it, Vol. II. p. 181.
———, line, a new way to find the true one, Vol. II. p. 251.
Messier's account of the comet of 1770, as it appeared in France, Vol. I. *appendix* p. 41.
Metallic conductors of lightning, a proposed addition to, Vol. III. p. 125—an improvement on, 321.
Metals, constitution of, Vol. IV. p. 452.
Meteor, or falling star, described, Vol. II. p. 174, 175.
Meteorological observations made at Philadelphia in 1770-1, Vol I. *appendix* p. 70—at Bradford, (N. England) in 1772, Vol. II. p. 122—at Williamsburgh (Virginia) in 1777 and part of 1778, p. 141—at various places between Richmond and the Alleghany mountains in Bottetourt, in 1791, Vol. IV. 219—at sea and on shore, 272—at fort Washington (Cincinnati) 329.
——— *enquiries*, Vol. III. p. xxv.
Mexican Indians, or Aztecas, some account of, Vol. IV. p. 183, 199, 204, 210, 213.
Mexico, instances of great longevity in, Vol. III. p. 50.

Mica membranacea, taken out of tumuli, Vol. IV. p. 203, 204—its ufes among the Mexicans, ib.
Middle colonies of N. America, their climate changed, Vol. I. p. 272.
Milk, method of preferving, Vol. II. p. 31.
Miller (Peter) of Euphrata, on the time for fowing peafe, Vol. I. p. 243—his defcription of a grotto at Swatara in Pennfylvania, Vol. II. p. 177.
Mills, an effential miftake corrected in the theory of, Vol. III. p. 144.
——- further obfervations on, Vol. III. p. 185, 319—obfervations and calculations concerning, Vol. IV. p. 350, &c.
Minerals, &c. in America, obfervations on, Vol. I. *pref.* p. xi. xiii.—abundant in Weft Florida, 251.
Mineral waters of Briftol (Pennfylvania) analized, Vol. I. p. 303—in what difeafes recommended, 306, 310, 313.
Miners, how to provide them with frefh air, Vol. II. p. 32.
Mifcellaneous papers, Vol. I. p. 281.
Miffiffippi, apparent height of its water formerly, Vol. III. p. 219.
Mobile bay, latitude of, Vol. I. p. 253.
——- *country*, great quantities of very large oifter fhells there, Vol. p. 218.
Moift air, not unwholefome, Vol. II. p. 21.
Moifture, certain laws of, Vol. II. p. 51.
———-difcharged from pit-coal, Vol. II. p. 54.
Moiftnefs of atmofphere in England and France greater than in N. America, Vol. II. p. 53 to 55.
Mokkafin fnake is a non-defcript, Vol. IV. p. 380.
Molaffes of maple-fugar, agreeable, Vol. III. p. 73—yields a fpirit, ib.
Moon, viewed through a telefcope, to different perfons prefents a different face, Vol. II. p. 38—inverted appearances on her face, when feen through the Caffegrain telefcope, accounted for, 41, 42.
——- eclipfed and obferved, Vol. III. p. 150.
Morel (John) his account of benni-feed oil, Vol. I. p. 239.
Morgan (Dr. John) his effay on the expreffing of oil from fun-flower feed, Vol. I. p. 235—his communication of the medical hiftory of the red bark, Vol. II. p. 289—his art of making anatomical preparations by corrofion, 366—his defcription of a living fnake in a horfe's eye, and of other unufual productions of animals, 383—his account of a motley coloured, or pyed negro girl and mulatto boy, 392.
Mofs (Henry) a negro, turned white, Vol. IV. p. 295.
Moffes and *Mufhrooms*, their fexual organs determined, Vol. III. p. 204.
Mould-board, one of the leaft refiftance and eafieft and moft certain conftruction defcribed, Vol. IV. p. 313.
Mounds (Indian artificial) fome account of curious difcoveries made therein, Vol. IV. p. 179, 181, 203, 205.
Mountains (white) of N. Hampfhire defcribed—the higheft lands in N. England—Indian fuperftition towards them—their figure and extent; number of their fummits uncertain—their materials and productions—meteors; why named the White Mountains, Vol. II. p. 42 to 49.
————- in Virginia, barometrical meafurement of thofe called the Blue Ridge

Ridge, Warm Spring and Alleghany, Vol. IV. p. 216—other obſervations thereon by Mr. Jefferſon, 222.
Mud-Inguana. See *Inguana.*
Muhlenburgh (Henry) his Index floræ Lancaſtrienſis, Vol. III. p. 157—his Supplementum indicis floræ Lancaſt. Vol. IV. p. 235.
Mulatto boy, with a pyed ſkin, Vol. II. p. 392.
Mulberry tree (native) the leaves of it unpleaſant to the native ſilk-worm, Vol. I. p. 253.
Muriatic and ſulphuric acids, of the ſolution of iron in, Vol. IV. p. 457.
Muſic, a new notation of, Vol. III. p. 139.
Muſkingum river, deſcription of ancient remains of art there, Vol. III. p. 214.

N

Naked-bear, account of, Vol. IV. p. 260.
Names of the officers and members of the Amer. Philoſ. Soc. Vol. I. p. xii.—Vol. II. p. xxiii.—Vol. III. p. xxviii.—Vol. IV. p. xiv.
Natchez, obſervations for determining the lat. and lon. of the town of, Vol. IV. p. 447.
Nancarrow (John) his calculations relating to griſt and ſaw-mills, for determining the quantity of water neceſſary to produce the deſired effect, when the head and fall are given in order to aſcertain the dimenſions of a new-invented ſteam engine, intended to give motion to water-wheels in places where there is no fall, and but a very ſmall ſtream or ſpring, Vol. IV. p. 348.
Natural hiſtory, method for preſerving ſubjects of, Vol. I. p. 244.
——— ——- enquiries in, Vol. III. p. xv.
——— *meadows*, ſome account of, Vol. III. p. 219.
Naval ſtores, combination between Ruſſia and Sweden to raiſe their price, Vol. I. *pref.* p. xiv.
Navigation, improvements in, Vol. I. p. 289, 293.
——— *inland*, propoſed improvements in, Vol. I. p. 293—Vol. IV. p. 299.
Negro girl, with a mottled ſkin, Vol. II. p. 392.
Negroes, their dark hue aſcribed to the leproſy, Vol. IV. p. 289—curious effects produced by their marrying with white women, 294—their ſkin turned white, 295—propoſal to excite among them a ſimilar change of colour by artificial means, 297.
Newcaſtle court-houſe (Delaware) its diſtance from Philadelphia, Vol. I. p. 93, 96.
New-England, population of, Vol. III. p. 43.
Newfoundland banks, ſuperior degree of cold in the water there, Vol. III. p. 93, 194.
New-Jerſey, its population, Vol. III. p. 35.
Niagara, account of an earthy ſubſtance there, called the ſpray of the falls —with remarks on the cataract, Vol. III. p. 17—Ellicott's deſcription of the ſame, Vol. IV. p. 227.

Nicola (Lewis) his method of preserving subjects in spirits, Vol. I. p. 244.
Nine-killer, a bird, described, Vol. IV. p. 124.
Nitre found and made, Vol. I. *pref.* p. xii.—mixed with water produces heat, and with snow, intense cold, *appendix* 33—other properties of, Vol. IV. p. 140.
Nitric acid, its composition, observations and experiments on the production of, Vol. IV. p. 472.
Nitrogen gas, what proportion in the atmosphere, Vol. IV. p. 128—observations on it, p. 6, &c. 418.
Non-descript animals, remains of, Vol. IV. p. 248, 259, 511.
Normandie (Dr. John De) his analysis of the chalybeate waters of Bristol (Penns.) Vol. I. p. 303.
Norriton observatory (Penns.) transit of Venus in 1769 observed at, Vol. I. p. 8, 13—its lat. fixed, 22, and lon. 59, *appendix* p. 2—account of the terrestrial measurement of difference of longitude between this observatory and that of Philadelphia, *appendix* p. 5.
North-America, diseases common to, Vol. III. p. iii. See *America*.
North-Carolina, described, Vol. III. p. 47—remains of a volcano in, p. 231.
North-west winds, why less frequent, Vol. I. p. 275.
North-western Territory, recapitulation of fine weather there during a certain period, Vol. IV. p. 347. See *Muskingum*, *Cincinnati*, *Niagara*.
Notation (a new) of music, Vol. III. p. 139.
Noxious vapours, in subterranean places, how easily dissipated, Vol. III. p. 324.
Numb-fish, or torporific eel, described, Vol. II. p 170.
Nutation of the earth's axis, how to calculate it, Vol. IV. p. 51.

O

O. (B.) his description of a new machine for cutting files, Vol. I. p. 300.
Oats destroy the wild garlic, enemy to wheat, Vol. I. p. 242.
—— (American) their qualities, Vol. I. p. 192.
Objects of the Amer. Philos. Soc. Vol. I. *pref.* p. xv.
Occultation of Aldebaran by the moon observed at Washington in 1793. Vol. IV. p. 48.
Ocean, its temperature at certain depths, Vol. III. p. 90 to 96.
Officers of the A. P. S. Vol. I. p. xii—Vol. II. p. xxiii—Vol. III. p. xxviii. Vol. IV. p. xiv.
Ohio river, great bones found there, Vol. III. p. 219—Vol. IV. p. 511—ancient hieroglyphics there, p. 195—very dense but not unwholesome fogs on the river, p. 225—thermometrical observations made there, p. 330—the daily rise and fall of its water, 340.
—— falls of, remarkable for petrifactions, Vol. III. p. 218.
Oil how affected on a surface of water, Vol. II. p. 310.
— mixed with water, a remarkable phenomenon produced from, Vol III. p. 14—explanations of it attempted, ib.
— of cotton-seed, its virtues, and how obtained, Vol. I. p. 235. See *Ol. Bombac.*

Oil of funflower feeds, account of, Vol. I. p. 234—effay on it, 235.
— *(Caftor)* recommended to Americans, Vol. I. p. 239.
— of benni-feed, account of, Vol. I. *appendix* p. 239—equal to Florence oil, ib.
Oils, from whatever vegetables, if obtained by expreffion, agree in their general qualities, Vol. I. p. 238.
Ol. bombac. [oil of cotton feed] how obtained, Vol. I. p. 235—medicinal in the cholic, ib.
Oliver (Andrew) his theory of lightning and thunder as oppofed to Dr. Franklin's, Vol. II. p. 74—his theory of water-fpouts, Vol. II. p. 101.
Opium, obfervations on, Vol. IV. p. 388—how and where obtained, 389—its component parts, 390, 394—compared with that extracted from garden lettuce, and various experiments made on both, 392 to 410—mode of extracting the latter, 411.
Optics, a deception in, explained, Vol. II. p. 37.
Optical problem, by Francis Hopkinfon, Vol. II. p. 201—and folved by Mr. Rittenhoufe, 202.
Orb of the earth, its eccentricity and diftance from the fun, Vol. I. p. 103.
Ores and minerals, variety of, in America, Vol. I. *pref.* p. xiii.
Organs of generation in plants afferted, Vol. III. p. 203.
Orthography and orthoepy, how to fix them, Vol. III. p. 262—imperfection of the former, 268—farther obfervations on orthography, 271—advantages of a new one, 272.
Orrery, defcription of a new one invented by Mr. Rittenhoufe, Vol. I. p. 1.
Otto (Dr.) his account of an oil drawn from funflower feeds, Vol. I. p. 234.
—- (Monf.) on the difcovery of America, Vol. II. p. 263.
Overfetting of fhips, a preventive againft, Vol. II. p. 302.
Owls venerated by certain nations, Vol. IV. p. 206.
Oxyds containing oxygen, Vol. IV. p. 458.
Oxygen, or dephlogifticated air, abforbed in the phlogiftication of air, Vol. IV. p. 1—obfervations on it, ib. to p. 11—in what oxyds contained, Vol IV. p. 458—combined with azote produces the nitric acid, 472.
——— *gas*, or pure refpirable air, what proportion of it in the atmofphere, Vol. IV. p. 128, 417—how to promote a fupply of it, ib. p. 141—mercury precipitated from it, 416—a principal ingredient in marfh air, 427.
Oyfter-fhell beds (foffil) in the Atlantic ftates, Vol. IV. p. 439.
——*fhells* of prodigious fize and vaft quantities in the Mobile country, Vol. III. p. 218—ufed in compounding Indian earthen ware, ib.

P

Page (John) his defcription of a meteor feen in Virginia, Vol. II. p. 173—letter to him from Dr. Rittenhoufe, with remarks on that meteor, 175.
Paints (valuable) Vol. I. *pref.* p. xii.
——- Vol. I. p. 256.
Pais de Vaud, proportion of deaths there, Vol. III. p. 41.

Papaver somniferum [white poppy]. See *Opium*.
Paraguay, account of the skeleton of a huge non-descript quadruped discovered there, Vol IV. p. 259.
Parallax of *Venus*, observations on it, Vol. I. p. 63, 102, 116, *appendix* 57.
——— of the *sun*, important to be known, Vol. I. p. 42—how to determine it, ib.—further observations on the, 65, 73, 75, 77, 102, 103, 105, *appendix* 3, 12, 54, 67—explained, 103—how to ascertain it with the greatest possible precision, *appendix* 40.
——— — —— and planets explained, Vol. I. p. 103.
Paris, births and deaths there compared, Vol. III. p. 56.
Parisian hypothesis of the eccentricity of the earth's orb from the sun, and distance, Vol. I. p. 103.
Parsnips, how to preserve them for any length of time, Vol. II. p. 199.
Partridge, with two hearts, Vol. II. p. 330.
Passage of herrings, Vol. II. p. 236.
Patterson (Rob.) his method of finding the true meridian line, and variation of the compass, Vol. II. p. 251—his explanation of a singular phenomenon, Vol. III. p. 13—on a new notation of music, 139—his improvement on lightning rods, 321 [*gained the Magellanic prize*]—his easy and accurate method of adjusting the glasses of Hadley's quadrant, on land, for the back-observation, Vol. IV. p. 154.
Peacock-flower described, Vol. III. p. 229.
Pea (common corn) excellent improver of soils, Vol. III. p. 227.
Pease secured against insects, Vol. I. p. 222—the time for sowing them and preventing the worm, 243.
Peale (C. W.) his merit in establishing a Museum, Vol. IV. p. 365.
Pearson (James) his account of the external contact of Venus with the sun, Vol. I. p. 50.
Pekin, in what similar to Philadelphia, Vol. I. *pref.* p. iv.
Penn (Tho.) sends a telescope and micrometer to the Amer. Philos. Society's committee for observing the transit of Venus, Vol. I. p. 10.
Pennsylvania legislature assist the society's committee, Vol. I. p. 10.
———, proposals for improving its inland navigation, Vol. I. p. 293—cause and encrease of bilious and intermitting fevers there, with hints for preventing them, Vol. II. p. 206, 290—its population in 1683, Vol. III. p. 35—and climate, 51—its extent from east to west, Vol. IV. p. 35—how bounded on the north, ib. p. 39—miscellaneous observations on its western parts, 224—crystallized basaltes found in, 445.
Pendulum, an improved one invented and described by Mr. Rittenhouse, Vol. IV. p. 27—wooden pendulums imperfect, ib. p. 29. curious experiments on the subject, ib. p. 30, 31.
Perkins (Dr. John) his conjectures concerning wind and water-spouts, tornadoes and hurricanes, Vol. II. p. 335.
Persimon trees, profitable—their wood hard and charged with salts, Vol. I. p. 233—yield a valuable gum, 234.
——— *fruit*, memoir on the distillation of, Vol. I. p. 231—produces a fine spirit, ib. *pref.* p. viii.—preparation of it for the still, 232—an excellent beer made from it, 234.

Petrifactions, frequent in the western country, Vol. III. p. 218.
——— (marine) intermixed with flint and limestone at L. Erie, Vol. IV. p. 226.
Pit-coal introduced into use in England, Vol. II. p. 19—neglected in France, 19, 20—used in China, 33—its smoke deemed salubrious, 20.
Phenomena, Vol. I. p. 283, 285—a singular one explained, Vol. III. p. 13—looming, 62—in magnetism the solution of, Vol. IV. p. 324.
——— attending the transit of Venus in 1769, Vol. I. p. 27, 28, 29, 46, 49, 50, 94, 101, 106, 109, 111, 115, *appendix* 12 to 20, 68.
——— of Mercury, Vol. I. p. 83.
Philadelphia compared with Pekin, Vol. I. *pref.* p. iv.—lat. of, Vol. I. p. 54—lon. of, 6, 59, 93, 96—difference between its meridian and that of Greenwich, 59—courses and distance from its observatory to that of Newcastle court-house, 92—difference of its lon. and lat. with those of Norriton, *appendix* 5—meteorological observations made there in 1770, p. 1. *appendix* 70.—births and deaths in, Vol. III. p. 37, 39, 44, 54, 56, 59—its size in 1683, p. 35.
Philological view of some very ancient words in several languages, Vol. IV. p. 476.
Philosophical society held at Philadelphia, their laws and regulations, Vol. I. p. v.—Vol. II. p. v.—Vol. IV. p. iii.—list of the officers and members Vol. I p. xiii.—Vol. II. p. xxiii.—Vol. III. p. xxviii.—Vol. IV. p. xiv.—charter of incorporation, Vol. II. p. xi.—law to encrease the deposits and subscriptions of the members, Vol. II. p. xix—extracts from minutes concerning the Magellanic prize, ib.—Vol. IV. p. vii.—See *American P. Soc.*
Phlogiston, observations and experiments concerning, Vol. IV. p. 1 to 11—not a simple substance, ib. p. 7—how formed, ib.—farther observations on it, p. 11 and 382—considerations thereon in opposition to Dr. Priestley, p. 452. See *Azotic gas.*
——— rejected, why, Vol. IV. p. 452.
Physico-mathematical subjects recommended to the attention of Americans, Vol. III. p. xiii.
Plague, sugar an antidote for, Vol. III. p. 75.
Planet, how to determine the true place of one, in an elliptical orbit, directly from the mean anomaly, by converging series, Vol. IV. p. 21.
Planets, their respective distances from the sun calculated, Vol. I. p. 62—Stuart's method of deducing their parallaxes and distances, 76. Their parallaxes explained, 103—method of calculating their eccentric anomaly, Vol. IV. p. 67.
Plants and seeds, directions for sending them abroad, Vol. I. p. 266, 269, 270.
——— (foreign) catalogue of such as merit attention in America, Vol. I. p. 255.
——— not originally natives of America, Vol. III. p. 241.
——— *(cryptogamick)* observations on, Vol. III. p. 202—their organs of generation asserted, 203—how regenerated, ib.—various opinions on the

the generation of, 205—their parts accurately defcribed, 211—the creative power of plants, 346.
Plant, a new one difcovered near Philadelphia, Vol. IV. p. 173.
Plico-Polonica of the Poles, a fymptom of leprofy, Vol. IV. p. 293.
Plough. See *Mould-board.*
Podophyllum diphyllum of Linnæus, botanically defcribed, Vol. III. p. 334.
Poifon of the rattle-fnake, how counteracted, Vol. III. p. 102, 104, 109, 114.
Poifonous plant, an account of one in Virginia, Vol. III. p. 234.
Pole-ftar and β Bootes, the right afcenfion and declination of, Vol. III. p. 116.
Poles of the earth fuppofed change of pofition, Vol. III. p. 12.
Polypi, opinion concerning their generation and reproduction, Vol. III. p. 204.
Ponds, method of draining them in level grounds, Vol. III. p. 325.
Popular diet of the Americans, the caufe of various difeafes, Vol. III. p. vi.
Population (Indian) See *Indian* population.
——— of N. America, obfervations on its vaft increafe and importance, Vol. III. p. 25—compared with that of other countries, 27, 134—doubled in 25 years, 42—in 15 and 20 years, 43.
Poppy (white) produce of it per acre, Vol. IV. p. 412. See *Opium.*
Prairies, fome account of, Vol. III. p. 219.
Preachers (illiberal) the bane of real virtue, Vol. III. p. vii.
Premiums, for what fubjects offered, Vol. IV. p. v.—to whom adjudged, p. vii.
Prefents to the A. P. Soc. Vol. II. p. 398—Vol. III. p. 351—Vol. IV. p. xxvii.
Preferving of meats, butter and milk, Vol. II. p. 30, 31.
——— of wine, Vol I. p. 183, 188, 190.
———, of parfnips, Vol. II. p. 199.
Prefqu' ifle, remarkable phenomenon there, Vol. III. p. 62—effect of the winds, Vol. IV. p. 225—fubject to heavy dews and fogs, ib.—its atmofphere moifter than in the middle Atlantic ftates, ib.—little limeftone there, 226—lat. of the town there determined, p. 231.
Prieftley's (Dr.) electrical battery defcribed, Vol. II. p. 93—his and Beccaria's ideas of electricity, 76—his experiments and obfervations relating to the analyfis of atmofpherical air, Vol. IV. p. 1—his farther experiments relating to the generation of air from water, ib p. 11 to 20—his appendix to, and correction of the two preceding articles, 382.
——— his doctrine of phlogifton rejected, 452.
Prior (Tho.) his account of the contacts in the tranfit of Venus, Vol. I. p. 49.
Prize Medal of Magellan, how and in what cafes to be awarded, Vol. II. p. xix. See *Magellanic* prize.
Problem (optical) by Mr. Hopkinfon, Vol. II. p. 201—folved by Mr. Rittenhoufe, 202.
Procefs for making maple fugar, Vol. III. p. 67.
——— ——— Eaft India fugar, ib. p. 79.
Projection of the tranfit of Mercury in 1769, Vol. I. p. 88.
Pronunciation, Dr. Johnfon's rule for it erroneous, Vol. III. p. 270—rules for the pronunciation of a new propofed alphabet, 280.

Providence

Providence (R. I.) aſtronomical obſervations at, Vol. I. p. 97—its longitude and latitude, 103.
Punctuation, obſervations on, Vol. III. p. 307—a new mode propoſed, 308.
Putrid ſore throat, &c. how cured, Vol. II. p. 288.
Putrifaction, excites heat, Vol. I. *appendix* p. 34.

Q

Quadrant (Godfrey's) an improvement of, Vol. I. *appendix* p. 21.
———— *(Hadley's)* an eaſy and accurate way to adjuſt its glaſſes, on land, for the back obſervation, Vol. IV. p. 154.
Queries concerning vapours, the electrical ſtate of the atmoſphere and aurora borealis, Vol. II. p. 100.

R

Rain, the effect of electricity, Vol. II. p. 100.
—— with fiſhes, frogs or tadpoles, accounted for, Vol. II. p. 117.
—— quantity of, at Bradford (N. Eng.) in 1773, Vol. II. p. 135—in Ireland, Vol. IV. p. 244.
Raiſins, method of making, Vol. I. p. 193.
Rarefied air explained, Vol. II. p. 2.
Rattle-ſnake [crotalus horridus] on the cure of its bite, Vol. III. p. 100—its poiſon how counteracted, 102, 104, 109—reputed cures for it, 114.
———— memoir on the faſcinating faculty aſcribed to this and other American ſerpents, Vol. IV. p. 74—other obſervations thereon, 363, 370.
———— curious particulars concerning the, Vol. IV. p. 366—different ſpecies of, 368—a new ſpecies deſcribed, ib.—winter retreats of, 374.
Red-bark [cortex ruber] medical hiſtory of, Vol. II. p. 289—more efficacious than common bark, 290.
Regeneration of plants, Vol. III. p. 203.
Regulations of the A. P. S. Vol. II. p. v.
Relaxation, the prediſpoſing cauſe of tetanus, Vol. II. p. 227.
Rigging and cables of ſhips, propoſed improvement in, Vol. II. p. 297.
Right aſcenſion and declination of β bootes and the pole-ſtar, Vol. III. p. 116.
Rittenhouſe (David) his new-invented orrery deſcribed, Vol. I. p. 1—his account of the tranſit of Venus, 8, 13, 26—he fixes the latitude of Noriton, 22—his delineation of the tranſit, 36—his account of a comet in 1770, *appendix* p. 37—his eaſy method of deducing the true time of the ſun's paſſing the meridian per clock, *appendix*, 47—his explanation of an optical deception, Vol. II. p. 37—his obſervations and conjectures on a falling ſtar or meteor, 175—his account of ſome experiments on magnetiſm, in a letter to John Page, Eſq. 178—his invention for placing a meridian mark, 181—his adoption of a ſpider's thread, inſtead of ſilk, a great improvement, 183—his obſervations on a lately diſcovered comet, 195—his ſolution of an optical problem ſtated by F. Hopkinſon, Eſq. 202—on the tranſit of Venus over the ſun's diſc, Nov. 12, 1782, p. 260—

p. 260—his and Dr. Jones's account of houſes in Phila. ſtruck by lightning, Vol. III. p. 119—his and Mr. Hopkinſon's obſervations on the effects of lightning on a houſe with two conductors, 122—his aſtronomical obſervations, 150—on the method of finding the ſum of the powers of the ſines, &c. 155—his account of a comet, 261—his mode to determine the true place of a planet, in an elliptical orbit directly from the mean anomaly, by converging ſeries, Vol. IV. p. 21—his improvement of time-keepers, ib. p. 26—his account of the expanſion of wood by heat, ib. p. 29—his method of raiſing the common logarithm of any number immediately, ib. p. 69.

River-navigation, improvement in its boats, Vol. IV. p. 298—inclined planes preferable to locks, 299—deſcription of a boat recommended to be uſed, 301.

Robinſon (Ebenezer) his eaſy and expeditious method of diſſipating noxious vapours in wells, &c. Vol. III. p. 324.

Rock, a curious one, Vol. II. p. 50.

Rocks artificially and curiouſly marked, Vol. III. p. 219—Vol. IV. p. 253.

——, &c. (the ſtrata of) beyond the mountains placed *horizontally*, Vol. IV. p. 227.

Roman (ancient) ſubſtitute for eſpaliers, Vol. I. p. 170—how they preſerved wine, 188.

Romans (Bernard) on the mariner's compaſs, Vol. II. p. 396.

Rome, proportion of deaths in, Vol. III. p. 41, 59.

Rooms, an improved mode of warming them, Vol. II. p. 29, 33.

Rules of the A. P. S. two fundamental ones, Vol. I. p. iii.

Rumſey's improvement on Barker's mill, obſervations on, Vol. III. p. 144.

Rural economy, enquiries into, and how applicable to the U. States, Vol. III. p. vii.

Ruſh (Dr. Benj.) on the cauſe of the encreaſe of bilious and intermitting fevers in Pennſylvania; with hints for preventing them, Vol. II. p. 206—on the late Dr. Martin's cancer-powder; with obſervations on cancers, 212—on the cauſe and cure of the tetanus, 225—on the ſugar-maple, and manner of obtaining the ſugar, Vol. III. p. 64—his obſervations intended to favour a ſuppoſition, that the black colour (as it is called) of the Negroes is derived from the leproſy, Vol. IV. p. 289.

Ruſk, the true original ſea biſcuit, Vol. II. p. 322.

Ruſſia and Sweden combine to raiſe the price of naval ſtores, Vol. I. *pref.* p. xiv.

Ruſſians unſucceſsful in obſerving the tranſit of Venus in 1769, Vol. I. *appendix*. p. 3.

Ruſton (Dr. Thomas) on the cauſe, cure and prevention of ſmoky chimnies, Vol. II. p. 231.

S

Saint Andrew, W. Florida (bay of) deſcribed, Vol. I. p. 253.

Saint Auguſtine, a moſt healthy climate, Vol. III. p. 49.

Salem

Salem (Maſſachu.) births and deaths at, Vol. III. p. 38, 56, 59.
Salt (marine) combined with vegetable acid, virtues of, Vol. II. p. 284.
Saltpetre mines, Vol. I. *pref.* p. xii.
Sand-hills of Cape Henry (Vir.) memoir on the, Vol. IV. p. 439.
Santa Roſa, W. Florida, lat. of, Vol. I. p. 253.
Sap of the ſugar maple, how excellent and uſeful, Vol. III. p. 69, 73.
Sargent (Winthrop) his letter to Dr. Barton, accompanying drawings and ſome account of certain articles which were taken out of an ancient tumulus, or grave, in the weſtern country, Vol. IV. p. 177.
Satellites of Jupiter, their eclipſes obſerved, Vol. I. p. 21, 55—their immerſions and emerſions as obſerved at Wilmington (Del.) and the weſtern obſerva. in 1784, Vol. IV. p. 32—at Baltimore in 1788, ib. p. 48.
Saturn's diſtance from the ſun, Vol. I. p. 62.
Savannah, in Georgia, proportion of deaths at, Vol. III. p. 59.
Saw-mills, particular deſcription of certain, Vol. IV. p. 350.
Scull, a new method of treating the effuſion that collects under it after fracture, Vol. IV. p. 433.
Sculptured rock on the Kenhawa, Vol. IV. p. 253.
Sea-veſſels; machine for pumping, without manual labour, Vol. I. p. 289.
Sea-water, warmeſt out of ſoundings, Vol. III. p. 83—its temperature compared, 194—ſtate of, 90, 200.
——*-air*, experiments on, Vol. IV. p. 262, 269, 272—more rarefied than land air, 269.
Seeds and plants, how to preſerve them for tranſportation, Vol. I. p. 266, 269, 270.
Sentinel, or ſelf-moving regiſter, for preſerving an equal degree of heat, deſcription of one, Vol. I. p. 286.
Serpents, M. de la Cepede's diſtribution of them into genera, Vol. IV. p. 379—their characters, 380—the mokkaſin, a non-deſcript ſnake, ib.
———, cures for their bite, Vol. III. p. 100, 102, 104, 109, 114—their faſcinating faculty controverted, Vol. IV. p. 74—obſervations on, p. 362.
Sexual organs of plants, Vol. III. p. 204—of moſſes and muſhrooms diſcovered by De Beauvois, ib.
Seybert (Dr. Adam) his experiments on land and ſea-air, Vol. IV. p. 262—his experiments and obſervations on the atmoſphere of marſhes, 415.
Shark's teeth (foſſil) abundant in Virginia Vol. IV. p. 433.
Sheep, various kinds deſcribed, Vol. IV. p. 149 to 154.
Shetland-wool peculiarly ſoft, Vol. IV. p. 433.
Shells. See *Foſſil*.
——*(prodigious oiſter)* in vaſt quantities in the Mobile country, Vol. III. p. 218.
——*(foſſil and petrified)*, numerous in the weſtern country, Vol. III. p. 218—and elſewhere, Vol. IV. p. 439.
Ship pumps, new method propoſed for working them, Vol. I. p. 289—bore to be enlarged, 290—powers of, 291.
——, an invention to aſſiſt her ſailing, Vol. III. p. 331.

——'s

Ship, a method to prevent their foundering, 301—propofed improvement in their rigging and cables, 297—objections to the modern conftruction of, 302—how to provide againft the accidents of overfetting, fire, lightning, running foul of others and ice at night, ib.
——*'s way*, a new machine for meafuring it, Vol. II. p. 159—Vol. III. p. 239—obfervations on, Vol. II. p. 294.
Shippen (Jofeph) his account of the contacts in the tranfit of Venus over the fun, Vol. I. p. 45.
Signals, an improvement on, Vol. IV. p. 162.
Silk cocoons of America (native) larger and the filk ftronger than Italian, Vol. I. *pref.* p. vi.
—— *worm*, its procefs from the egg to the cocoon, Vol. II. p. 347—natural hiftory of, 350—how to manage it, 351—cocoons of various kinds, 353—the filature, 361.
—— *worms* (native) Vol. I. *pref.* p. vi.—obfervations on them p. 224—more beautiful on every change, 228—averfe to change of food, 229—eafier to raife than the Italian worm, 230—great weight of their cocoons, ib.
—— *grafs*, or Indian hemp of America—*Q.* if not the Chinefe *herba* ? Vol. I. *pref.* p. vi.
Sines, how to find the fum of their powers, Vol. III. p. 155.
Siren, or inguana [firen lacertina] a new fpecies of, Vol. IV. p. 277. See *Inguana*.
—— operculata, a new fpecies of inguana difcovered, Vol. IV. p. 277—defcribed, 279.
—— an intermediate clafs connecting the amphibiæ with fifh, Vol. IV. p. 277.
Skeleton of an unknown quadruped lately difcovered in Virginia, Vol. IV. p. 236. See *Bones*.
Slate, and excellent freeftone on L. Erié, Vol. IV. p. 226.
Smith (Reverend Dr. Wm.) his communication of calculations and projections of the tranfit of Venus in 1769, Vol. I. p. 4—his account of that tranfit, 8—and of the contacts, 29—of Mafon and Dixon's meafurement of a degree of latitude, 96—of the terreftrial meafurement of the difference of lon. and lat. between Philadelphia and Norriton obfervatories, *appendix* p. 5—of the tranfit of Mercury in 1769, *appendix* p. 50—his deduction of the fun's parallax from a comparifon of the American obfervations of the tranfit of Venus in 1769, with European obfervations of the fame, p. 54.
—— *(Thomas P.)* his account of a kettle for boiling inflammable fluids, Vol. IV. p. 431—his account of cryftallized bafaltes found in Pennfylvania, p. 445.
Smoke of burnt bones, deftructive to bugs, &c. Vol. I. p. 222.
——, why it afcends, though heavier than air, Vol. II. p. 2—how to burn it, 33, 57—burning it a great faving of fuel, 67—ufeful for heating the walls of a hot houfe, 74.
Smoky chimnies, caufes of, Vol. II. p. 5, 10, 12, 13, 14, 15, 16, 17, 232—how to remedy them, 6, 11, 12, 14, 15, 17, 18, 23, 232.
Snake in a horfe's eye, Vol. II. p. 385.
—— *(rattle)* on the cure of its bite, Vol. III. p. 100—how to counteract its poifon, 102, 104, 109—reputed cures for it, 114—on its fuppofed fafcinating faculty, Vol. IV. p. 74. See *Serpents*.

Snow and froſt, decreaſed of late in N. America, Vol. I. p. 275.
Sore throat, how cured, Vol. II. p. 288.
Sounds in the Engliſh language, a table of, Vol. III. p. 292—from what borrowed, 296.
South-Carolina climate, Vol. III. p. 48.
Southern colonies, directions concerning the culture of the grape in, Vol. I. p. 157.
Soy, or India ketchup, method of preparing it, Vol. I. p. 259.
Spaſms, how cured, Vol. II. p. 225.
Speedy elevator, a new invented machine for gaining inacceſſible heights, &c. deſcription of a, Vol. IV. p. 519.
Spice-wood, excellent in ſome diſeaſes, Vol. I. *pref.* p. ix.
Spider, its thread placed in a tranſit teleſcope by D. Rittenhouſe, with vaſt effect, Vol. II. p. 183.
Spirito Sancto, E. Florida (bay of) capable of receiving firſt rate ſhips of war, Vol. I. p. 253.
Spirits, an eaſy method of preſerving ſubjects in, Vol. I. p. 244.
——— *of perſimons*, Vol. I. p. viii.—*appendix*, p. 231,—Vol. III. p. x.—of currants, 249.
——— *of wine* convertible into inflammable air, Vol. IV. p. 19.
——— *(ardent)* obtained from the ſugar-maple, Vol. I. *pref.* v. and from perſimons, *pref.* viii.—*appendix* p. 231.
Spring-block. See *Block*.
Standard for weights and meaſures, Vol. III. p. 328.
Stirling (Lord) his account of the comet of 1770, Vol. I. *appendix* 45.
Staffordſhire chimney, or fire-place, deſcribed, Vol. II. p. 25.
Star Arcturus, his great celerity of motion weſtward, Vol. II. p. 224.
Stars (attendant) diſcovered, Vol. II. p. 222—uſeful for determining the motion of fixed ſtars, ib.
——— how to calculate their aberration, Vol. IV. p. 51.
Steam-engine, a new invented one deſcribed for giving motion to water-wheels in certain ſituations, Vol. IV. p. 355—its ſuperior advantages, 357.
Steel, rubbed againſt ſteel, produces magnetiſm, Vol. II. p. 180.
Stove (Chineſe) deſcribed, Vol. II. p. 33—propoſed addition to it for burning the ſmoke, ib.
Stoves, great ſavers of fuel, Vol. II. p. 20, 35.
——— *(German)* deſcribed, Vol. II. p. 34.
——— for burning of pitcoal and conſuming its ſmoke, Vol. II. p. 57.
Strata of earths beyond the Atlantic mountains lie *horizontally*, Vol. IV. p. 227.
Sub-marine veſſel, a new invented one, Vol. IV. p. 303—curious experiments thereon, 308, 310, 311—various attempts made on Britiſh ſhipping, 310—the Britiſh fire on and ſink it, 311—origin of the battle of the kegs, 312.
Subterranean places, how to diſſipate noxious vapours from, Vol. III. p. 324.
Süe (Monſ.) his manner of preparing anatomical ſubjects in wax, Vol. II. p. 381.
Sugar, very nouriſhing, Vol. III. p. 74—its virtues and uſes, ib.—an antidote for the plague, Vol. III. p. 75—manner of making it in the Eaſt Indies, 79.

Sugar-

Sugar-maple tree fap, yields a fine fpirit, Vol. I. *pref.* p. v.—and excellent vinegar, p. 73. See *Acer faccharinum*, and *Maple*.
Sulphuric and muriatic acids, of the folution of iron in, Vol. IV. p. 457.
Sumach tree fuppofed to yield the gum copal—propofed as a dye—the leaves mixed by Indians with their tobacco, Vol. I. *pref.* p. ix.
Sun, caufe of his rays producing heat, Vol. I. *appendix* p. 34—his parallax important to be known, ib. p. 42—how to determine it, ib. and *appendix* 40—the parallax deduced from a comparifon of American obfervations on the tranfit of Venus with thofe of Europe, p. 54—method for afcertaining it firft conceived by Dr. Halley, ib.—an eafy mode of deducing the true time of the fun paffing the meridian per clock, *appendix* p. 47—his diftance from the planets, p. 62—parallaxes, 65, 73, 75, 77, 78—do. explained, 103—mean diftance from the earth, ib.—horary motion, 86—diameter, 52, 79, 84, 101—eclipfed, 113.
—, obfervations on the tranfit of Mercury over the, Vol. III. p. 150.
—, eclipfes of, Vol. II. p. 239, 250.
Sun-flower oil, account of, Vol. I. p. 234—effay on expreffing it, 235.
Supplementum indicis floræ Lancaftrienfis, Vol. IV. p. 235.
Surinam (torpedo of) defcribed, Vol. II. p. 166, 170.
Swamp, definition of the word, Vol. IV. p. 440.
Swatara, defcription of a grotto there, Vol. II. p. 177.
Sweden, fome account of its climate, Vol. III. p. 52—proportion of deaths in, 59.
Swedes unfuccefsful in viewing the tranfit of Venus in 1769, Vol. I. *appendix* p. 3.
Sweet fprings of Virginia, obfervations on, Vol. II. p. 197.
Swimming anchor to keep a veffel from off a lee fhore, propofed and defcribed, Vol. II. p. 311.
Syllables, obfervations on, Vol. III. p. 299.

T

Tadpoles falling in a fhower of rain, the caufe of, explained, Vol. II. p. 117.
Tar and hog's-lard ufeful to preferve grape vines, Vol. I. p. 164.
Tatham (Col. Wm.) his obfervations on the country below the falls of James river (Vir.) Vol. IV. p. 444.
Teeth and bones (foffil) account of fome found in Virginia, Vol. IV. p. 439.
Telegraph, an effay on one, Vol. IV. p. 162.
Temperature (prefumable) in comets, Vol. I. *appendix* p. 29.
———— of Ireland, Vol. IV. p. 244.
Tenneffee river, petrifactions there, Vol. III. p. 219—curioufly marked rocks on a branch of, ib.
Teft (infallible) for detecting the prefence of arfenic, Vol. II. p. 214.
Tetanus, on the caufe and cure of, Vol. II. p. 225—predifpofed by relaxation, 227—not confined to the human fpecies, 230.
Theory of thunder and lightning, Vol. II. p. 74.
——— of water-fpouts, Vol. II. p. 101.
——— of the earth, queries and conjectures concerning a, by Dr. Franklin, Vol. III. p. 5.
——— of water-mills, &c. obfervations on, by W. Waring, Vol. III. p. 144—continued, 319.
——— of calculating the aberration of the ftars, the nutation of the earth's axis, and the femi-annual equation, Vol. IV. p. 32.

Thermometer, exceeded not 95° in Virginia, Vol. II. p. 143—at 100° in Philadelphia, ib.
———, its uſe in diſcovering banks, ſoundings, &c. at ſea, Vol. III. p. 82, 198.
Thermometrical journals of the water and air at ſea, Vol. III. p. 90, 200.
——— obſervations at Fort Waſhington, Cincinnati, in 1790–1.
Thomas (Rich.) and others, their account of Venus's tranſit over the ſun in 1769, Vol. I. p. 89.
Tin known to the Florida Indians, Vol. IV. p. 202.
Thomſon (Charles) his obſervations on the internal contact in tranſit of Venus, Vol. I. p. 50, *appendix* 38.
Thornton (Dr. Wm.) his treatiſe on the elements of written language, &c. with an eſſay for teaching the dumb to ſpeak [*gained the Magellanic prize*] Vol. III. p. 262.
Thunder and lightning, a theory of, Vol. II. p. 74.
Time-keepers, Rittenhouſe's improvement of, Vol. IV. p. 26.
Tolteca Indians, ſome account of them, Vol. IV. p. 184.
Tornado deſcribed, Vol. II. p. 342, 346.
Torpedo, or electrical eel of Surinam deſcribed, Vol. II. p. 166.
Torporific eel, or numb fiſh deſcribed, Vol. II. p. 170—its power compared with electricity, 172.
Torricellian vacuum, magnetic experiments in the, Vol. IV. p. 327.
Towns, greateſt proportion of deaths in, Vol. III. p. 41.
Trade-wind, defined, Vol. I. p. 273.
Traditions (*Indian*) Vol. III. p. 216, 220—Vol. IV. p. 207, 209, 260, 516—not to be depended on, ib.
Trajectory of the comet's path in 1770, with the elements of its motion, Vol. I. *appendix*, p. 37.
Tranſit of Venus over the ſun in 1769, as obſerved at different places, Vol. I. p. 4, 5, 8, 13, 26, 28, 29, 32, 45, 48, 49, 50, 82, 89, 97, 105, 114—*appendix* p. 1—Vol. II. p. 246—delineations of the tranſit, Vol. I. p. 36, 66—account of obſervations on the former, 4, 2—obſervations on it, made at Hudſon's bay, *appendix* p. 4—all the obſervations made thereon, collected together, *appendix* 12.
——— ——— in 1761—obſervations on it, Vol. I. p. 67.
——— ——— in 1769—do. Vol. I. p. 82—projection of this tranſit, 88—another account of the ſame tranſit, *appendix* 50—how uſeful towards fixing the longitude of places, *appendix* 53. See *Venus*.
——— of Mercury, in 1769, Vol. I. *appendix* p. 50—Vol. II. p. 249—in 1782, Vol. II. p. 260—Vol. III. p. 150.
Trees contribute to health, Vol. II. p. 209—the willow a great purifier of air, ib.
—— recommended in certain ſituations, Vol. IV. p. 142.
Trepanning, obſervations on, Vol. IV. p. 433.
Trichoma [Plico-Polonica] a ſymptom of leproſy, Vol. IV. p. 293.
True honey-bee, of America, Vol. III. p. 241.
Tulip-tree [liriodendron tulipifera] ſtimulant effects of camphor upon it, Vol. IV. p. 232.
Tumuli, in the N. W. T. &c. curious diſcoveries made therein, Vol. IV. p. 179, 203, 205.

Turbith

Turbith mineral, not a pure oxyd, but a ſulphate of mercury, Vol. IV. p. 453.
Turner (Judge) his thermometrical obſervations made at Fort Waſhington, commencing June 1790, and ending April 1791—to which are added, for ſome time, the riſe and fall of the Ohio, Vol. IV. p. 329, 334—his memoir on the extraneous foſſils denominated mammoth bones: principally deſigned to ſhew, that they are the remains of more than one ſpecies of non-deſcript animal, Vol. IV. p. 510.

U

United States of N. A. an eſſay on thoſe enquiries in natural hiſtory which at preſent are moſt beneficial to them (viz.)
Medical enquiries, Vol. III. p. iii.
Rural economy vii.
Phyſico-mathematical xiii.
Natural hiſtory xv.
Meteorology xxv.
——— population of, Vol. III. p. 25, 42, 134—in what period doubled, 43—proportion of inhabitants under 16 years, ib.—particular inſtances of longevity in, 44—account of climates in, vi, 43, 47, 51—Neptunian origin of their ſea coaſt, Vol. IV. p. 439, 445.
Univerſal magnetic needle, Vol. I. p. 254.
——— alphabet recommended, Vol. III. p. 263.
Unquiculated quadruped, the bones of an unknown one diſcovered in Virginia, Vol. IV. p. 247.
Utenſils, uſed in making of wine, Vol. I. p. 195.

V

Vapour, query concerning, Vol. II. p. 100—produced in cold air, experiments on, Vol. III. p. 125.
——— (noxious) in wells, &c. an eaſy and quick method of diſſipating, Vol. III. p. 324.
Variation of the compaſs, how to find it, Vol. II. p. 251.
Vegetable acid, combined with marine ſalt, its virtues, Vol. I. p. 284.
——— *kingdom* in N. America, obſervations on it, Vol. I. *pref.* p. viii. 250.
——— *productions* of the U. States, Vol. III. p. x.
——— ——— all reproduced by their own particular organs, Vol. I. p. 211.
Vegetables, ſtimulant effects of camphor on, Vol. IV. p. 232, 234.
———, on the early condition of, Vol. IV. p. 503.
Velocity of light, Vol. I. *appendix* p. 36.
——— (extraordinary) of the comet of 1770, Vol. I. *appendix* p. 37, 46.
Venereal diſeaſe, Indian cure for it, Vol. I. p. 250.
Venus, calculations and projections of her tranſit in 1769, Vol. I. p. 4, 5, 26, 89—accounts of the tranſit, 8, 13, 26, 28, 29, 32, 45, 48, 49, 50, 82, 89, 97, 105, 114, 246, *appendix* p. 2, 12, to 20—delineations of it, p. 36, 66—ſurrounded by a denſe atmoſphere, 49—obſervations

tions on her transit in 1761, p. 67—her diameter, 52, 79, 84, 101, *appendix* 4—mean distance from the sun, ib.—observations on her parallax, 63, 102, 116, *appendix* 57. See *Transit*.

Vessel, how to assist her sailing, Vol. III. p. 331.

—— (a sub-marine) principles and construction of, Vol. IV. p. 303—curious experiments with, 308, 310, 311.

Vesuvius, account of its eruption in 1767, Vol. I. p. 281—most remarkable eruptions of, 285.

Vienna, proportion of deaths, &c. at, Vol. III. p. 57.

Vinegar of the sugar-maple excellent, Vol. I. p. 73.

Vines, essay on the culture of, and of making wine in N. America, Vol. I. p. 117—climate well adapted, 120—productiveness, 121—how to plant and manage them, 123—and form a nursery, 125, 134—the vines most suitable, 126—selection of cuttings when to procure and preserve them over winter, 128—planting them and preparing the ground, 130—summer culture of them the first year, 137—autumn culture the first year, ib.—do. the second year, 140—do. the third year, 142—culture in their bearing state, 143—new mode of trimming them, 144—general directions, 146—concerning the trimming, ib.—number of branches to be retained, 147—when to uncover the vines in spring, and how to form proper heads, ib.—transplanting, 149—espaliers the first year, 151—second do. ib.—third do. 153—fourth do. 156—directions to the Southern Colonies, 157—Portugueze mode of culture, 159—of dunging, 160—a rich manure, 161—guarding the fruit against boys, birds and wasps, 163—how to prevent the effects of grub-worms, &c. 164—to be planted alone, and at what distance, 165—bands for binding them to the stakes, 170. See *Vineyards*.

—— *(American)* described, Vol. I. p. 192.

Vineyards, their importance in America, Vol. I. p. 119—productiveness and cultivation, 121—proper situation, soil, and treatment, 123—manuring, 124—nurseries, 125—particular vines to be chosen, 126—method of laying out vineyards, 131, 135—how to preserve the soil on hill sides, 149—site to be chosen, 150—how to gather in the vintage, 170—art of making the wine, 171. See *Vines*.

Virginia, its population, Vol. III. p. 42—climate, 51—barometrical measurement of the Blue-ridge, Warm-spring and Alleghany mountains there, Vol. IV. p. 216—other observations thereon, 222—unknown bones found there, 246, 439—description of the Sand-hills of Cape-Henry, and the natural formation of the country below the falls of its rivers, 439—of Neptunian origin, ib. 444.

Vocabularies, existing alphabets ill calculated for, Vol. III. p. 265.

Volcano, a supposed one in N. Carolina, Vol. III. p. 231.

W

War, interesting reflexions on, by Mr. Jefferson, Vol. IV. p. 320.

Waring (Wm.) on the theory of water-mills, &c. Vol. III. p. 144—his investigation of the powers and a description of Barker's mill, as improved by James Rumsey, 185—his continuation of the subject of water-mills, with an explanation of a passage in his former communication, 319.

Warm Spring (Virginia) barometrical meaſurement of, Vol. IV. p. 216.
Was-iſt-das deſcribed, Vol. II. p. 9, 10.
Waſhington (city of) account of the manner of laying it out, and running the 10 miles ſquare, Vol. IV. p. 49.
Water, experiments on the generation of air, from Vol. IV. p. 11.
——-, the difference between pure and impure, in the preſervation of dead fleſh, Vol. IV. p. 131.
——-, why warmer or colder than land, Vol. I. p. 274—compared to a ſponge, Vol. II. p. 75—experiments on the evaporation of, 118—great attraction to heat, 144.
——- how to find its retarded velocity, and time of aſcent into an exhauſted receiver, through a tube by the aſſiſtance of the parabola, Vol. IV. p. 353.
——- ſtate of at ſea, Vol. III. p. 90, 200.
Water-mills, on the theory of, Vol. III. p. 144, 185, 319. See *Mills*.
——- rattle-ſnake. See *Rattle-ſnake*.
——-*ſpouts*, theory of, Vol. II. p. 101—occaſioned by whirlwinds, 116—a beautiful account of one, 102—curious effect of one, 116—conjectures concerning them, 335—ſeveral deſcribed, to prove that their water deſcends, 336.
Wax, not produced by the bee only, Vol. III. p. 244.
Way of a ſhip, a new invention for meaſuring it, Vol. III. p. 239.
Weights and meaſures, new ſtandard for, Vol. III. p. 328.
Wells (Richard) his account of a new invented machine for pumping leaky veſſels at ſea, without manual labour, Vol. I. p. 289.
Wells, how to diſſipate their vapours, Vol. III. p. 324.
Weſt (Benjamin) his account of the tranſit of Venus as obſerved at Providence (R. I.) 1769, Vol. I. p. 97.
Weſtern country of N. America, antiquities, &c. of, Vol. III. p. 214—prodigious oiſter ſhells found there, 218—alſo abundance of other foſſil ſhells, concretions, petrifactions, bones, &c. ib.—prairies, 219—former height of the Miſſiſſippi, ib.—lofty rocks curiouſly marked, ib.
Wheat, deſtroyed by a fly weevil, with means for preventing it, Vol. I. p. 205—on the ſame ſubject, 218.
Whip-poor-will, ſome account of, Vol. IV. p. 208.
White mountains of N. Hampſhire, deſcribed by Rev. Jeremy Belknap, Vol. II. p. 42 to 49.
Whirlwinds, the cauſe of water ſpouts, Vol. II. p. 111—deſcribed under the name of wind ſpout, 342.
Williams (Rev. Dr. Samuel) his experiments on evaporation and his meteorological obſervations, Vol. II. p. 118—his account of the tranſit of Venus, June 3, 1769, p. 246—of Mercury, Nov. 9, 1769, p. 249—his obſervation of a ſolar eclipſe, Nov. 6, 1771, p. 250—his magnetic obſervations at Cambridge, Maſſachuſetts, in 1785, Vol. III. p. 115.
Williams (Jona.) his communication on Captain Billings's thermometrical journal of the air and water at ſea, Vol. III. p. 194—on the uſe of the thermometer, in diſcovering banks, ſoundings, &c. p. 82—his thermometrical journal of the temperature of the atmoſphere and ſea, 90—his barometrical meaſurement of the Blue-Ridge, Warm-Spring and Alleghany mountains in Virginia, Vol. IV. p. 216.

Williamson (Dr. Hugh) his account of the contacts in the transit of Venus, in 1769, Vol. I. p. 48—his determination of the sun's parallax, 78—his essay on comets, and the origin of heat, *appendix* 27—his attempt to account for the change of climate in the middle colonies, 272.
Wilson (Rev. Matt.) on the severity of the winter of 1779-80, Vol. III. p. 326
Willow tree, a great purifier of air, Vol. II. p. 209.
Wind (Easterly) in hot weather accounted for, Vol. II. p. 91.
—— ———the cause of epidemic inflammatory diseases in Ireland, Vol. IV. p. 244.
—— *spout* described, Vol. II. p. 342.
Winds, properties of the N. American, Vol. I. *pref.* p. iv.—their cause, p. 273—why the north-west less frequent than formerly, 275—prevailing ones at Cape Henry (V.) Vol. IV. p. 440.
Winds on L. Erie commonly resemble the sea and land breezes of the W. Indies, Vol. IV. p. 224—in what directions blown, ib.
Wine recommended to be made in America, Vol. I. *pref.* p. ix. 119—essay on the making and preserving it, 118—concerning white wine, 172—red do. 175—how to improve weak wines, 178—boiling the must, ib.—vessels proper to be used, 179, 180—also utensils, 195—effect of lees on wine, 180—curious and useful experiment, 185—principles of wine, 186—old Roman and other methods of preserving wine, 183, 188, 190, observations on American wine, 192.
—— *(currant)* recipe for making it, Vol. I. p. 248.
—— juice, yields a fine spirit, Vol. I. p. 249.
Wistar (Dr. Caspar,) on evaporation in cold air, Vol. III. p. 125—his experiments on evaporation, Vol. IV. p. 72,
Wood, its expansion by heat, Vol. IV. p. 249.
Woodhouse (Dr. James) his answer to Dr. Joseph Priestley's considerations on the doctrine of phlogiston, and the decomposition of water; founded upon demonstrative experiments, Vol. IV. p. 452.
Wool-bearing animals, a disquisition on, Vol. IV. p. 149.
Worm in a horse's eye, Vol. II. p. 183.
—— *(Guinea)* several yards long, Vol. II. p. 389.
—— *(a jointed,)* 20 inches long and near 3 in circumference, in the liver of a woman of Pennsylvania, Vol. II. p. 390.
—— living in a dog, ib.
——*s* in the human kidnies, &c. Vol. II. p. 391—sugar obnoxious to, Vol. III. p. 75.
—— in the brain, ib.
Wright (Wm.) account of the antiseptic virtues of vegetable acid and marine salt combined, in putrid disorders, Vol. II. p. 284.
—— *(Thomas)* on the mode most easily and effectually practicable of drying up the marshes of the maritime parts of N. America, Vol. IV. p. 243.

Y

Youghiogeny river, a remarkable rock and cascade near it, Vol. II. p. 50.

Z

Zanthoxilum, a tree of the southern colonies, a powerful stimulant of the glands of the mouth, Vol. I. *pref.* p. viii.