





AGRICULTURAL RESEARCH INSTITUTE  
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PHILOSOPHICAL  
TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

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V O L. LVI. For the Year 1766.

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M.DCC.LXVII.



# ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

## ADVERTISEMENT.

should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of nature or art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons, through whose hands they receive them, are to be considered in no other light, than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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T O

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Year	Time	Par. of the Microm.	Parts of a gr. Circle.	Mag. of the Ec. Dig.	Remarks
1765	Aug. 16				
	0 45	c   2484	31 41 c		Diameter of the Sun measured in the Parallel.
	3 52	12   2486	31 42 3c		Diameter of the Sun.
	3 58	13			Beginning of the Eclipse to a Second. The Sky clear about the Sun.
	4 7	57   2316½	29 33* c	o 2c	Magnit. of the Eclipse.
	4 9	57   969	12 21 3c		Distance of the Horns.
	4 13	33   2200	28 3* 3		Magnit. of the Eclipse.
	4 15	0			The Sun covered.
	4 20	2   2144	27 21* c		Magnit. of the Eclipse measured without a Glass, light Clouds.
	4 22	3   1275	16 16 c		Dist. of the Horns, measured without a Glass, light Clouds.
	4 25	27   2063	26 19*		Magnit. of the Eclipse measured without a Glass.
	4 28	0			Thick Clouds covering the Sun.
	5 20	c			The Sun appears again, but the Eclipse is over.

The quantities marked with \* are the remaining bright parts of the diameter of the sun measured in a direction perpendicular to the line of the horns.

II. *Remarks on the Palmyrene Inscription at Teive. In a Letter to the Rev. Thomas Birch, D. D. Secretary to the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Jan. 23, 1766. **T**HE Palmyrene inscription at Teive having been inaccurately taken by Sig. Pietro della Valle, the transcript published in the *Philosophical Transactions* must be looked upon as incorrect, and consequently the explication of that inscription, which the Royal Society did me the honour (1) formerly to publish, cannot in all points be intirely depended upon. Having therefore been informed, that the stone itself, brought a few years since out of the East, was in the possession of the Right Honourable the Earl of Besborough; I resolved to attempt getting a sight of it, that a true copy of so curious a monument might in proper time be imparted to the learned world. Having opened my design to John Wood, Esq; member of parliament for Brackley in Northamptonshire, a gentleman of great merit and erudition, he carried me, with the utmost politeness and good nature, to Lord Besborough's house in Cavendish-Square, May 21, 1764; where I had a full view of the stone, examined the in-

(1) *Philosoph. Transact.* Vol. XLVIII. Par. II. p. 746-751.  
scription











scription with all the attention I was capable of, and took the transcript exhibited here upon the spot. In order therefore to rectify all former mistakes, occasioned by Sig. Pietro della Valle's blunders, I judged it might not be improper to communicate this, together with a new Latin and English version of it, attended by a copy of the correspondent Greek inscription, as it appears on the stone, to the Royal Society; submitting to the consideration of that illustrious body, with all possible deference, the short remarks transmitted you in this paper.

For the Palmyrene inscription, see TAB. I.

The inscription in Hebrew or Chaldee Characters.

לבעל שמיך מרא עלמא קרב  
כסתא וערשא אנתגלם

JUVI FVLMINATORI IN AETERNVM sit REVERENTIA—OPERIMENTVM ET LECTVM ei DEDICAVIT AGATHANGELVS.

TO JUPITER THE THUNDERER FOR EVER be REVERENCE—AGATHANGELVS DEDICATED to him this COVERED BED.

For the correspondent Greek inscription, as it appears on the stone, see TAB. I.

Remarks on the Palmyrene Inscription.

1. That Baal, the great divinity of Syria and Phœnicia, answered to the ZEVS of the Greeks, and the JUPITER of the Latins, as we find intimated by the two inscriptions before me, is acknowledged by  
some

some of the most celebrated ancient writers; and has been clearly evinced by (2) me, in a former paper.

2. The word שֶׁמֶטֶץ, SHEMETZ, sometimes denotes NOISE, or SOUND, according to (3) Schindler, Schmidius, and Cocceius. It is likewise taken in Scripture for A VOICE IN THE AIR (4), as we learn from Stockius. BAAL SHEMETZ, therefore, may be rendered DOMINVS MVRMVRIS SONI, SVSVRRI, VCI VOCIS IN AERE, THE LORD OF THE NOISE, OF THE SOUND, OR OF THE VOICE IN THE AIR; and consequently may be deemed perfectly equivalent to ΖΕΥΣ ΚΕΡΑΥΝΙΟΣ, or JUPITER THE THUNDERER, in the correspondent Greek inscription. A new figure of *Izade* here presents itself to our view, which has not been handed down to us by any other monument of the Palmyrenes.

3. The following word, מִרָא, or מִרְרָא, TIMOR, REVERENTIA, VENERATIO, &c. appears, in the very same signification, (5) in two Palmyrene inscriptions, some years since by me explained. Nothing can be more common in the oriental languages than the ellipsis, or suppression, of the verb (6) substantive, which occurs here.

(2) *Philos. Transf.* Vol. XLVIII. Par. II. p. 748, 749.

(3) Vid. Schindlerum Schmidium, Cocceium, Jo. Christ. Clod. *Lex Hebraic. Select.* p. 509, 510. Lipsiæ, 1744. & Jo. Leonhard. Reckenberger. *Lib. Radic. sive Lex. Hebraic.* p. 1515. Jenæ, 1749.

(4) Christ. Stock. *Clav. Ling. Sanct. Vet. Test.* p. III5. Jenæ, 1721. It must be remarked, that the word SHEMETZ, as explained here, will greatly illustrate two passages in the book of Job, (IV. 12. XXXVI. 14.) and decide in favour of the explication of those passages given by Schmidius and Cocceius.

(5) *Philos. Transf.* ubi sup. p. 698.

(6) Johan. Buxtorf. *Tesaur. Grammat.* p. 472. Basilæ, 1663.

4. With regard to **עלמא**, of the Chaldee or Syriac form, it will be sufficient to observe, that it answers here to **AETERNITAS, PERPETVITAS, &c.** So that **מורה עלמא** may be rendered **REVERENTIA AETERNITATIS, or REVERENTIA AETERNA.** The term **עלמא** may likewise be considered as equivalent to **ל'עלמא**, IN **AETERNVM**, the particle **ל** being not improbably here understood. Such ellipses as this were by no means uncommon in the eastern world, as we find clearly evinced by (7) Noldius.

5. Nothing farther is requisite to be observed of the Hebrew, Chaldee, or Syriac verb **קרב** (8), **OBTVLIT, LIBAVIT, DEDICAVIT, &c.** than that it seems perfectly consonant to the tenor of the inscription, and conveys to us the same idea that is exhibited to our view by the word **ANEOHKEN**, in the correspondent Greek inscription.

6. The next word **כסתא**, or **כסותא** (9), **OPERIMENTVM**, is intirely Syriac. It may not be improper to remark, that the letter *Thau* here is of a somewhat unusual form; and that the *Vau* is understood, or suppressed, after the Phœnician manner. The figure likewise of the *Sameck*, unless part of it has been effaced by the injuries of time, does not perfectly agree with any of those characters that have been hitherto considered as forms of that element.

7. The copulative *Vau*, that follows, and connects **כסתא** with the substantive **ערישא**, or **עריסא**, **LECTVS,**

(7) Christian. Nold. *Concordant. Particular. Ebræo-Chaldaicar.* p. 416, 417. Jenæ, 1734.

(8) Vid. Johan. Buxtorf. *Sen. et Jun. Val. Schindl. aliosq. Lexicograph. Hebr. Chal. Syr. &c.*

(9) Johan. Buxtorf. *Jun. Lex. Chald. & Syriac.* p. 260. Basileæ, 1622.

SPONDA LECTI, &c. which is both a Chaldee and a Syriac word, renders the latter part of the inscription sufficiently intelligible. For OPERIMENTVM ET LECTVM here may be considered as equivalent to LECTVM OPERTVM, or perhaps simply LECTVM, as the correspondent Greek word has been translated by Dr. Bernard. The character representing *Vau*, prefixed to the word I am now upon, agrees with the figure of that element, as it occurs in my second Palmyrene (10) alphabet. For a farther account of these beds of state, as well as several other similar inscriptions, recourse may be had to the (11) author here referred to.

8. The last word of the inscription is apparently אנתגלס, ΑΓΑΘΑΝΓΕΛΟΣ, AGATHANGELVS, the name of an Abilenian, who erected a cupola, or camera, and placed under it a bed of state, dedicated to Jupiter Maximus Fulminator, or the supreme God Jupiter the Thunderer, for the health and safety of the emperor Hadrian, his sovereign. This happened, according to the correspondent Greek inscription, in the 445th year of the æra of Seleucus, and the 17th of that prince's reign. The word ΑΓΑΘΑΝΓΕΛΟΣ, AGATHANGELVS, seems to be of nearly the same import with ΑΓΑΘΟΔΑΙΜΩΝ, AGATHODAEMON, the name of an (12) Alexandrian geographer of pretty considerable note. It also occurs in (13) one of Gruter's inscriptions, but is evidently a cognomen there. The

(10) *Philos. Transact.* Vol. XLVIII. Par. II. p. 740.

(11) Sellar. *Antiquit. of Palmyr.* p. 364, 369.

(12) Agathodæm. Alexandrin. per Mercator. et Bert. Amst. 1618. Vid. etiam Agathodæm. Alexandrin. *Delineat. Orb. ex Lib. Ptolemæi*, Lat. Bas. 1552.

(13) Jan. Gruter. *Inscript. Romanar. Corp.* p. 644. 1.

character



character representing *Ghimel*, in this word, is somewhat different from all the other figures of the same letter, that have hitherto appeared.

9. The Palmyrene alphabet deducible from this inscription [see TAB. I.] being curious, as the forms of several of it's letters cannot be deemed the same with those of the correspondent elements in any of the Palmyrene alphabets hitherto published; I should be thought guilty of an omission, did I not take the liberty to insert it here. The characters representing *Ghimel*, *Samech*, *Tzade*, and *Tbau*, in particular, differ considerably from the figures of those letters on every other monument of the Palmyrenes.

As I have spoken pretty largely of this inscription in a former paper, I shall not expatiate any farther upon it here; but at present only beg leave to assure you that I am, with all due sentiments of respect and esteem,

Sir,

Your most faithful,

and most obedient,

humble servant,

Christ-Church, Oxon.  
Nov. 28, 1765.

John Swinton.

Received December 3, 1765.

III. *A Letter to William Heberden, M. D. Fellow of the Royal College of Physicians in London, and of the Royal Society, from Daniel Peter Layard, M. D. Physician to her Royal Highness the Princess Dowager of Wales, Member of the Royal College of Physicians in London, and of the Royal Societies of London and Gottingen; giving an Account of the Somersham Water, in the County of Huntingdon; and transmitting a Letter from Michael Morris, M. D. F. R. S. Member of the Royal College of Physicians in London, and Physician to the Westminster Hospital, to Dr. Layard, on the same Subject.*

Dear Sir,

Read Feb. 6, 1766. **A**T last I venture to lay before you the result of those experiments and observations, which I have made on the Somersham water. They were undertaken with your approbation, and pursued through your encouragement.

To ascertain the contents of a mineral water, requires a repetition of the analysis; and notwithstanding the experiments have been repeated these fourteen years last past, either at the spring, at Huntingdon,

don, or in London, and the effects of this water carefully observed in the many cafes it has been drank, yet I could not before presume to offer you a positive determination, till I was convinced, by the trials of an abler and more competent judge, that I was not mistaken. But now through the obliging and friendly assistance of Dr. Morris, I am enabled to communicate to you, and the public, the great utility and excellence of a medicinal water, well known and esteemed many years ago. By a repetition also of every experiment made in the country, Dr. Morris, has in his laboratory, and in my presence, ascertained the contents and properties of the Somersham water; nay more, as you will observe, Sir, by Dr. Morris's letter annexed, one considerable ingredient, namely allum, has been not only proved to exist in this water, but likewise the Dr. has produced crystals of allum, which Doctors \* Lucas and Ruddy † declare, have not yet been procured from any water, although allowed to contain an aluminous salt.

I shall not trouble you, Sir, with a detail of all the common experiments so well known to you, and which I had the satisfaction of shewing you, and many gentlemen of the different branches in the profession of physic, at my house the two last summers; but proceed to an account of the water, and it's contents.

The Somersham water, commonly called the Somersham Spa, issues out from the declivity of a small hill, which is situated on a heath, bearing the same

\* Vol. II. p. 24. Essay on Waters.

† P. 299. Synopsis of Mineral Waters.

name, and laying near the high road, between the towns of St. Ives and Somersham, in the county of Huntingdon, about three miles distance from St. Ives. This heath was formerly covered by part of the royal forests cut down in the reigns of Henry II. III. or of Edward I. and now serves only for the grazing of sheep.

The different strata of the earth on this heath are, immediately under the sward, first, about six inches depth of mould, or arable, then different strata of clay, each stratum about ten or twelve inches deep, growing darker from a yellowish or grey color, to a dark blue golt the deeper it lies. At about seven feet deep is found a bed of gravel, out of which the water springs forth very clear. This stratum of gravel is about twelve or fourteen inches thick, surrounded with a bed of a very dark blue golt, beset with large quantities of selenites, which have shot in it, and are surrounded with some fine yellow clay sticking to them.

The water, flowing from this spring,, which is perennial, but runs more or less according to the different seasons, is received from the bed of gravel by three small brick channels, about two feet long, which meet in one of about six inches square. This channel which is near twenty feet in length, conveys the water to a basin also of brick, and about two feet square. The channel and basin were made about 40 years ago under the direction of the late Rev. Dr. Knight; were opened and cleaned in the years 1755 and 1759, when I directed the workmen to new lay the bricks of the channel in some of the stiff blue clay, instead of lime-mortar, that the water might be

be less liable to a decomposition in its passage. From the basin the water frequently overflows, and runs trickling along a trench into a small brook; wherever it stops, it leaves the clay ground tinged of a rusty ochry color, and the water stagnating is covered with a thick pellicle variegated of many colors. In frosty weather, I have often found stalactites hanging round the edges of the basin, which, upon examination, were formed by the selenites impregnated with the vitriol of iron, changing their white appearance to a rusty yellow-colored crust covering the stalactites.

The water taken up out of the basin, is generally clear and transparent, unless after heavy rains, when it appears thick and of a muddy yellow; or when the basin has not been cleaned some time, lumps of a black gelatinous substance, like the sediments in ink bottles, are taken out from the bottom of the basin.

By dipping carefully a glass into the basin in a dry season and fair weather, the water is quite clear, full of bubbles sparkling up, some of which stick to the sides of the glass. By the hydrostatical balance, its weight differs from distilled rain water, weighed at the well, as 1006  $\frac{1}{2}$  is to 1000; when carried to Huntingdon, as 1010, to 1000; and brought to London, it has about the same weight.

The Somersham water, drank at the spring, is cool, pungent, and of an austere, sharp, astringent, ferruginous taste, somewhat inky, but not in the least disagreeable; when carried to any distance, it loses a little of its pungency, by its suffering a decomposition; but carefully bottled under water, and then

then well corked, covering afterwards the corks closely with rosin and wax, the water preserves its briskness and volatility a long while, and provided the bottle be kept corked, though half the water may have been drawn out, yet after keeping it months, nay many years, it will still preserve its irony principle, so as to turn with galls, purple, or dark blue.

*Experiments at the Spring Head.*

Experiment I.

Half a grain of powdered galls turns a pint of Somersham water of a dark purple.

Experiment II.

Brandy, or rum, drawn from oak casks, or an infusion of green tea leaves, turns it of a blackish cast.

Experiment III.

Vegetable, nor mineral acids, cause the least alteration in the water, which remains clear, and without the least effervescence.

Experiment IV.

Alcalies, whether volatile or fixed, cause an effervescence and turn the water green and curdled.

Experiment V.

It mixes with milk without alteration.

Experiment

## Experiment VI.

Syrup of violets turns the water green.

*Experiments at home, in Huntingdon.*

## Experiment VII.

Half a grain of galls powdered turned a pint of Somersham water of a dark mazarine blue, inclining to purple.

## Experiment VIII.

In two vials were poured some Somersham water, each vial containing eight ounces. In one had been put two drams of filings of iron. After standing twenty-four hours in a moderate warm place, three grains of powdered galls were added to each vial. The pure Somersham water immediately turned purple, with the galls, which next day fell like a purple sediment to the bottom, and left the supernatant liquor clear. The water poured on the filings of iron threw up several air bubbles, turned black as ink, some few hours after the addition of the three grains of galls; and kept that color, and an inky taste, several days; after which the black sediment fell likewise to the bottom, and left a clear supernatant water, which still preserved its inky taste.

Experiment

## Experiment IX.

Brandy, or rum, drawn from oak casks, or an infusion of green tea leaves, produced a purple color, inclining to black.

## Experiment X.

Somersham water doth not lather with soap, but curdles immediately.

## Experiment XI.

Acids cause no alteration, but mix as at the spring head.

## Experiment XII.

Volatile or fixed alcalies did not effervesce with the Somersham water, at home, as at the spring head, but produced a cloud, then curdled, and threw down a sediment.

## Experiment XIII.

The Somersham water, in a few hours, lets fall an ochry sediment to the bottom of the bottle, which is of a different color, according to the season in which the water is taken up; in dry times, the sediment is of a rusty, cinnamon, or orange color; in wet weather, it is of an olive, or brown color, smelling like the fumes of sulphur.

Experiment



## Experiment XIV.

After the first separation of the ochry sediment, the water continues clear, and transparent; and notwithstanding I have kept some bottles half full, months, and years, yet the water preserved its purity, and would in a lesser degree turn purple with galls.

## Experiment XV.

The water being filtered from the ochre, which was precipitated at the bottom of the bottle, and then put on the fire, emits many air bubbles, then grows turbid and yellow, and after a little evaporation throws up a thick scum, which separated by filtration proves to be a fine ochre beset with selenites. The water will then, after boiling, turn purple with galls. By accident I let fall some hot Somersham water on an iron fender, which it instantly turned to so many blue spots, as there were drops on the fender.

## Experiment XVI.

After this second filtration, the water appears greenish at the top, and lets fall a white sediment; when the liquor is evaporated to nearly the quantity of half an ounce, a thick pellicle is formed.

## Experiment XVII.

If to this residuum a small quantity of distilled rain water be added warm, and then filtered, a very

white sediment will be separated; and the rain water being evaporated to a pellicle, will leave a brown deposit, which will appear to be a bitter muriatic salt, containing sometimes a few crystals.

### Experiment XVIII.

The Somersham water mixes well with warm and boiling milk, when carried at a distance as well as at the spring head; but when equal quantities of the water and milk are boiled together, then the milk is turned immediately, the curd of which becomes of a bright pink color with galls, the whey of a reddish brown, and both are of an agreeable rough and ferruginous taste.

### Experiment XIX.

To discover if the water contained any allum, according to Dr. Shaw's direction, some leaves of the herb *Geranium Robertianum*, commonly called *Ragged Robert*, were infused in four ounces of Somersham water, a comparison made between this infusion, and one of the same herb, in the same quantity of distilled rain water, and another in as much distilled rain water, with two grains of allum powdered. The glasses were all placed in a moderate heat; and the several infusions appeared, as follows, after standing twenty four hours; the Somersham water of a purple color, with a reddish hue; the rain water, with allum, of a green, with a reddish cast; and the rain water of a fine green. The herb subsides with the Somersham water, floats

in the middle with the aluminous rain water, and swims on the top in the simple rain water. The herb is turned purple, by infusion, in the Somersham water; is of a yellow green in the rain water with allum; and remains of a bright green when infused in rain water alone.

### Experiment XX.

The sediments, by evaporation, from Somersham water differ, as is said, of the spontaneous separation, in Experiment 13. according to the seasons, both as to color and quantity. Four pounds of the Somersham water have, by evaporation, yielded in a dry season, sixteen, or at most twenty grains of a rusty colored, or orange colored sediment; whereas, after rainy weather, two pounds of the same water have yielded one dram of a dark olive brown sediment.

### Experiment XXI.

I put two drams of the yellow ochry sediment into a crucible, covered with a tile, and having calcined it in an open fire, it had lost a sixth part of its weight; the residuum appeared of a red rusty color, mixed with a white earth; and on the sides of the crucible a small quantity of greyish powder sticking to it. The red powder was partly attracted by the magnet.

### Experiment XXII.

I put, at the same time, into another crucible, a lump of the blue clay, brought from the Somersham Spa,  
 D 2 weighing

weighing about two ounces ; this clay was intermixed with small fhoots of selenites, surrounded with the fine yellow ochry clay mentioned above. After calcination, part of the clay had vitrified, part was become of a dark red, and the selenites were burnt to a white powder.

### Experiment XXIII.

I threw some of the white sediment, procured by filtration and evaporation mentioned in Experiment 17. on a red hot iron ; it partly blistered up, and turned of a greyish color, and the remainder, which was the greater quantity, appeared of a very clear white.

### Experiment XXIV.

Having, by evaporation and filtration, procured some selenites from the Somersham water, I threw it into a red hot crucible, wherein it partly swelled up like blisters, and when cold appeared of a greyish white, intermixed with particles of iron.

These Experiments plainly demonstrate, Sir, that the following contents are to be found in the Somersham water. First, by Exp. 1, 2, 6, 7, 8, 9, 13, 14, 15, 18, 20, 21, 24, iron. Secondly, by Exp. 13, 16, dissolved pyrites. Thirdly, by Exp. 1, 3, 4, 10, 11, 12, 14, 16, 18, a vitriolic acid. Fourthly, by Exp. 22, 23, 24, a calcareous earth. Fifthly, by Exp. 15, an ochre. Sixthly, by Exp. 15, 16, 21, 22, 23, 24, selenites. Seventhly, by Exp. 17, a muriatic salt, which doth not crystallize, And, Eighthly, by Exp. 18, 19, 21, 23, 24, allum.

The

The Somersham water, therefore, is a chalybeate water, strongly impregnated with the vitriol of iron and allum, and containing some calcarious earth, felenites and salt.

It would be absurd in me to trouble you, Sir, either with any more experiments, or the relation of the several cases, in which I have found the salutary effects of the Somersham water, since no one knows its properties better than you, Sir, nor directed it formerly with greater efficacy. I shall, therefore, now lay before you Dr. Morris's letter, in confirmation of what I have said, and only mention, that upon the several trials made with this water on human calculi, both at Huntingdon and in London by me, and lately by Dr. Morris, the Somersham water has acted powerfully on those substances; that it mixes well with the blood, and passes visibly with the urine. But these experiments, and the above-mentioned cases, trite as they must be to you, may probably, with other particulars relating to the county of Huntingdon, be laid before the public at some other time.

I am happy in this opportunity of acknowledging myself, with the greatest regard,

Dear Sir,

Your most obliged,

and most obedient humble servant,

Lower Brook-street,  
Nov. 15, 1765.

D. P. Layard.

*A Letter from Michael Morris, M. D. Member of the Royal College of Physicians in London, and of the Royal Society, to Daniel Peter Layard, M. D. Physician to Her Royal Highness the Princess Dowager of Wales, Member of the Royal College of Physicians in London, and of the Royal Societies of London and Gottingen, relating to Experiments made on the Somersham Water.*

Dear Sir,

FROM your very accurate account of the singular mineral water, found at Somersham, in Huntingdonshire, and its salutary effects in many obstinate diseases, it appears to be highly deserving the attention of the public. I, therefore, agreed with pleasure to your proposal of repeating the experiments here, which you had formerly made at the spring head, and at Huntingdon; and adding such as you were obliged to omit in the country for want of a proper apparatus.

As you intend to oblige the public with an account of the former, to which our repeated trials here were perfectly conformable, I shall confine myself in the following narrative, principally to the latter, at which you assisted in my laboratory.

*Experiments on the Somersham Water.*

The water seemed clear in the bottles, though there was some sediment at the bottom. It was clear and bright when poured into a glass, but did not sparkle nor emit air bubbles for a considerable  
time

time. It was austere, subacid, and chalybeate to the taste.

### Experiment I.

On adding five grains of powdered galls to a glass of the water, it soon became of a muddy blue, which in a little time changed to a light purple.

Several bottles of the water, which had been kept upwards of two months, exhibited the same appearances.

The water, in some bottles, which had been half emptied on purpose, and corked slightly, still preserved its property of striking a blue and purple with galls, though more faint.

From these experiments, the Somersham water appears manifestly to have preserved its ferruginous quality, notwithstanding the long carriage from the spring to London, and to have lost very little of it for a considerable time after, which renders it a valuable acquisition to the public, as there are very few mineral waters, generally known in England, that do not lose their chalybeate properties in a few days, and even at a small distance from their source; which renders the importation of the foreign chalybeate waters absolutely necessary, at a considerable price.

### Experiment II.

Two pounds of the Somersham water were exposed to a gradual fire in a glass retort luted to a receiver,

The water, as it grew warm, became turbid, and discharged air bubbles, depositing at the same time an ochry sediment at the bottom. These appearances ceased before eight ounces were drawn off by distillation.

The distilled water, proving on examination, to be no wise different from common distilled water, the distillation was discontinued; the contents of the retort were poured into a white basin; on standing a night to settle, the liquor in the basin seemed clear, and a yellow sediment was seen at the bottom; the clear liquor was decanted into another basin.

The sediment carefully dried, weighed near four grains, and proved to be chiefly ferruginous.

The clear liquor was suffered to evaporate, by the heat of the atmosphere, in the month of August; pellicles were formed successively on the surface, which breaking in a little time fell to the bottom; this continued until there remained about two ounces of liquor, which was poured carefully from the pellicles into a cup, and was set to evaporate in a moderate heat. The dried pellicles weighed 30 grains. They were insipid, gritty, and not soluble in water.

### Experiment III.

Six grains of the pellicles, exposed to a strong fire, in a covered crucible, for three hours, became reddish when cold, and lost a grain in weight; but seemed not altered in other respects.



## Experiment IV.

Six grains of the same pellicles exposed on a test to a reverberatory heat for the same space of time; the vitriolic acid being volatilized, by the reverberated flame, was expelled from its terrene basis, so that the residuum, when cold, weighed but three grains, was acrid to the taste, grew hot with water, and communicated the same qualities to it that lime does. Hence the saline pellicles deposited by evaporation, appear to be the selenites, or the vitriolic acid, united to a calcarious earth, with a little iron.

## Experiment V.

The liquor in the cup being quite evaporated, there remained some regular crystals standing in a whitish powder; the crystals weighed five grains, and proved on examination to be regular crystals of allum.

## Experiment VI.

The whitish powder soon attracted moisture from the air, and in the space of 24 hours ran, *per deliquium*, into a brownish subacid austere liquor, of a ferruginous and saline taste.

It appears from these experiments, that the contents of the Somersham water are: First, Iron. Secondly, Selenite. Thirdly, Allum. Fourthly, From its taste, and attracting the moisture of the air, some marine salt, with a little allum and vitriol

in the state of an *aqua magistra aluminis & vitrioli*, incapable of crystallization.

The Somersham water, therefore, seems to differ considerably from any of the mineral waters known in Great Britain or Ireland\*. For among the writers, who even admit and mention aluminous waters, no one, except Dr. Short, pretends to have separated crystals of allum from them; and even he declares, that in subsequent trials on the Nevil Holt water (from which he had once obtained some) he could not succeed.

Though if we consider that allum is composed of the vitriolic acid, united to an argillaceous earth, it will not be difficult to conceive that an acid water, passing through a stratum of such earth, should act upon, and unite with a small portion of it; or the water may dissolve some allum in the stratum of decomposed pyrites, where it is impregnated with iron and selenite. So that probably allum has been often overlooked in water wherein it existed; nor did we obtain any in our experiments, until nearly all the selenite had been separated.

However, as allum is a very powerful medicine; the quantity discovered in the Somersham water, must have contributed not a little to its efficacy, in some of the remarkable cases wherein you have observed its success.

I am,

S I R,

Your obliged humble Servant,

Michael Morris.

\* Rutt's Synopsis of Mineral Waters, p. 299. Lucas Essay on Waters, Vol. II. p. 24.

Received December 12, 1765.

IV. *Account of an inedited Coin of the Empress Crispina. In a Letter to the Rev. Thomas Birch, D. D. Secretary to the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

S I R,

Read Feb. 13,  
1766.

**A**N inedited Greek coin of the empress Crispina, which seems to have had a place formerly assigned it in the cabinet of the celebrated Professor Ott, and from thence afterwards to have passed into that of his son, the late Reverend Mr. Ott, some years since fell into my hands. The medal is nearly of the size of the middle Roman brass, and tolerably well preserved. The workmanship is somewhat rude, and favours sufficiently both of the age and the remote province in which it first appeared. On one side is exhibited the head of Crispina, wife of the emperor Commodus, attended by the Greek legend ΚΡΙСПΕΙΝΑ ΣΕΒΑΣΤΗ, CRISPINA AVGVSTA; and on the reverse two human figures, one sitting in a chair, with a lance in its left hand, and the other standing by its side, present themselves to our view. They are both

surrounded by the inscription ΔΑΡΔΑΝΟΕΕΗΝΩΝ, DARDANOSSENVVM, or DARDANOSSENSIVM, which evidently points at the inhabitants of some antient town. Not one of the letters, either of the legend or inscription, has suffered greatly from the injuries of time.

Who the Dardanossians were, or in what part of the world situated, I must not take upon me absolutely to decide; the word ΔΑΡΔΑΝΟΣΣΑ, DARDANOSSA, not appearing, as the name of a city, in any antient writer. But that this word occurred, in such a sense, in the original text (1) of Ptolemy, and was afterwards converted by some ignorant transcriber into DARANISSA, which still remains in all the printed and manuscript copies of that author, will, I persuade myself, not be contested by the critics of the present age. The coin therefore was struck at Dardanossa, or Daranissa, which seems to have been a town seated in Sophene, a province of the Greater Armenia, in the reign of the emperor Commodus, where the Roman power at that time prevailed. And this is consonant to the faith of history (2), from whence we learn, that the conquest of Armenia was effected, after the reduction of Artaxata, by Statius Priscus, not many years before Commodus ascended the imperial throne. Nay, the whole country is said to have been conquered, and reduced to the form of a Roman province, in the days of Trajan. The figure of the *Sigma* likewise on this medal, so similar to the form of that element on certain Armenian

(1) Ptol. *Geograph.* Lib. V. c. 13.

(2) Dio, Lib. LXVIII. Jul. Capitolin. in *Marc.* & in *Ver.* Lucian. p. 347. Jamblichus apud Photium, p. 242.

coins of the same age, will bring a fresh accession of strength to the notion here proposed to the consideration of the learned; if it will not, in conjunction with what has been offered, evince this beyond the possibility of a doubt.

As the medal before me has never been hitherto published, nor perhaps ever seen in any other cabinet, either of the curious or the learned, I was inclined to believe, that a description of it, as well as the draught of it attending this paper, [See TAB. I.] might not prove altogether unacceptable to the Royal Society; especially, as it enables us to emend the corrupted proper name of a town in Ptolemy, and evinces Dardanossa, or Daranissa, to have been subject to Commodus when he presided over the Roman world. Nor can any thing, as the authority of MSS. must give way to that of antient coins and inscriptions, be better supported than such an emendation. Nor have any coins of this city been ever discovered before. All which will, I flatter myself, be deemed a sufficient apology for the trouble given on this occasion by,

S I R,

Your most obedient humble Servant,

Christ-Church, Oxon.  
Dec. 7, 1765.

John Swinton,

*V. Observations*

Received December 5, 1765.

V. *Observation of the Eclipse of the Sun, of August 16, 1765, made at Leyden, by Professor Lulofs, F. R. S. to Charles Morton, M. D. Sec. R. S.*

Carolo Morton, M. D. Soc. Reg. Lond. Secretario, & Academiæ Imperial. Leopoldin. & St. Petropolitæ. Socio. Joannes Lulofs, Academ. Leidens. Matheseos Professor. Societ. Reg. Lond. Socius, &c. S. P. D.

Read Feb. 13,  
1765.

**A** Bbas de la Caille (*Ephemerides des Mouvements Celestes pour l'année 1765*) initium deliquii pro urbe Amstelædamensi determinaverat 5<sup>h</sup> 4' post merid. finem 5<sup>h</sup> 24', quantitatem 0 dig. 5'. Cum de la Caille conspirabat peritissimus de la Lande, maximamque eclipsos phasin locum habituram prædixerat in eadem urbe 5<sup>h</sup> 15'.

Hisce determinationibus plus fidens, quam rudioribus, quas ipse tantum feceram, ut typum deliquii in usum observatorii nostri parare mihi liceret, (secundum quas initium esset Leydæ 4<sup>h</sup> 10', finis 5<sup>h</sup> 10', quantitas 1<sup>d</sup> 22') aliisque insuper negotiis distractus, observatorium conscendi hora 4, sed, disposito telescopio Newtoniano septem pedum, eidemque adplicato micrometro Bradleyano, admirandus percipi, hora 4 29' 1" eo-usque jam proventam fuisse Lunam,

Lunam, ut distantia cornuum deliquii  $19' 36''$  adæquaret; unde quantitas obscurationis jam erat 2 dig. &  $38'$ ; ab eo tempore non admodum increvit eclipsis, ita ut non ultra 2 dig. &  $40'$  vel  $42'$  ascenderit. Hora autem  $4 44' 48''$  distantiam cornuum denuo deprehendi  $19' 36''$ , adeoque medium deliquii non longe abfuit ab hora  $4 36' 55''$ .

Reliquæ observationes circa distantiam cornuum, & magnitudinum obscurationis exinde erutarum determinationes, sunt sequentes.

Temp. ver.	distantia cornuum	Magnitudo.
		dig.
$4^h 51' 45''$	$18' 51''$	2 24
55 55	18 6	2 11 $\frac{1}{2}$
59 19	17 21	2 7
5 3 51	15 50	1 38
7 9	14 20	1 20
12 42	11 18	0 48
17 21	6 2	0 13 $\frac{1}{2}$
18 58 Finis		

Datum Lugd. Batav. d. 5 Novemb. 1765.

VI. *A Letter from James Parsons, M. D.  
F. R. S. to the Right Honourable the Earl  
of Morton, President of the Royal Society;  
on the the double Horns of the Rhinoceros.*

My Lord;

Read Feb. 27,  
1766. **W**HEN I had the honor of lay-  
ing my natural history of the  
Rhinoceros before this learned Society in 1743,  
which is printed in number 470, page 523, of the  
Transactions, I had not an opportunity of shewing  
a double horn to the members; I have, therefore,  
taken this first occasion to entertain the present mem-  
bers with a sight of a noble specimen of the horns  
of an African Rhinoceros, brought from the Cape  
of Good Hope, by my curious and worthy friend  
William Maguire esquire, among many other cu-  
riofities; presuming that few of the Society have  
ever seen a pair of the like kind. But what renders  
this subject the more particular, and worthy of ob-  
servation, is that, by means of knowing there is a  
species of this animal, having always a double horn  
upon the nose, in Africa, Martial's reading is sup-  
ported against the criticism of Bochart, who changed  
the true text of that poet, in an epigram upon the  
strength of this animal; for when Domitian ordered  
an exhibition of wild beasts, as it was the custom  
of



of several emperors. The poet says: The Rhinoceros toss'd up a heavy bear with his double horn:

*Namque gravem gemino cornu sic extulit ursum.*

and as Bochart knew nothing of a double horn, he changed this line both in reading and sense, thus:

*Namque gravi geminum cornu sic extulit eorum.*

as if two wild bulls were tossed up into the air, by the strong horn of the Rhinoceros.

Mr. Maittaire adopted the notion of a single horn, but was of opinion that the *geminum eorum* of Bochart ought to have been plural, *geminos euros*, as being more elegant; and he was followed by Doctors Mead and Douglas, with this difference, that these changed the *euros* for *ursos*, as imagining they were rather bears than bulls, that were thrown up by this noble animal.

Our then worthy president Martin Folkes Esquire, had seen my account of this subject, at the end of which, I endeavoured, however presumptuously, to defend Martial's reading against Bochart and the other eminent persons mentioned; and desired I would let it be read and printed, which I very readily agreed to, as his request did me much honour.

Before my paper was printed, Mr. Maittaire and Doctor Douglas died; and the learned Doctor Mead was the surviving critic, upon this line, of the three. Upon this occasion, therefore, I have a double pleasure; first, in amusing the present gentlemen with a most curious specimen in natural history; and, secondly, in remembering, in this place, the nice candor and generosity of Doctor Mead upon that subject. For, about four months after the paper was printed, he received a present of several cu-

rious shells, seeds, &c. and with them the bones of the face of a young Rhinoceros, with two horns *in situ*, all intire, by a captain, of an African trader, who brought them from *Angola*.

As soon as he saw the horns, he sent to invite me to breakfast, and there, in company, ingenuously gave up his past opinion, and declared for Martial; and, indeed, I must add to the praise of that great man, that, as I was happy in being frequently at his house, I was witness to many such instances of the most disinterested candor and generosity, where any part of science was the topic, among his select friends.

This anecdote I thought proper to mention upon the present occasion; nor can too much be said to his honour, among all lovers of philosophical learning. I am,

Your Lordship's  
most obedient Servant,

James Parsons.

P. S. The figure of the double horn of the Rhinoceros here described is seen in TAB. II. The dimensions are as follows; viz. The length of the anterior horn, measuring with a string along the convex fore part, is 20 inches; perpendicular height 18; circumference  $21 \frac{1}{2}$  at the base; the posterior horn is in perpendicular height  $19 \frac{1}{4}$ ; circumference round the base 18; length of both bases together upon the nasal bones 14; and the weight of both together is 14 pounds 10 ounces.

The Rhinoceros of the year 1739, described in the Transactions, was three years old; and the horn not three inches high; and hence by comparing that with this, one may imagine this to be many years old, perhaps above twenty; and that this animal lives to a great age.

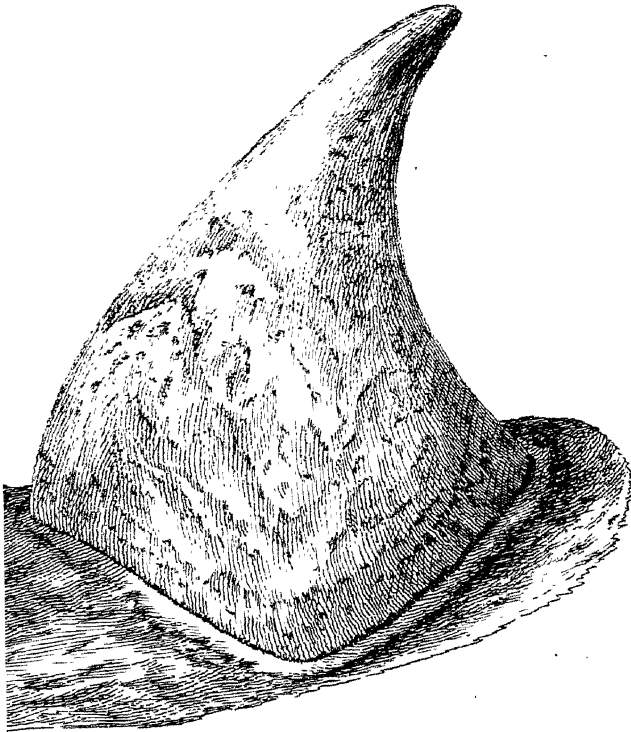
It is also plain that the horns are perpetual as are those of oxen,





*H. Parsons del.*

*The Double Horn of an  
African Rhinoceros, brought  
from the Cape of Good Hope,  
by William M. Guire Esq.*





Received February 20, 1766.

VII. *Extract from Two Letters, dated December 7th and 12th, 1765, from the Rev. Mr. William Borlase, of Ludgvan, in Cornwall, F. R. S. to Emanuel Mendes da Costa, Librarian, &c. to the Royal Society.*

Dear Sir;

Read March 5,  
1766. **O**N board the Spackman tin ship, I have sent a contribution to the fossil collection of the Royal Society, which I beg you will present with my duty and respects to that illustrious body; it is

N A T I V E T I N,

a rarity indeed! and I shall not be easy till I hear it is safely lodged in your Museum, as it is the fairest specimen I have seen.

As the existence of Native Tin is absolutely denied by all mineralists both antient and modern; and at the time I wrote my Natural History of Cornwall, having no evidences before me to evince the contrary, I contented myself with suggesting, page 185, that its existence was far from being improbable, and in that manner I left the dispute undecided. But a late fortunate discovery, which has furnished me with

three specimens of this metal native, or pure, will now exclude all further doubt.

The account of it is as follows. In the month of May last was found near St. Austle, by some streamers, a large cake, or nodule, of tin ore, weighing about six pounds, irregular in shape, cracked or jagged at the edges, lying about five feet under the surface, and in the middle of that stratum of tin ore, so remarkably spread in the moor adjoining to the forementioned town (*vide* Natural History of Cornwall, page 163). When the lump was broke, it appeared to consist of two coats, or incrustations, surrounding the whole, and of a nucleus or central substance of a quartz intermixed with the purest malleable tin.

The first specimen is now lodged in my desk of Cornish fossils at the Museum in Oxford. The outmost crust was about  $\frac{1}{4}$  of an inch thick at a medium, and of a brownish straw color; the second or inner coat was blacker, closer grained, with some faint appearances of whitish specks interspersed, and about  $\frac{2}{3}$  of an inch thick; these two coats inclosed a third substance, consisting of laminated crystals, rising side by side out of an edging, which shines like melted tin, and lies as it were at their roots coherent to the second coat. These crystalline laminæ are thin almost as the flakes or scales of talc, and being shot in a great variety of directions intersect each other, and leave a vast number of cells, within which are plainly seen, and may be cut freely with a knife, many specks and granules of pure native tin.

The second specimen, which I have the honor to present to the Museum of the Royal Society, is  
of



of the same structure, and part of the above-described lump, but is much richer in quality. It was sent me on the 12th of August last by Mr. Henry Rosewarne, of Truro, a gentleman well versed in the knowledge and fusion of metals. Besides all the appearances of native tin taken notice of, in the former specimen, here, in this N<sup>o</sup> 2. we see the malleable tin, in color equal to the finest tin of the furnace, more liberally and distinctly dispersed. The metal is not only found in granules, but in a foliaceous manner issuing out of the quartz, and formed like a thick, jagged, or scolloped lace or edging, of which the specimen itself only can give the justest idea.

The lump, or nodule, of which these two specimens are fragments, was so richly impregnated with tin, that though the best tin ore, in general, will not melt without flux, nor do twenty pounds of black tin usually produce more than fourteen pounds of white, this melted without flux, and twenty ounces produced eighteen ounces of the purest tin.

The third specimen is as follows. On the 17th of July last was found in a stream work near the borough of Granpont, and two days after brought to the above-mentioned Mr. Henry Rosewarne, by Jonathan Crowle, tinner, another lump of the same kind of tin ore as the former; its weight between eleven and twelve pounds; the native tin was inclosed so securely, that, but for the extraordinary weight, it had passed unnoticed. Within the crust, the metal was not in granules, as in the first specimen, nor thin as a leaf, as in the second; but much more abundant,  
and

and in some places more than one inch thick; but unfortunately the person employed to discover the contents, injudiciously broke the outward crust all to pieces, by which means he got all the pure metal indeed, but prevented that absolute conviction, which this noble specimen would otherwise have conveyed to the doubtful. However, the like structure of the crust, and that of the before-mentioned specimens, and its connexion, to some fragments still in the keeping of Mr. Rosewarne, with the granulated surface, and shotten edge, of the metal, pronounce it, upon comparison, to be native tin.

The crust, inclosing this third specimen, was certain stone of the quartz kind, very hard to break, and exactly the same, to all appearance, with that of the first mass. I employed a tinner dextrous in vanning (a way of breaking and trying ores, by washing them on a shovel gently with water) to try it in his usual way; he bruised it in my sight, and observed to his surprise, that it suffered no diminution, or decrease, as all other ores do; that it was very rich in its kind; that he had never seen any such before; and that he could not say what metal it contained.

Thus far is the relation Mr. Borlase gives; but as the existence of native tin is so universally doubted, I thought it necessary, that other proofs than a meer historical account, and the exhibition of only two specimens, and both from the same hand, should be produced to prove it. Mineralists might then doubt whether what Mr. Borlase calls tin, was really that metal, or rather an arsenical marcasite, or other mi-

neral, which might appear like, or be mistaken for, tin. I thought it very necessary to remove all doubts, by making proper experiments to try if it was tin, before I presumed to communicate it to this learned body; it being so extraordinary a discovery. The experiments I made, and which, I hope, will prove satisfactory, to convince every one that it is really tin, are as follow,

1. It is perfectly ductile and malleable; and, bent between the teeth, gives the same crackling noise as tin always does.

2. In an open fire it melts easily, calcines on the surface, and smokes somewhat; forced in a stronger fire, with borax, it detonates with small phosphorescent sparks, which is a property of pure tin.

3. It is only corroded to a white calx in spirit of nitre, and oil of tartar *per deliquium* being added to the solution, not any thing was precipitated.

It is, therefore, pure Tin.

I am, with great respect and obedience,

Gentlemen,

Your ever devoted,

and most obliged humble servant,

Feb. 20,  
1766.

Emanuel Mendes da Costa.

Received January 3, 1766.

VIII. *A Letter from Edward Wortley Montagu, Esquire, F. R. S. to William Watfon, M. D. F. R. S. containing an Account of his Journey from Cairo, in Egypt, to the Written Mountains, in the Defart of Sinai.*

S I R,

Read March 13, 1766. **I**T is with a good deal of difficulty that I have prevailed upon myself to write to you, for, as coming now to Italy was quite unforeseen, and I am immediately going back to the East, I have not my journal with me, but luckily have the famous inscriptions. I am sensible every paper I send to the Royal Society exposes more and more my incapacity. However, as these inscriptions are much wanted, I cannot avoid sending them. I shall only speak to some of the points the bishop of Clogher mentions; but cannot avoid being now and then a little prolix.

I fet out from Cairo, by the road known by the name of Tauriche Beni Ifrael, Road of the Children of Ifrael. After twenty hours travelling, at about three miles an hour, we passed, by an opening in the mountains on our right hand, the mountains Maxattee. There are two more roads, one to the northward of this, which the Mecca pilgrims go, and one to the south,

south, between the mountains, but never travelled (as it does not lead to Suez, to which it is thirty hours march from Cairo). Through this breach the children of Israel are said to have entered the mountains, and not to have taken the most southern road, which I think most probable: for those valleys, to judge by what one now sees, could not be passable for Pharaoh's chariots. This breach, the inhabitants told me, leads directly to a plain called Badeah, which in Arabic signifies something *new* and *extraordinary*, and also *the beginning*, as the beginning of every thing is new, *i. e.* was not before known.

At Suez I found an opportunity of going to Tor by sea, which I gladly embraced, that, by going nearer the place, at which the Israelites are supposed to have entered the gulf, and having a view from the sea, as well of that as of the opposite shore, I might be a little better able to form a judgment about it. Besides, I was willing to have the views, bearings, and soundings, which I took, and they will appear some time or other; but this paper would scarce be their place, if I had them with me.

When we were opposite to Badeah, it seemed to me (for I was not on shore) a plain, capable of containing the Israelites, with a small elevation in the middle of it. I saw something too like ruins. The captain and pilots told me, that this was the place, where the Israelites entered the sea, and the ruins were those of a convent (I suppose built on the spot in commemoration of the fact); they added that there was good water there. There is here a strong current, which sets to the opposite shore, about south east; it forms

by its strength a whirlpool, where sailors said ships were lost, if forced into it, for want of wind, by the current. This pool is about six miles northward of Cape Karondel; and just below this pool there is a sand, a flat island at low water, which runs east and west about three miles. This sand, I suppose, is thrown up, by the force of the current; and the same current, by the resistance it meets with from this bank, being forced back into the cavity made by this excavation, forms the whirlpool. This pool is called Birque Pharaone, the well, or pool of Pharaoh; and here they affirm his host was destroyed. I shall say more of this as I travel back by land. We came to an anchor in fifteen fathom water, within a mile and a half of the shore, to the southward of this sand, and in the Birque Karondel, to the northward of the cape; here the eastern shore is already mountainous, which, near this place, was a sandy beach: the Egyptian shore, from Suez to Badeah, is likewise rocky and steep; so no entering upon the gulf from that shore, but at Badeah or Suez.

It is high water always, when the moon is at her meridian height, and it ebbs six hours. At Suez, it flows six foot; the spring tides are nine, and in the variable months, from the beginning of November, to the end of April, sometimes twelve. From the beginning of May to the beginning of October, a northerly wind generally rises, and goes down with the sun; it is often very strong. This wind never fails in these months, unless there be some violent storm; the rest of the year the winds are variable, and when they blow hard at S. and S. S. E. these winds set up the sea through the narrow streight of Babel Mandel, and up this gulf through its mouth,

between.

between Gebel El Zait, on the west side of this sea, and the southermost point of the bay of Tor, on the east side of this western branch of this sea, where it is not above twelve or fourteen miles over. I suppose such a wind, hindering the water from going out, causes this extraordinary encrease in the spring tides. We see the same thing happen with the same winds at Venice, both golfs running nearly in the same direction.

The Egyptian, western, or Thebaic shore, from Badaeh southward to opposite Tor, on the eastern shore, is all mountainous, and steep; and at Elim, the northermost point of the bay of Tor, ends the ridge of mountains, which begin on the eastern shore of this western branch at Karondel. I say nothing of Elim, or Tor, or the marine productions of this golf, as this paper is intended to give an account of Sharme, Meenah El Dzahab, Kadesh Barnea, the stone which Moses struck twice, and the inscriptions. I, however, must say, that, from this place, mount Sinai, properly called, cannot be seen; but only the ridge or groupe of mountains, in which it is, and which altogether form that part of this tongue of land called in general Mount Sinai. The garden of the Monks of Mount Sinai at Elim renders in dates, &c. 20.000 piaftres per ann. or £ 2,500.

We from thence crossed the plain, in about eight hours, and entered the mountains of Sinai. They are of granite of different colours. At the entrance of the narrow breach, through which we passed, I saw, on a large loose granite stone, an inscription in unknown characters, given, I think, by Dr. Pocock, bishop of Ossory; however, as the Israelites had no writing, that we know of, when they passed here, I did not think it of consequence enough to stop for;

the Arabs told me, it was relative to a battle fought here between Arabs, and indeed I do not see what point of history it can illustrate; besides, there are not above five or six words. We arrived at the convent of Mount Sinai, after the usual difficulties mentioned by other travellers, were received as usual, and saw the usual places, of which, however, I shall give the plans as well as elevations, which I took. I must say, that the Monks were far from owning to me, that they had ever meddled with the print of the foot of Mahomet's camel. I examined it narrowly, and no chissel has absolutely ever touched it, for the coat of the granite is entire and unbroke in every part; and every body knows, that if the coat of less hard stones than granite is once destroyed, it never returns. It is a most curious *Lusus Naturæ*, and the Mahometans turn it to their use.

Meribah is indeed surprizingly striking. I examined the lips of its mouths, and found that no chissel had ever worked there; the channel is plainly worn by only the course of water, and the bare inspection of it is sufficient to convince any one it is not the work of man. Amongst the innumerable cracks in rocks, which I have seen in this, as well as other parts of the world, I never met with any like this, except that at Jerusalem, and the two, which are in the rock Moses struck twice, of which hereafter.

I had enquired of the captain and the two pilots of our ship, about Sharne and Dzahab, on the western shore of the eastern branch of the Red-sea; they told me that they were often forced up the Elanitic gulf, the eastern branch of the Red-sea, and generally went to Sharne,



Sharme, and sometimes as high as Dzahab ; that they generally ran from Cape Mahomet, the southermost part of the peninsula between those two golfs, to Sharme, in six hours, because they always made as much more way, as they commonly do, they very seldom going there but in a storm : they generally run four knots, so this makes forty eight miles, which brings it to the northward of Tor. Tor is in lat. 27. 55. Cape Mahomet thirty miles southward, lat. 27. 25. Sharme forty eight miles nearly N. lat. 28. 13. consequently about E. N. E. of Sinai. The port is pretty large, surrounded with high mountains, the entrance very narrow, and the water deep quite to the rocks, which are so very steep, that a stone dropt from the summit falls into the basin. No wind can be felt here ; they don't cast anchor, but fasten their cables to the rocks. There is good water ; some habitations are found on the sides of the mountains, and a pretty large village at top : this seems to answer the idea of Nest-Ken. Dzahab lies as high again up the golf, so forty eight miles more, or in lat. 29. This port is considerably larger than the former, and very good, but not so closely surrounded with mountains ; it is however, very safe. There is a well of great antiquity with very good water ; very considerable ruins are found, and they say, there was a great city formerly ; but no habitations now, except an Arabian camp of 2000 men. There is a road from it to Jerusalem, formerly much frequented. Thus far the captain and pilots. I enquired from the Monks, as well as Arabs, about these places, as well as about the ruins, supposed by my learned friend, the bishop of Offory, to be Kadesh Barnea : the former could only tell me, they had not received any fish from thence.

thence in many years, that it was two easy days journey off, but the road was mountainous; so one may suppose the distance less than forty miles. The Arabs agreed as to the road; but they said, it was once a large place, where their prince lived, whose daughter Moses married, that Moses was afterwards their prince, and the greatest of all prophets. These Arabs place Moses the first, Salomon the second, Mahomet the third, Christ the fourth, and then the prophets of the Bible. As to Dzahab, the Monks only knew the distance to be four days journey, and that there was a road from it to Jerusalem: the Arabs told me the same, so the distance is about eighty miles. I enquired of them all about the ruins; they told me there were very considerable ones about half way to Dzahab, about forty miles from Sinai; but I should think Kadesh must have been much nearer to Jerusalem. I would willingly have gone to these places; but as the four clans of Arabs, which inhabit this promontory, were then at war one with the other, I could get no conductor. In another journey I hope to be more lucky, for this is all hearsay; however, combining the whole together, and comparing it with what we collect from Scripture, I think we may well conclude, Sharme to be Midian, and Meenah El Dzahab to be Eziongeber: what the interjacent ruins are I cannot conjecture; but I believe I have found Kadesh Barnea to be elsewhere. I think it cannot be here, for the Israelites were on the borders of the Holy Land, or Land of Promise, when they were ordered back; and when they were stopped by the Moabites, they are said to have been brought up from Kadesh Barnea; and I meet with no place in


sacred writing, or any antient geographer, neither Strabo nor any other, that draw the line of division between this promontory and the Land of Promise so low down; nor could they do it, as these ruins are within almost seventy miles of the extremity of it. There are two roads from Mount Sinai to Jerusalem, the one through Pharan, the other by the way of Dzahab: that through Pharan is eleven days journey; two to Pharan, three to a station of the Mecca Pilgrims called Scheich Ali, one and an half to some considerable ruins; all this to the northward: from thence four and something more to Jerusalem, by way of Hebron, leaving the Asphaltic Lake on the right hand to the southeastward. The other way is longer, on account of the road being more mountainous; that too passes the same ruins, and also Scheich Ali. I enquired about this, when I was at Jerusalem, and received the very same account, with this addition, that such Mahometans, as went from Jerusalem to Mecca, went that way, to join the Cairo caravan at Scheich Ali. This seems to be a situation opposite to Kadesh Barnea; at the line drawn by all the geographers; it is without Mount Sinai (taken for this whole tract); and just before the Moabites, as the children of Israel passed by Mount Hor, now Acaba, leaving the Asphaltic Lake on their left hand, to the northwest. The tradition too of the Arabs is, that they passed this way; therefore, I think, Kadesh Barnea must be near this spot. There are here considerable ruins; and I know of no city that ever was here, for Petra lay more to the east, between the Asphaltic lake and the Elanitic gulf. To leave no enquiry wanting, I asked the Rabbins of Jerusalem, where they placed Kadesh Barnea; and they said, these ruins.


We set out from Mount Sinai by the way of Scheich Salem; and, after we had passed Mahomet's stone, came to the beautiful valley, mentioned in the Journal. I lay there (and hope I have discovered the manna, but that will be the subject of another paper) and did not set out before day-light, that I might not pass the rock which Moses struck twice. I searched, and enquired of my Arabs, but could neither hear nor see any thing of it. I saw several short inscriptions stained on some parts of the mountains, the characters being the same with those on mount Sinai, Meribah, &c. given by the bishop of Offory. About four miles before we arrived at Pharan, we passed through a remarkable breach in a rock; each side of it is perpendicular as a wall, about eighty feet high, and the breach is about forty broad. It is at this breach, I imagine, the Horites were smote, four miles beyond the present ruins of Pharan; for having passed this breach they could make a stand, nor could they well be pursued. Here, on the tops of the mountains to our right hand, were ruins of buildings, and one seemed a castle. From Meribah to near this place, we had always rather descended; in most places there is the bed of a stream and after rain the water runs; but a little before we came to this breach, it wended off towards the west, for the waters fall into that part of the desert we crossed from Tor. Between this breach and Pharan, there are several springs, and one at Pharan where we encamped; there is the bed of the river mentioned by the Journal, the traditional account of which agrees with what is said by St. Paul. Waters seem to have run from Meribah to within about six miles of this place; the bed of a stream is here again very plain and a spring at the  
upper

upper end of it, which does not yield water enough to make a stream, the bed then is dry; four valleys terminate here, and form a large area. I enquired about the road to Jerufalem; the people agreed in the distance and ruins. We travelled in the bed of the river through the valley to the north; and in about half an hour, the sight and appearance of a large stone, not unlike Meribah, which lay at some distance from the mountain on our right hand, struck me; and I also observed, it had many small stones upon it. The Arabs, when they have any stone or spot in veneration, as Mahomet's stone, and the like, after their devotion, lay some smooth stone upon it. I asked what it was, they told me Hagar Moufa, the stone of Moses. I told them that could not be, for that lay in Rephidim; they said that was true, but this was Hagar il Chotatain, the stone of the two strokes; that he struck it twice, and more water came from it than from Meribah; witness the river. The bed of the river winds to the eastward, about E. S. E. I asked how far it went; they said this bed ran by Sheich Ali to those ruins, and quite away to the sea; so the river must have begun here, and not at Pharan, and the bed from Pharan here is only formed (I suppose) by winter torrents. If this is the bed of the river mentioned by St. Paul, as I dare say it is, we have the second rock: if it runs to the ruins, as is said, and there is no reason to doubt it, they will be pretty plainly those of Kadesh Barnea; and if this bed continues in the same course to the sea, as it probably does, this probably is the river at Rinocolura, supposed, by Eratosthenes, to be formed by the Arabian lakes; because he did not know its miraculous

head. This river is doubted of by Strabo, because dried up to the source, from the time the Israelites entered the Land of Promise, and the tradition was then lost. You may see Strabo's *Affyria*, edit. Cafaubon, p. 5. 10. towards the bottom. Pardon this bold conjecture; but it coincides and conciliates sacred history with antient geography. This too seems a proof, that this is really the second struck rock. As to the springs between the breach and Pharan, they certainly did not exist in the time of Moses; or, if they did, they would have been as nothing to so many people.

We went down a large valley to the west, towards the sea, and passed the head of a valley, a part of the desert of Sin, which separates the mountains of Pharan from those which run along the coast, and the same plain, which we had passed from Tor. We had scarce entered these mountains, and travelled an hour, when after passing a mountain, where there were visible marks of an extinguished subterraneous fire, we saw, on our left hand, a small rock, with some unknown characters cut on it, not stained upon it, as those hitherto met with; and, in ten minutes, we entered a valley six miles broad, running nearly North and South, with all the rocks, which enclose it on the West side, covered with characters. These are what are called *Gebel El Macaatab*, the written mountains. On examining these characters, I was greatly disappointed, in finding them every where interspersed with figures of men and beasts, which convinced me they were not written by the Israelites; for if they had been after the publication of the law, Moses would not have permitted them to engrave images, so immediately  
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after.

after he had received the second commandment: if they went this way, and not along the coast, they had then no characters, that we know of, unless some of them were skilled in hieroglyphicks, and these have no connection with them. It will be difficult to guess what these inscriptions are; and, I fear, if ever it is discovered, they will be found scarce worth the pains. If conjecture be permitted, I will give my very weak thoughts. They cannot have been written by Israelites, or Mahometans, for the above reason; and if by Mahometans, they would have some resemblance to some sorts of Cuphic characters, which were the characters used in the Arabic language, before the introduction of the present Arabic letters. The first MSS. of the Alcoran were in Cuphic: there is a very fine one at Cairo, which I could not purchase, for it is in the principal Mosque; and the Iman would not steal it for me, under four hundred sequins, £200. These have not the least resemblance to them: Saracenic characters are very unlike; besides, I should place them higher than the Hegira. I think it then not unprobable, that they were written in the first ages of Christianity, and perhaps the very first; when, I suppose, pilgrimages from Jerusalem to Mount Sinai were fashionable, consequently frequent and numerous, by the new Christian Jews, who believed in Christ; therefore, I should believe them Hebrew characters, used vulgarly by the Jews about the time of Christ. I shewed them when at Jerusalem to the Rabins; they were of the same opinion, and thought , which is frequent, was שלם; and to that

 which is just before with a

small cross שלב שך ישוע, by changing the *skin* into *sin*, and adding *je*, it would be an Arabic word سليم a cross, and might be explained, the cross borne or carried by Jesus. The Hebrew would be Jesus brought safety, or salvation. But, Sir, more able than me will judge better. These are all conjectures; and it seems much easier to say what these inscriptions are not, than what they are. They can scarce be of St. Helen's time; for they would have some analogy with Greek characters, and they have none. Perhaps some gentlemen will think them *antient Egyptian*, written, by the colony, which they suppose went to inhabit China. That is a matter I won't meddle with; but, amongst many others, it will be liable to one great objection, which is, that such colony, if there ever was one, probably went the *straight road*, from the head of one gulf to the head of the other, from Hierapolis to Eloth, the way the Mecca pilgrims now go. This place would have been far out of their way, being at least sixty miles to the southward of the pilgrims road, unless they were supposed to have had transports at Dzahab, or Sharme. I, for the first reason given, did not think them written by the Israelites, and could not conceive that they were of any great consequence. I then took these few as a specimen. Here are on other parts of this rock, some Greek, and Arabic, as well as some Saracen inscriptions, and an Hebrew one, which is הנה ישוע. The Saracens and Arabic only say, "such an one was here at such a time"; the same say the Greek ones, except one, which says, as I remember for I have it not with me, "The evil genius of the army wrote this," which can only prove, that some body of Greeks was worsted here, after the characters were



were written, and that they attributed their defeat to some magick power in these characters: as we are now fruitful in conjecture, perhaps some gentlemen will bring Xenophon here. The characters seem to be of the very same kind with those stained on different parts of Mount Sinai, Meribah, &c. which my learned and accurate friend the Bishop of Ofsory has given.

The third day from this place, travelling westward, we encamped at Sarondou, as the Journal calls it; but it is Korondel, where are the bitter waters, Marah. I tried if the branches of any of the trees had any effect on the waters; but found none: so the effect mentioned in Scripture must have been miraculous. These waters at the spring are somewhat bitter and brackish, but as every foot they run over the sand is covered with bituminous salts, grown up by the excessive heat of the sun, they acquire much saltiness, and bitterness, and very soon become not potable. This place, off which the ships cast anchor, is below the sand, which I mentioned before, near the Birque Korondel. After nine hours and a half march we arrived, and encamped at the Desert of Shur, or Sour. The constant tradition is, that the Israelites ascended from the sea here; this is opposite to the plain Badeah, to which the above-mentioned pass in the mountains lead. From this place the openings in the mountains appear a great crack, and may be called a Mouth, taking Hiroth for an appellation. However, I should rather adopt the signification of Liberty. It would hardly have been necessary for the Israelites to pass the sea, if they were within two or three miles of the northern extremity of the gulf; the space of at most two miles, the breadth

of

of the golf at Suez, and at most three foot deep at low water, for it is then constantly waded over, could not have contained so many people, or drowned Pharaoh's army. There would have been little necessity for his cavalry and chariots to precipitate themselves after a number of people on foot, incumbered with their wives, children, and baggage; when they could soon have overtaken them with going so little about. These reasons, added to the significant names of the places, Tauriche Beni Israel, road of the children of Israel; Attacah, Deliverance, Pihahiroth, whether an appellative or significative; Badeah, new thing, or miracle; Bachorel. Polsum, sea of destruction; convince me, that the Israelites entered the sea at Badeah, and no where else. Besides, all the rest of the coast from Suez, and below Badeah, is steep rocks, so there must have been another miracle for them to descend: the current too sets from this place where we encamped, toward the opposite shore, into the pool Birque Pharaone, Pool of Pharaoh, where, the tradition is, his host was drowned: a current, formed, I suppose, by the falling and rushing of one watery wall on the other, and driving it down: a current, perhaps, by God permitted to remain ever since, *in memoriam rei*; the distance to the bitter waters is about thirty miles. I omitted to mention in its place, that, between this and Korondel, we were not so lucky as the author of the Journal, who met with a charming rivulet of sweet water; we met with none, good or bad. The Ain Moufa, which the Israelites would have met with, if they had passed at Suez, and the coast from hence southward, about a mile to Tor, being all rock and steep too, induce me to believe, that they entered

ed the sea at Badaah, and ascended from it here, and not at any other place. But I am too sensible of my own inability to decide, and leave that to better judges than I am. I only throw out what occurs to me, from the inspection of the country, an inspection as accurate as I am capable of. If any thing I have said can in the least support that revelation, to which I dare declare myself a friend, even in this enlightened age, I shall be very happy; or if this trip of mine can be of any use whatever, as I had great pleasure in it, I may truly say with Horace—*Omne tulit punctum, &c.*

The denomination of אֵי הַיָּם הַקָּדוֹן, I believe, only regards the Hierapolitic branch, as the marine productions, Madreporas, &c. which form admirable forests in the bottom of it, are not in the Elanitic branch, or the gulf; I mean the broad part below Cape Mahomet. No more than that western branch was known to the Israelites at the time of their passage, if it was to the Egyptians: but the name descended to the whole, as their knowledge of it. The Red Sea seems to regard the broad part alone; for tho' there are not the above-mentioned sea productions, yet there is so great a quantity of tube coral (not found in the western branch of the Hierapolitic gulf) and such rocks, as one may say of them, that the Gedda ships fasten themselves to them instead of casting anchor. It is of a deep red, so that possibly, the first navigators entering at the streight of Babel Mandel, from the red they saw, called it the Red Sea, and that name descended to the whole with their navigation. This sea is tempestuous and full of shoals; there is no harbour on the Arabian coast after Tor, except one, I mean between Suez and Gidda or Mecca, which is a day and a half from Gidda:

Gidda. Gidda is its port; and there is only one on the other coast, Cossire; but it is a very bad one; however, ships sometimes go thither, and caravans cross the country to Morshout. The ships are, as the bishop of Offory has described them; the helm is on the outside, as I suppose, with his lordship, that of St. Paul was. They make use of but four sails, and no compass, nor do they ever cast the lead. They sail only by day light, from anchoring place to anchoring place and are not, above two days out of sight of land, from Cape Mahomet to the Arabian main: if a gale happen, they are often lost; about one in ten every year. I shall be glad to be honoured with the Society's command, and in communicating this, you will oblige,

Sir,

Your most humble servant,

Pisa, Dec.  
2, 1765.

Ed. Wortley Mountagu.

*P. S.* I am a very bad draughtsman; but I assure you, the sketches contained in Plate III. are rather better than the originals. They are about six inches long, the marble is whitish, in some places reddish, of a flesh color; they are engraved with a pointed instrument, for one sees, in the bottom of them, round marks of the point of the instrument. I have met with much basalto, but not one piece of that soft stone of which is the bust at Turin, nor any of the characters upon it, except some are found amongst these, I have neither seen any head, bust, or statue, in the character of that.

The







The second rock struck by Moses is, I think, 43 feet long, 16 broad, 13 high; it has two cracks, oblique ones; in them are some mouths, like those of Meribah: it is of a hard stone, not granite or marble.

I have the exact dimensions and elevation of the second stone, as well as of Meribah.

*IX. A Discovery, with Observations, of two new Comets in the Marine Obvatory at Paris; by M. Messier, F. R. S. and Member of the Academy of Sciences at Paris; translated from the French, by M. Maty, M. D. Sec. R. S.*

Read March 20, April 24,  
and May 15, 1766.

**O**N the 8th of March 1766, the sky having been clear the whole day, I had a mind to make use of this fine weather to look for the fatellite of Venus, which for some years has been talked of. I employed, for this purpose, an excellent Gregorian telescope, of 30 inches focus; the great speculum of which, being six inches diameter, magnified objects about 109 times. I could discover nothing with this instrument, the planet appearing only surrounded with small telescopic stars. I likewise made use of a very good achromatic telescope of 5 feet, constructed at Paris, and belonging to the Pr. of S. with which I discovered at about 7 o'clock, and at some distance of Venus, a nebulosity of a small extent,



with a luminous center. The time did not permit me that day to assure myself, whether it was a comet or a nebulous star. I was only able, before its setting, to take its position, by comparing it with a star of the 4th magnitude, and to defer to the next day the completing of this discovery. This very night, after the observation, I looked over my copy of the celestial maps of Mr. Flamsteed, upon which I have delineated all the nebulous stars, which I have discovered for some years, and found one in that part of the heaven, which I began to see August 25, 1764. My description is thus entered in my journal: "I applied myself to the discovery of nebulous stars, the night between the 25th and 26th of August 1764, and I discovered one near the star  $\alpha$  of the great triangle, which I compared with that star, in order to obtain its position. This nebulous star is a whitish spot 15 minutes in diameter; the light nearly uniform, tho' somewhat brighter on the right side; it is seen with difficulty by a common refracting telescope of one foot." Imagining, that what I had then been looking at was this nebulous star, I lost all hopes for the next day. Being, however, impatient to observe the sky, I found in the evening, that this nebulous star had altered its position, being got nearer to the star, with which I had compared it the day before, and which I then found to be the star  $\eta$  of the knot of Pisces. of the 4th magnitude. I determined the position of the comet with respect to this star, with all the care imaginable, by means of a Newtonian telescope of  $4\frac{1}{2}$  feet in length, furnished with a filken-threaded micrometer. This is the table of my observations.

A TABLE of the Places of the first Comet of 1766, observed during Part of the Month of *March*, deduced from the Determination of its Position, with respect to certain Stars.

1766.	tr. Tim.			Rig. Asc.			Dec. Nor.			Stars with which the Comet was compared.
	h	'	"	°	'	"	°	'	"	
Mar. 8	7	34	22	17	39	46	14	58	11	Com. comp. with $\eta$ Pisc.
9	6	55	4	18	42	46	14	34	40	with the same
	7	35	28	18	44	46	14	35	3	with the same
	7	47	8	18	45	37	14	31	4	with the same
10	6	49	52	19	45	1	14	10	38	with the same
	7	0	23	19	45	31	14	10	37	with the same
	7	9	30	19	45	46	14	10	2	with the same
	7	40	58	19	47	16	14	9	33	with the same
11	6	58	47	20	44	46	13	46	35	with the same
	7	21	37	20	45	31	13	46	10	with the same
	7	27	0	20	45	36	13	46	24	Com. with Flam. 31 of Pisc.
	7	35	21	20	46	6	13	46	13	with the same
12	6	56	28	21	39	43	13	23	45	with the same
	7	5	47	21	40	13	13	23	48	with the same
	7	20	38	21	40	43	13	23	52	with the same
	7	41	48	21	42	16	13	22	51	with $\eta$ Pisc.
	7	51	26	21	44	51	13	23	58	with 101 Pisc.
13	6	49	46	22	31	21	13	1	46	with the same
	7	34	17	22	32	1	13	0	19	with $\eta$ Pisc.
	7	54	46	22	36	1	13	0	18	with the same
14	6	53	0	23	22	16	12	39	40	with Flam. 104 Pisc.
	7	25	16	23	22	46	12	38	46	with $\eta$ Pisc.
	7	47	44	23	25	1	12	38	33	with the same
15	7	6	3	24	5	51	12	18	2	with $\times$ above 101 Pisc.
	7	34	16	24	8	51	12	17	27	with the same
	7	58	57	24	0	21	12	17	7	with the same

**TABLE** of the Positions of the Stars, with which the Comet was compared, reduced to the time of the Observations.

Rig. Asc. | Dec. Nor. |

19	45	1	14	8	○ n of the knot Pisc. Comet comp. 8, 9, 10, 11, 12, 13, & 14.
20	49	6	13	28	30 101 Pisc. of Flamsteed Com. comp. the 11, 12, 13, & 15.
21	41	24	13	6	30   104 of Pisc. Comet compared the 14.

From these Observations, Mr. Pingré has computed the Elements of the Orbit of this Comet, as follows.

Place of the ascending node $\varnothing$	—	8 <sup>s</sup>	4	10	50
Inclination of the orbit	—		40	50	20
Place of the perihelium	—	4	23	15	25
Logarithm of the perihelion dist.				9.70	3570

The comet pass'd its perihelion, the 17th of February, at 8<sup>h</sup> 50', mean time at the meridian of Paris.

The motion of the comet retrograde.

*Observations of the second Comet of 1766, discovered at the Marine Observatory at Paris, one month after the former, viz. the 8th of April. By Mr. Messier, &c.*

The 8th of April, the sky having cleared up after many days of cloudy weather; on the evening at 8 o'clock, being gone to the Marine Observatory, to observe some transits of stars on the meridian, and looking on the heavens towards the west, I discovered, by my naked eye, near the horizon, and at a little distance from the Pleiades, a comet, which already appeared considerable; the tail was about 4 degrees in length, the light lively, the nucleus very bright and equally luminous with stars of the 3d magnitude. The comet was at a small distance from the brightest star of the constellation of musca, which Flamsteed, in the second

second edition of his catalogue, makes of the 3d magnitude. I several times compared the nucleus of this comet with this star, to deduce its position. The next day, being the 9th, the sky appearing perfectly serene without a moon, I began to see the comet a few minutes before 8 o'clock. During one hour, it was distinctly seen with the naked eye, the tail was  $6^{\circ}$  or  $7^{\circ}$  in length, the nucleus very bright. I measured its diameter, by comparing it with the thickness of one of the threads of the micrometer of my instrument, and found it about  $36''$  of a degree. On the 10th, the sky being equally bright, I saw the comet some minutes before 8 o'clock, and between 8 and 9 it was seen with the naked eye, with a longer tail than yesterday, but not so distinct; the nucleus had also lost part of its brightness. On the 11th, the sky being clear as the preceding days, the comet could only with difficulty be seen with the naked eye; its appearances were much less distinct than the day before, both on account of the vapors of the horizon where it was, and of the twilight which was considerable, and still more so by the light of the moon, which, as well as the two preceding days, was in the part of the heavens, where the comet appeared. On the 12th, the comet could no longer be seen with the naked eye. Through the telescope it appeared very faint, the tail not exceeding now one degree and a half. It certainly became thus invisible, as well as the nucleus, through the too great power of the twilight. The 13th, the sky was serene, with some clouds towards the horizon; I looked for the comet, but could not find it, so that the evening of the 12th was the period of its visibility.

TABLE

TABLE of the Observations of the Comet.

1766.	tr.	Tim.	Right Asc	Dec.	Nor.	Stars with which the Comet was compared.	
			observ.	observ.			
		h	°	'	"		
Apr. 8	8	33	54	39	29	56 25 12 10	Com. com. with the Star 41 of Aries, according to Flamsteed.
	8	57	40	39	25	41 25 11 23	Com. com. with the same
	9	6	43	39	24	41 25 10 22	— with the same
	8	7	7	37	59	26 24 26 5	— with 33 of Aries
	8	7	7	37	59	41	— with new Star of 7 Mag.
	8	21	40	37	59	18 24 25 5	— with 33 of Aries
	8	21	40	37	59	41	— with new Star of 7 Mag.
	8	33	4	37	58	41 24 24 20	— with 33 of Aries
	8	33	4	37	58	41	— with new Star of 7 Mag.
	8	44	46	37	58	56 24 25 3	— with the same
	8	44	46	37	58	41 24 24 32	— with new Star of 6 Mag.
	8	4	15	36	31	15 23 36 46	— with 4 of Aries
	8	16	35	36	30	34 23 36 2	— with 30 of Aries
	8	16	35	36	30	41 23 36 45	— with new Star of 7 Mag.
	8	16	3	36	30	56 23 36 49	— with the Star of 6 Mag.
	8	36		36	29	11 23 36 1	— with new Star of 7 Mag.
	8	36	26	36	28	56 23 35 31	— with the Star of 6 Mag.
	11	8	6	57	34	57 53 22 33 38	— with α of Aries
	12	7	39	5	33	29 23 21 39 16	— with the same
	8		2	11	33	28 46 21 39	— with the same

TABLE of the Positions of the Stars, with which the Comet was compared, reduced to the Time of the Observations.

Right Asc. Dec. Nor. Mag

	h	°	'	"	
39	3	41	26	17	41 of ♀ accor. to Flam. ded. fr. A. de la Caille's Cat. It is only men. in Flam. Select M. I deter. its Posit. New. I determined its Position
38	48	56	24	16	25
37	35	11	24	40	51
36	46	26	26	2	0
35	51	34	23	46	6
28	30	38	22	20	55

From my observations, M. Pingré has calculated the elements of this Comet as follows;

	"
Place of the ascending node	1 17 22 19
Inclination of the orbit	8 18 45
Place of the perihelion	6 26 5 13
Perihelion distance	0,636825
Logarithm of the perihelion distance	9,804020

It passed the perihelion April 17, 0<sup>h</sup> 26' 13"  
mean time at the meridian of Paris.

The motion of the Comet direct.

By these elements M. Pingré judged that this Comet might be seen again in the morning, after getting clear of the sun's rays; and the following is an Ephemeris he computed for finding it in the months of May and June.

An Ephemeris of the second Comet of 1766, for the months of May and June, at 16<sup>h</sup> mean time at the meridian of Greenwich.

May		Com. rifes.	Comet's Long.	Com. Lat. S.	Com. R. Af.	Com. Dec. S.	Comet's Diff. fr. Earth.	Comet's Diff. fr. Sun.
d	h	h /	o /	o /	o /	o /		
1	10	15 52	♄ 5 2	3 43	6 3	1 23	0,437	0,709
3	16	15 45	3 0	4 37	4 37	3 3	0,455	0,729
5	16	15 39	1 19	5 26	3 24	4 28	0,473	0,760
7	16	15 32	♄ 29 54	6 10	2 23	5 42	0,492	0,773
9	16	15 26	28 41	6 51	1 32	6 48	0,510	0,797
11	16	15 20	27 39	7 28	0 50	7 46	0,528	0,821
13	16	15 14	26 47	8 1	0 16	8 38	0,546	0,847
15	16	15 8	26 3	8 32	359 47	9 24	0,564	0,874
17	16	15 2	25 24	9 1	359 23	10 6	0,581	0,901
19	16	14 55	24 49	9 28	359 3	10 45	0,597	0,929
21	16	14 49	24 18	9 54	358 45	11 29	0,613	0,957
23	16	14 42	23 49	10 18	358 29	11 53	0,628	0,986
25	16	14 35	23 22	10 40	358 13	12 25	0,642	1,016
27	16	14 29	22 56	11 2	357 58	12 56	0,656	1,046
29	16	14 22	22 31	11 23	357 44	13 26	0,669	1,076
31	16	14 15	22 7	11 42	357 30	13 56	0,682	1,105
				June	5 356 48	15 3		
					10 355 58	16 13		
					15 354 57	17 24		
					20 353 41	18 37		
					25 352 10	19 53		
					30 350 22	21 10		

In the months of May and June, I sought for the comet in the morning, when the sky was serene, in the places indicated in this ephemeris, without being able

able to discover it: the twilight, which was considerable at the time of the comet's rising, might hinder my seeing it, and I have not yet learned that it was seen any where else.

*Mr. Pingré's remarks on the two comets of this year.*

The elements of the first comet I give as absolutely certain, those of the second I cannot be so sure of. The interval was only four days between the first and the last observation. The two last days, and especially the last; the twilight and the moon light must have produced some uncertainty in the observation. The ephemeris is founded upon the certainty of the elements. Supposing there were no more than three or four minutes error in the two last observations, this would not much alter the theory from what I have given; but an alteration in the elements would produce two or three degrees difference in the place of the perihelion, which might be sufficient to render the reappearance of the comet uncertain in these high latitudes. Some German observations sent to Mr. Messier, made in the beginning of April, induce me to conclude, that the place of the perihelion ought really to be placed a little more eastward; but these observations were sent in so confused a manner, that it seems impossible to obtain the least light from them. Perhaps we may hereafter receive from some southern parts, observations sufficient to make out the true orbit of this comet.



X. *A Letter from Mr. Alexander Brice, to the Earl of Morton, President of the R. S. giving an Account of a Comet seen by him.*

Kirknewton, April 11, 1766.

My Lord;

Read April 17, 1766. **I** Beg leave by this, to acquaint your lordship, that, last night and the night before, I observed a comet in the northwest, and very near the horizon. It began to appear at half an hour after 8 o'clock, and set 25 minutes after 9. The tail was very visible to the naked eye; but the nucleus could not be seen by any, that were present with me, without a telescope, thro' which it appeared very distinctly, like a star of the 4th or 5th magnitude. It was surrounded with a gleam of light, like what is seen round the stars in Orion's sword, commonly called Janua Cœli. The tail stretched upwards, and inclined to the west, and was about 4 degrees long: the body of the comet was also 4 degrees distant from the new moon (then 34 hours after the change) and almost perpendicular above it; and it appeared to more advantage after the moon was set. The comet, when setting, was 37 degrees to the north of due west, and 13 degrees more northerly than the Pleiades, below them, but in the same tract: it is descending towards the sun, at the rate of 6 degrees as near as I could guess, in the space of twenty four hours; and unless the light of the moon, or the twilight prevents, it will



Observed April 10<sup>th</sup>, 1793

From VIII. 30 to IX. 25

At Night

Pleiades



15 deg.

The Comet

☾ New Moon

West

37 deg.

N. West

Horizon

*Pole Star*



*North*



will make a more remarkable appearance after it has passed the sun, than it does at present. I know not whether this comet has been observed by any person about London; if I thought it had, I would not have given your lordship the trouble of this; but I wished to put other people upon the look-out, who will probably make more of it than I can. [For the appearance of this comet, see Plate IV.] I have the honour to be,

My Lord,

Your Lordship's

most obedient and most humble servant,

Alex. Brice.

XI. *A Report concerning the Microscope-Glasses, sent as a Present to the Royal Society\*, by Father di Torre of Naples, and referred to the Examination of Mr. Baker, F. R. S.*

Read April 17, 1766. **M**R. Baker now returns the microscope glasses, which the Royal Society did him the honour to refer to his examination: and he would have returned them much sooner, had he not waited till he could examine them by bright day-light, being desirous to do them all the justice in his power.

\* Vid. Phil. Transf. Vol. LV. p. 258.

They are globules of glass, formed over a lamp, by Father di Torre, and ingeniously placed in cells of brass, adapted to Wilson's microscope. Four of these cells, thus furnished, were sent as a present from the Father to this Society, under the care of our late worthy member Sir Francis Eyles Stiles: but when they came into Mr. Baker's hands, one of these minute glasses was wanting, having probably been shaken out of its cell in carriage: the loss, however, signifies little, as there remains another of the same magnifying power.

These globules are wonderfully small: the largest being in diameter only two Paris points, and said to magnify the diameter of an object 640 times: the second is the size of one Paris point, magnifying the diameter of an object 1280 times; and the third is so extremely minute, as to be no more than one half of a Paris point, or the 144th part of an inch in diameter, and is said to magnify the diameter of an object 2560 times, and consequently it must magnify the square of such diameter 6,553,600 times.

Now as the focus of a glass globule is at the distance of  $\frac{1}{4}$ th of its diameter, it is with the utmost difficulty that globules so minute as these can be employed to any purpose. For instance, the focus of that globule, whose diameter is but one half of a Paris point (or the 144th part of an inch) is no farther from the object to be examined, than the 576th part of an inch. In attempting to find this focus, it is scarce possible to avoid touching the object with the glass, if it be not placed between laminae of talc or isinglass; and if it be so placed, even the thinnest talc bears a considerable proportion to this 576th part of

an inch, and will prove an unfurmountable obstacle to the seeing any object, unless by some very happy accident.

The other globules, whose focus is not quite so near, are liable proportionally to the same inconvenience.

The very great magnifying power of glass globules is sufficiently well known: many years ago they were much used, and highly boasted of on that account. But they now, long since, have been laid aside, and convex lenses substituted in their room; and that with very good reason, from the difficulty in the application of such globules, from the deficiency of light, from the distortion of the image seen, from the painful straining of the eyes, and from the boundless latitude given to imagination and conjecture, for want of sufficient distinctness and precision.

Nothing can be more injudicious than the desire of such excessive magnifying power: whenever we can see an object clearly and well defined, we ought to be contented; all beyond this there is no dependence on.

In some letters, sent with these glasses, the Society has been favoured with uncommon observations on the globules of the blood, described as having been viewed (it is not said by these glasses) floating in the serum, and sometimes changing their figure therein: and also with a long account of the impregnation of vegetables; wherein we are told, that the exquisitely minute corpuscles or seminal particles, emitted by the grains of the Farina sæcundans, have been seen to enter into, and be conveyed along tubes exceeding small, which at the time dilated and contracted.



ed occasionally to convey them to the ovarium\*. Mr. Baker was extremely desirous to repeat these experiments: but as it was absolutely necessary to spread the blood as thin as possible, to render it very transparent, without which nothing can be seen by such small glasses, he could not possibly prevent its becoming quite dry, before he could apply it to the eye, and consequently was unable to perceive any floating globules: and though he has been many years conversant with microscopes, he has not been able to contrive any method of applying the parts of generation of plants in such manner, to these glasses, as to view this wonderful impregnation.

\* The curious will find the whole account, with copper-plates relating thereto, Phil. Transf. Vol. LV. p. 258—270; from whence one single passage shall be here quoted, viz. p. 262. “ The grains being arrived at a state of maturity before they  
 “ issued from the antheræ, are prepared to burst and discharge  
 “ their contents when they fall on the hairs: and the female  
 “ organ assists likewise in producing this effect; for soon after a  
 “ grain has lodged itself, the point of the hair begins to open,  
 “ and the mouth extends itself by degrees over the surface of the  
 “ grain, till almost the whole body of the grain is drawn within  
 “ the tube; in this situation the grain soon yields to the com-  
 “ pression of the tube, and discharges its corpuscles, which, with  
 “ the assistance of the fluid parts of the pulp that enter with them,  
 “ or of the juices with which the tube itself is furnished, float on  
 “ till they enter the longitudinal ducts, which convey them to  
 “ the germen.”—It must be observed here, in justice to Mr. Turberville Needham, F. R. S. that he was the person who first discovered, that, on applying water to the Farina fœcundans, many of its grains emitted streams of exquisitely minute globules, as if through a small aperture: this he published in the year 1745 †, and from thence imagined the impregnation of plants to be carried on in a manner somewhat similar to that in the account referred to; but the same justice must allow, that, before Father di Torre, nobody is supposed to have seen these several progressions towards impregnation.

† Vid. Microscopical Discoveries by Mr. Needham, p. 73, &c.

It is, however, proper to take notice, that in these letters an apparatus is described, to be added to Wilson's microscope, when these glasses are made use of; which apparatus Mr. Baker was not at the expence of procuring, as it would answer no other purpose: but the method he contrived, instead thereof, he imagines to be equally effectual.

In truth, Mr. Baker has employed much time and his best endeavours in the examination of these glasses, as they were supposed capable of such wonderful discoveries: and that as well by candle-light, as (by what is recommended) the strongest day-light: and yet he must declare, with some concern, that through the smallest globule, viz. of one half of a Paris point in diameter, he has not been able to distinguish any thing; and even through that which magnifies the least, he could never view any object with satisfaction; though he applied the most minute, and consequently the properest objects for these glasses, viz. the globules of the blood, the farina of vegetables, the seeds of mushrooms, the feathers of butterflies, pepper-water, &c. He hopes his eyes are not injured by these examinations, as they have been much used to microscopes; but he believes there are very few, who would not have been nearly blinded thereby.

Upon the whole,—Mr. Baker thinks the Royal Society much obliged to the Father di Torre for these specimens of his great dexterity, ingenuity, and patience, in forming and setting glass spheres thus extremely minute; but he considers them as matters of curiosity rather than of real use.

Strand, April 17,  
1766.

Henry Baker.

Received

Received December 1, 1765.

XII. *De Veneris Transitu, per discum Solis,*  
*A. 1761, d. 6 Junii, Auctore F. Mallet,*  
*Astronomo Regis Upsal.*

Read April 17,  
 1766.

**Q**UUM Philosophicas Transactiones, A. 1763, nuper vidi, subtituli imprimis circa duas ad parallaxin Solis pertinentes, quæ illam ex observato transitu Veneris per discum Solis determinandam docent; in iisdem vero, observationum Upsalienfium usum inveni minus accuratum, forte ex non sufficienti apud Anglorum astronomos cognitione circumstantiarum, in quibus factas easdem descripsi in Actis Stockholm, A. 1761. Quam ob causam, illas denuo explicare & Societati Regiæ communicare decrevi, additis nonnullis meditationibus, quæ eandem materiam illustrare poterunt.

Anno 1761, 6 Junii, hora 2 ante meridiem, frequentes adsumimus in observatorio astronomico Upsalienfi, ortum Solis jam flagitantes, & singulare illud phænomenon transitus Veneris, per discum Solis, strenue expectantes. Nostrum vero ardorem restringebat subnubila cæli facies versus eandem plagam horizontis, quæ Solis apparitionem pollicebatur; nec minor fuit nostra anxietas, cum Solem, inter nubes, fluctuante, per intervalla deteximus. Spes tamen supererat, aut descensuram esse nebulam, aut Solem e vaporibus

e vaporibus cursum directurum, ut copulæ gloriam cum rariore planeta mathematicorum adstanti coronæ manifestaret. Augebat fiduciam Sol mox distinctius conspectus in margine superiori, & lumen ejus versus inferiorem paullatim crescens. Dubitavimus vero, adessetne Venus, suam præsentiam, plus quam seculo exoptatam, velociore ingressu acceleratura, & si globosæ quædam vaporum condensationes in parte imaginis Solaris confuse apparerent, quique a fallaci phænomeno, quasi globulo viso perculsus, dolebat, & cæli obscuritatem querebatur, quæ raram Veneris in splendidiore Solis disco immersionem nobis obvelaret. Tandem inclaruit Sol paullo altior, ut eundem per integram faciem scrutari possemus, et Veneris absentiam oculis armatis perspicue ediscere: hanc vero ut statim haud injucunde tulimus, ita planetæ desideratissimi accessum retardatum optavimus, quia Soli jam intenti hunc undique exæstuantem vidimus per motum vaporum, quibus graviter involutus conspiciebatur; erat videlicet limbus Solis vehementer quasi ebulliens, ita ut sollicitudinem haud levem injiceret marginum tremulatio de contactibus immersionis rite non observandis. Solis præterea lumen admodum debile, quia horizonti vicinior, erat, adeo ut eundem absque vitris coloratis per integra minuta intueri liceret.

Sub hæc, appropinquabat gratissima Venus; nobis autem de ephemeridum dissensu, in determinando primo ingressus momento, differentibus, accidit, ut inopinato adesset hora 3, 20', 45'', quod ego quidem primus observavi, postquam hora 3, 19' circiter, margines Solis, examine instituto, intactas invenissem, & telescopium meum commodius sub intervallo

transposuiffem. Apparuit primo Venus hirsuta facie in meo telescopio, quod est 18 poll. Angl. a D. Short fabricatum, & micrometro objectivo Dollondiano 30 ped. munitum, quodque ita adornatum objecta 55 vicibus magnificat. Hujus vero instrumenti eam indolem deprehendi, quod objecta horizonti vicina haud probe terminata repræsentet, & in eodem casu focum genuinum difficulter admodum assequi liceat; quapropter pluribus antediebus, instrumentum hocce pleno suo apparatu instruxeram, & diversa ratione focum exquisiveram atque correxeram, notato indicis loco in scala, quæ speculo minori adjuncta est, ne sub tempore observationum de hoc dubium restaret. Eandem ob causam, statim post Solis ortum, indicem repetitis vicibus scrutatus sum, & rite collocatum inveni; qua licet adhibita cautione, nescio quomodo factum sit, ut sub contactu interiore immerfionis confusio, licet maxime exigua, oculos molestaret, & paullo post focum alteratum deprehenderim: erat vero mutatio foci vix quingentesima parte pollicis Angl. & obscuritas marginum tantilla, ut aëris constitutioni, vel levissimæ oculorum sub horis matutinis diversitati a solita eorum acie tuto adscriberetur; quibus præmonitis, debiti ordinis & perspicuitatis gratia, ab observationibus Domini Strömer jam describendis incipiam.

Hic astronomiæ ad nostram academiam professoriam emeritus, tum vero officio Carolicoronensi defunctus, ad nos redux utebatur tubo refractionis 20 ped. Suec. cum oculari o. 35 ped. quem in observandis immerfionibus & emerfionibus satellitum Jovialium per viginti annos adhibuit; interposuit vero vitrum rubrum in observationibus Solaribus ab illo semper adplicatum. Ab hoc astronomo accuratissimo & quam maxime exercitato,

exercitato, contactus immerſionis Veneræ interior hora 3, 38', 5'' æſtimatus fuit; ordo vero obſervationum hic erat: hora 3, 15' Solem e nube, qua involutus fuerat, emerſum vidit, ſed Veneris adventum non niſi hora 3, 42' circiter exſpectabat. Eodem vero cognito h. 3, 21', Solem Veneremque intuitus, utriuſque margines male terminatas perſpexit. Hora 3, 24', 35'' margo Solis ad illas Veneris perpendicularis videbatur, ſed continuavit ſimili apparentia tempore 2 aut 3 minutorum, adeo ut verum momentum dimidii ingreſſus valde incertum eſſet. Dein appropinquante planeta ad contactum interiorem, Venus quidem circularis apparuit, ſed figuram excedens verſus marginem Solis, eodem fere phænomeno, ac guttula aquæ corpori adhæret, & hoc eadem ex parte magis extenſum præbet. Eadem excrescentia Veneris parva Solis cornua diſtinguebat, quæ ſubrotunda erant; Venus quoque largius ſupra marginem Solis extenſa, quam par credebatur, huic adhærens, immerſionem tamen intra pauca momenta perficiendam oſtendebat. Mutabatur paulatim ſingulare hoc Veneris incrementum, ex quo tarda videbatur Venus in margine Solis linquendo, aut foribus ingreſſa poſt ſe claudendis; Sol contra, ſub oſcillatione cornuum, contactum interiorem feſtinare & planetam levioſe vi a limine propellere, ut pleno amplexu ſua Venere fruereſetur. Sub brevi hac pugna, planeta, toto licet corpore ingreſſus, ligamine quodam margini Solis unitus conſpiciebatur; idem etiam diſtentum quaſi, in medio ruptum fuit hora ſupra dicta 3, 38', 5'', & ſpectatoribus Venerem intra Solem pauliſper moraturam pollicebatur. Diſtantia Veneris, cujuſ imago jam diſtinctior, a margine Solis mox apparuit & circiter ſecundis, ſive octava aut ſexta

parte diametri suæ. Sunt hæc fere totidem verba nostri Strömeri in Diario Observatorii Upsaliensis, quæ jam adduxi, ut insigne hoc phænomenon uberius inculcarem, quia a nemine observatorum extra Upsaliam æque perspicue fuit descriptum, aut cum suis circumstantiis adeo exacte observatum.

Sub mora transitus Veneris ad filum verticale observavit D. Strömer appulsus marginum Veneris ac Solis & horizontale quadrantis 3 ped. Suec. quos omnes hic recensere nimis longum foret. Accedente vero Venere egressura, ad marginem Solis occidentalem, paullo ante contactum interiorem, vel prius quam erumperet Venus, splendor Solis ante illam, sive ea ex parte, ad quam procedebat planeta, offuscabatur, & pressa ab illo Solis margo, atque in filum extenuata, flavescere incepit, mox vero cum exiguo Veneris disco coincidere & evanescere hora 9, 28', 0". Adnotavit hoc momentum pro contactu interiore D. Strömer in schedula, quam pro describendis observationibus ad manus habebat, & mox Solem respexit: verum jam largiter excisa videbatur Solis a Venere obscurata margo hora 9, 28', 7" ut miraretur noster tantam in illa a parvo planeta & tam brevi tempore factam aperturam. Illi præterea singulare hoc visum fuit, admodum obtusa apparere exigua Solis cornua, ut judicari posset, Venerem intra discum Solis, adhuc restare, licet ignota vi marginem ejus obtegeret, aut nostris oculis subduceret. Emergentem dein Venerem armato oculo persecutus, ejus centrum in margine Solis collocatum putavit hora 9, 35' circiter, ut supra de immerfione dictum fuit; contactum etiam exteriorem dubium accepit, mutato foco tubi ex percussione instrumenti sub fulcri transpositione, quæ paullo ante contactum evenit, ita ut genuinum

inum focum restituere non potuerit ante  $9^h, 46', 13''$ , quo Venerem Soli valedixisse dubitans credit.

Aderat Dominus Melander, mechanics tum director, nunc Strömeri successor, & astronomiam multo cum applausu scienterque profitens: illi vero tubum 16 ped. Suec. concessi cum vitris rubris, tenui & non polito pro immersione, crassiori vero et per obscuro pro emersione Veneris, quia omnem defectum vitrorum coloratorum antea supplere non licuerat. Quanta fuit acies & perspicacia hujus observatoris ex observationibus patebit; nam hora  $3, 37', 9''$  Venerem intra discum Solis contineri judicavit, ita ut circuli discorum se invicem tangere viderentur, licet obscurata maneret exigua pars marginis Solaris, super quam nuper procederat Venus: hora  $3, 38', 2''$  vero contactum interiorem absolutum vidit. Hæc ex æsse conveniunt cum observatione Domini Wargentini, nisi quod intervallum eorundem phænomenorum ab illo statuatur tribus secundis majus. Paulo ante contactum interiorem emersionis, defuit cochlea fulcri, quod tubum Melandri sustentabat, ita ut eundem manibus levare coactus fuerit, quod ipse infortunium sagaci prudentia patienter tulit, ne suas collegas turbaret, sub instante e Sole egressu; ipse vero oculis contactum exteriorem instantaneum determinavit hora  $9, 46', 29''$ .

Sequitur jam, ut in propriis observationibus recensendis pergam. Primo itaque undulationes marginis Solaris circum planetam tolli penitus animadverti, & extendi excavationem illius fere in lineam rectam; deinde intra pauca secunda dilatari obsurationem limbi Solaris ultra modum, quem ferebat magnitudo Veneris postea visa, & quemadmodum parva hæc Solis eclipsis post contactum exteriorem immersionis, quem



quem tamen videre non licuit, subito crescebat, ita versus contactum interiorē rationem figuræ Veneris non imitabatur, ut supra explicatum est in observationibus Dom. Strömer. Erat quoque hora 3, 27', 12'', cum Venus ultra dimidium immersa certo apparebat, licet per contactum interiorē evidens sit, centrum ejus ante horam 3, 28', marginem Solis non attigisse. Postea vero cornua Solis extra circulum disci circum Venerem paulisper surgentia videbantur, & mediante lumine fusco, quod Venerem circumdabat, integra hæc apparuit 4 circiter minutis post momentum mediū ingressus, vel forte antea si advertissem; imago tamen partis a Sole aversæ eo confusior quo magis a Sole distabat. Videbatur præterea Sol, sub vehemente limbi sui æstu, nisum exercere ad planetam complectendum, eo majorem, quo magis tendebat Venus ad contactum interiorē: ante hunc, porrexit sua cornua Sol extra circulum disci, discrimine vere notabili, versus horam 3, 37', 47''; post hoc vero momentum, circa Venerem oriebatur levior lux, primo confusa, deinde crescens in distinctiorem, ita ut hora 3, 37', 56'' cornuum Solarium conjunctio esset sensibilis; verum radii pone Venerem emicantes, sub debili adhuc Solis lumine, difficulter admodum observabantur, ita ut splendoris levitas dubium me redderet, utrum veram marginem Solis diutius expectarem, donec intra semi-minutum Venerem super Solem progressam, hujusque marginem clariorem post illam conspicerem. Equidem non diffiteor, novam hanc apparentiam me aliquantisper suspensum tenuisse, id tamen audeo asserere, contactum interiorē fuisse perfectum dicta hora 3, 37', 56'', meque, licet de indole phænomeni præmonitus fuisset, eundem ante 37', 52'' non potuisse

potuisse assignare. Advenerant paucis ante diebus suasiones Domini De Lisle, ut in observando transitu Veneris vitrum fumigatum cum viridescente conjungeremus, quam ob rem, accepto vitro rubro, loco fumigati, tenue ac viride illi super imposui, sed ob lumen Solis sub immersione admodum debile eundem apparatus tollere coactus sum, ut perspicacius viderem, & vitrum gracile virideque substituere, per quod Solis imaginem albescentem intuitus sum: eadem quoque fuit apparentia in descripto vitrorum apparatu, quem postea resumsi & constanter retinui.

Procedente Venere a contactu immersionis interiori, omnium primo ejus diametrum determinare studui, quam, mediante proportione mensurarum diametrorum Veneris & Solis, optime inveniri putavi. Diametrum igitur Veneris, quæ exigua erat, utrinque sumsi respectu initii scalæ micrometri Dollondiani, quod in meo telescopio has excursiones instituere finit, eamque constanter & sub toto transitu, non majorem 51 partibus scalæ, nec minorem 50 inveni. Diametrum etiam reperi eandem in quacunque directione mensuræ institutæ, sive verticali ad horizontem, sive obliqua utcunque, sive horizontali, adeo ut valde miratus sim Dominum De la Chappe diversas ejus magnitudines in diverso Veneris situ supposuisse: certe variantem ejusdem mensuram invenire mihi quoque contigit fatigato oculo; sed, & vigore restituto, & debita attentione adhibita, limites jam indicatos in dicto telescopio genuinos fuisse fidenter assero, & aciem oculorum vix intra pertingere nisi conjectura. Duplicem vero hic proposui limitem, quia de maximo & minimo illius determinando sollicitè laboravi, ambas Veneris imagines vel tantillum ad invicem retrahendo, vel ad arctiorem contactum perducendo.

cendo. Simili studio, limites diametri Solaris quæsi; verum margines Solis, ut & aliorum corporum lucentium, vix eadem accurate affeque licet; & quia minor in his jam requiritur exactitudo, limites inventas eo usque extendam ac fuit differentia observationum maxima, scil. diameter horizontalis Solis minima at rarius accepta fuit 1665 partium scalæ, maxima 1671, quam veræ propiorem esse plures observationes confirmant. Posita itaque diametro Solis =  $31', 35'' .5$  patet illam Veneris non fuisse minorem  $56'' .7$  nec majorem  $58'' .1$ , adeoque mediam pro apparente sub transitu ejus accipiendam =  $57'' .4$ , cui apprime conveniunt fere omnes determinationes diametri Veneris.

Inter has observationes, quas pro diametro Veneris invenienda institui, facile intelligitur, quod distantias Veneris a margine Solis inferiore minus neglexerim mensurare. Eas vero hic omnes recensere nolo, quoniam ab astronomis usum ejusmodi observationum hucusque factum nequaquam inveni, meisque observatis distantis calculum per otium applicavi, quem brevi absolvere conabor. Præterea existimo, mediantibus hujusmodi observationibus, satis tuto determinari parallaxin Solis ex unius loci observationibus; unde D. Mason nosque Upsalienses eandem parallaxin in potestate habere credo, per ingentem numerum distantiarum ab illo nobisque captarum: si quis vero meas observationes communicatas desideret, antequam supputationes dudum inceptas perfecerim, huic lubens adero, quia mensuratas distantias, ultra modum reliquarum observationum ad Venerem institutarum, certas & indubias existimo. Ex his autem, minimam centrorum distantiam a me sumtam in præsentis solum explicabo.

explicabo. Accidit vero ut, circa tempus dimidiæ moræ Veneris in Sole, postquam per horæ octavam partem quievisssem, assiduus in observando distantiam centrorum vix amplius variare deprehenderim; & quia de mediæ transitus Veneris nulla apud me opinio adhuc surgebat, illud phænomenon primo observationum vitio tribuerem; deinde, omni adhibita cautione, eidem medelam adferre studerem; sed quo major opera & quo certior essem de bene instituta observatione, eo magis distantias coincidentes observaverim. Jam itaque persuasus de exactitudine mensurarum, observationes refraganter quidem descripsi, sed fideliter tamen adnotavi, sub anxia mentis dubitatione de causa irregularitatis (licet præter naturam suppositæ) qualem in hoc instrumento antea non inveneram. Crevit etiam sub paucis momentis animi despondentia, quando distantias marginum paullo diminutas mox observavi, unde de mensuris exactissime capiendis maxime sollicitus tempori opportuno relinquere decrevi, ut de his rite disquirerem. Illustravit vero omnia Venus a centro Solis magis discedens, animumque sic reddidit timidulo suo observatori. Hæc vero cum ita evenerint, minor apud me manet suspicio de quantitate minimæ distantie centrorum Veneris & Solis haud exacte determinata. Quod ad methodum attinet, qua distantias marginum aut centrorum erui, hæc talis fuit: marginem Veneris Solisque in duplicata imagine ad contingentiam perduxim, & convolvendo micrometrum detexi speciem hujus contactus, in instrumento videndam, ut distantia a margine acciperetur minima, h. e. fita in linea recta, quæ per centra transit. Difficilior ab initio erat hæc opera, & parcius hinc numerus observationum, sed

paullo adfuetus uno fere ictu oculi perspexi, utrum genuinus esset contactus marginum Veneris atque Solis, mansitque error semper intra partem scalæ quæ minutum secundum proxime continet. Intelligent hoc observatores, qui macularum Solarium distantias a margine Solis proxima, simili instrumento, sumserunt, mihique brevitatem jam studentem concedent observationum certitudinem non uberius arguere, quia in ejusmodi mensuris rarissime ultra minutum secundum aberrare soleo, modo debitam attentionem adhibuero. Hac igitur ratione accepi distantiam marginis Veneris tam superioris quam inferioris, sive proximæ & remotæ, adeoque ipsius centri distantiam a margine Solis proxima, unde illam centrorum ex determinatis Solis Venerisque diametris facillime supputavi. Ad minimam vero centrorum distantiam ejusdem limites sollicitè quæsi, quos eo certiores existimo, quo magis ex observationum exactitudine unice dependent, & opinioni conceptæ potius repugnantes quam accommodatæ fuerunt: scilicet habebatur eadem distantia æqualis 520 aut 521 partibus scalæ micrometri, ipsaque diameter Solis capta in eadem obliquitate ad horizontem ac distantia centrorum planetæ Solisque fuit 1670 aut 1671 partium ejusdem scalæ. Observandum hic est, obliquitatis diametri Solaris mentionem studio hic factam, quia diametrorum Solis variationem in mensuris, pro diversa illarum inclinatione ad horizontem, soleo deprehendere, præter illam quæ a refractione dependet: eadem vero adeo est exigua, ut pauca tantum comprehendens secunda dubium me semper reliquerit, cuinam causæ erit tribuenda, an veræ apparentiæ, vel instrumenti defectui, vel etiam incertæ oculorum hebetudini, quæ continuatis observationibus

Solaribus

Solaribus paullatim crescit, & irregularitatem mensurarum prodit. Quicquid sit, accepta diametro Solis =  $31', 35''$ , ut antea, per rationes limitum minorum erit distantia centrorum =  $9', 50''.2$ , per rationem vero limitum majorum =  $9', 51''$ , harumque determinationum media =  $9', 50''.6$ : omnes autem hic nominatæ mensuræ, ut certiores essent, in eodem loco campi telescpii, prout fieri protuit, sumptæ fuerunt ex rationibus supra descriptis. Quamobrem a Domino Pingré dissentire cogor, quando eandem distantiam =  $9', 55''.6$ , ex mora Upsalienfi transitus colligit, quia, ut patet, ex immediatis mensuris determinatur quinque secundis minor, & observationum diversimode institutarum plenum consensum, ad me totidem secundis decipiendum, per cognitam mihi exactitudinem, & circumspeditionem, quibus peractæ fuerunt, denegare audeo: neque distantiam centrorum Veneris atque Solis hic stabilitam incongruam quis judicabit, modo consideret eandem ex utriusque marginis Veneris distantia a margine Solari deductam esse, unde errores mensurarum se invicem compensare necesse est, si qui ad imaginum contactum commissi fuerint. Præterea, omnis fere labendi occasio, qua distantias marginum nimis magnas invenire debui, præventa fuit, eo studio, quod nuper enarravi: haud etiam credibile videtur, si vera distantia foret =  $9', 55''.6$ , me, in omnibus mensuris, circa minimam distantiam sumptis, a directione centrorum Solis & Veneris per  $2^\circ, 23'$  constanter aberrasse, quod fieri oportuit, ut dictam centrorum distantiam quinque secundis vera minorem determinarem. Ex his itaque patet, parallaxin Solis, repugnantibus jam descriptis observationibus, a celeberrimo Pingré nimis augeri, & illam ex iisdem intra

novem secunda statuendam esse: quisque enim intelligit, data ratione diametri Solaris ad distantiam centrorum minimam, hanc ex illa cognita tuto determinari, & si quæ obscuritas limbi Solaris visioni noceat, debet eadem, aut in utramque mensuram æqualiter influere, aut extimas Solis partes invisibiles reddendo, Venerem ad marginem Solis proximam paullo retrahere, adeoque distantiam centrorum extendere; erit itaque distantia minima centrorum Veneris & Solis sub hoc transitu  $= 9', 50''.6$  pro observatorio Upsaliensi, in assumpta Solis diametro, in illa vero a Pingré supposita  $= 9', 51''.2$ , quod me parum moratur.

Progredior ad apparentias exitus Veneris e Sole, quæ omnino singulares mihi visæ sunt: ad has vero observandas præmonitus accessi, & similia in emerfione phænomena exspectavi ac sub immerfione, sed spe ex parte frustratus sum; eo diligentius tamen ea quæ apparuere potui adnotare; videlicet hora 9, 27', 30'' circiter, Venerem margini Solis multum approximatum vidi, & totam animi vim exercui ad momentum contactus interioris feliciter & exacte determinandum. Hora 9, 27', 55'' circuli discorum Veneris atque Solis se invicem contingere videbantur, sed Venus a Solis margine undique comprehendebatur, quia hæc supra Venerem elevata paullo apparuit. Eadem excrescentia Solis intra pauca secunda extenuabatur, & paullatim minuto lumine ejusdem, hora 9, 28', 1'' marginem Solis adhuc quidem continuam ostendebat, sensim vero evanuit, ita ut momentum præcisum vix determinari potuerit: tamen hora 9, 28', 3'' cornua Solis adeo distincta apparuere, ut Venerem per saltum excurrisse crederem. Figura Veneris circularis distinctior

stinctor quoque videbatur, a debili lumine, quod illam circumdabat, eadem ratione ac ante immerfionem, & obtusa Solis cornua ultra circulum disci exporrecta æstimabantur. Lumen vero Venerem, post hunc contactum, comprehendens paullatim evanuit, in parte a Sole averfa, & adversæ solum inhæsit, donec Venus fere tota emerferat: disparuit idem circa duo minuta ante contactum exteriorem, & Venere ad illum appropinquante, hæc rursus, quasi per lineam fere rectam, super marginem Solis, largo contactu, extendebatur, quemadmodum de prima apparitione immerfionis dictum fuit; deinde rotundius apparuit frustum obscuratum, tandemque in angulum desiit, qui hora 9, 46', 23'', major recto apparuit; postea idem angulus acuebatur, & speciem mucronis gladii exhibuit, qui unico instanti e Sole excidit hora 9, 46', 29''.

Hiscæ omnibus jam recensitis observationibus addimerentur illæ Domini Bergman, olim Matthesin ad nostram academiam legentis, nunc munere adjuncti philosophici eandem profitentis. Fuit vero tubus refractionis, quo hic utebatur, reliquorum omnium præstantior, quia vitrum objectivum 21 ped. Suec. ab illustrissimo Domino Klingenskierna A. 1754 fabricatum, oculare vero 0. 20 ped. a Lehnbergio nostro nuperrime paratum erat. Idem instrumentum proprio usui destinaveram, sed multiplicatis præter meam opinionem observatoribus, eundem tubum Bergmanno concessi, ut foci alterationem in telescopio meo, quæ, ob diversam oculorum constitutionem, necessaria erat, penitus evitarem. Est vero hic tubus diversis vitris rubris instructus, quorum tenuissimum adhibuit D. Bergman sub immerfione, crassius autem sub emerfione Veneris: evenit hinc ut D. Bergman, omnium



omnium præsentium optime armatus, contactum immerfionis interiorem, sub minus favente cœlo, citius exploraverit quam reliqui adstantes, nempe hora 3, 37', 43'', pariterque contactum emerfionis interiorem paullo ferius acceperit, quam a reliquis fuerit observatus, hora videlicet 9, 28', 9'': erat tamen minor hic differentia temporum quam in ingressu, ob circumstantias cœli, respectu apparatus, magis coincidentes. Tandem contactum exteriorem observavit D. Bergman hora 9, 46', 30'', eumque instantaneum judicavit.

Sequitur jam, ut nonnulla transitus Veneris phænomena repetam, & nostras meditationes circa præsentem materiam adferam. Primo itaque, durante ingressu Veneris, pars ejus immersa major semper adparuit, quam par erat: augebatur videlicet Solis diameter ab æstuatione limbi per motum vaporum, sed accedens planeta ejusmodi disci Solaris incrementum tollebat, quantum a Venere tegebatur margo Solis, unde etiam, quæ actu deprehensa fuit, minor æstuatione circa Venerem. 2. Lumen illud debile, quod planetam, ante immerfionem perfectam & post emerfionem inceptam, circumdedit, adstipulantibus reliquis observatoribus Upsaliensibus, & suo suffragio adnuente Domino Wargentini, qui idem simul conspexit, atmosphæram Veneris indicare credo, quæ sententia plena demonstratione haud carere videtur, si omnia jam recensita phænomena colligamus. Et si quis statuatur eundem splendorem ab atmosphæra Solaris derivandum esse, huic injunctum erit rationes explicare, cur in eclipsibus Solaribus a Luna perfectis desit omnino eadem apparentia. 3. Ex allatis colligitur ordinem observationum contactuum interiorum vim vel efficaciam

viam instrumentorum & apparatusum sequi iusta fere proportione, quantum serenitas cœli permiserit, adeoque earum exactitudinem esse in aprico, prout hæc a nostro officio & industria dependet; & nisi hæc obtigisset earum successio, observationibus minor fides vel auctoritas tribuenda fuisset, quemadmodum de Grenovicensibus idem merito docuit D. Short. 4. Denique patet, tutissimam fuisse observationem contactus exterioris emersionis, ex coincidente momento plurium astronomorum nulla ratione inter se communicantium, sed momenta contactus separatim, licet in eodem loco, adnotantium, ex audito numero secundorum, quem ex horologio Grahamenti, per vices sed constanter, enunciabant studiosi astronomiæ ad hoc munus dispositi & exercitati. Idem confirmat harmonia observationum D. D. Klingensfierna & Wargentini, qui Stockholmie fuerunt observatores in praxi astronomica maxime idonei, & optimis instrumentis instructi. Instituto igitur genuino examine observationum nostrarum elucet, neutram alteri præferendam esse, nisi ex rationibus apparatus instrumenti observatorisque in correspondente observatione, quod ut evidentius fiat, hic monebo, differentiam meridianorum Upsaliensis & Stokholmienfis, ex multiplici correspondentia observationum circa eclipses satellitum Jovis, derivatam esse  $1', 42''$ , quam etiam confirmant observati contactus, interior immersionis & exterior emersionis, adeo ut hanc  $1', 59''$  assumere non liceat, ut fecerunt D. D. Short & Hornsby. Præterea observandum, ephemerides D. De la Lande, quæ statuunt differentiam meridianorum Upsaliensis & Parisiensis esse  $1^h, 1', 30''$  corrigendas esse, ita ut ponatur eadem  $1^h, 1', 9''$  quando pro Stockholmia assignatur  $1^h, 2', 51''$ ,  
eandem

eandem differentiam deduci ex correspondentibus observationibus eclipsium satellitum Jovis, ut in Actis Upsaliensibus hoc demonstratum est ex communicatis dictis observationibus apud Parisios institutis. Huic vero differentiae meridianorum Upsaliensis & Stockholmienfis, quam nuper dedi  $1', 42''$ , repugnat, quod scio, contactus interior emersionis utrobique observatus, qui etiam D. Wargentini & me ipsum statim ab initio anxios tenuit, quia tantam discrepantiam momentorum, quae est circiter 20 secundorum, nulla ex certa causa deducere valemus; dedi tamen, & jam adducam conjecturam non levi fundamento innixam. Accidit Upsaliae, quod probe memini, & in diario descripsi, ut paullo post eundem contactum, emersionis nempe interioriorem, nubes Solem obtegerent, deinde cito transirent, & Solem perfectae claritati restituerent, antequam centrum Veneris in limbo Solis versaretur: potuit ergo, opinor, fieri, ut aëris quaedam intemperies adfuerit, quae a nobis haud perspecta fuerit, quemadmodum nubes appropinquantes praecedere solet tenuior nebula, quae ab oculis observatorum interdum non percipitur, at in phaenomenis eclipticis accelerandis vel retardandis maximam vim exercet, quia horum momenta citius vel serius, pro diversa observantium aëre, saepius adnotantur, & eorundem perspicacia ab aëris coelique serenitate dependet. Haec confirmant plures comparationes immersionum & emersionum satellitum Jovis, Upsaliae & Stockholmiae observatarum; confirmant quoque aberrationes altitudinum correspondentium Solis, si quando nubes leviores huic appropinquaverint. Sic igitur Sol tenuiore hoc velo, nobis infelix, forte obductus, potuit contactum emersionis interioriorem aliquot secundis acceleratum ostendere.

Provoca

Provoco ad astronomorum experientias, si quid huic simile ex aëris constitutione derivandum probent, non aliter namque licebit diffensum momentorum explicare, quando observationes, ut apud nos factum, in nexu phænomenorum satis exacte conveniunt. Elucet quoque, accelerationem phænomenorum observationibus Upsaliensibus tribuendam esse, quia contactus exterior paullo post feliciter observatus cum illo Stokholmienfi exactissime adnotato plane coincidit.

Quemadmodum igitur ex allatis patet, neutram observationum Upsaliensium alteri præferendam esse, ita mea sententia perspicuum erit, nulli harum postponendas esse observationes Stokholmienfes, quod ex superioribus a D. Short concessum iri credo, licet in sua Transfatione contactum interiorem immersionis a D. Wargentini observatum, quasi erroneum assumat, quod nequaquam probandum existimo. Novi, & ipse monstravit, suam in hoc puncto sagacitatem dictus D. Short; sed nostras e longinquo institutas observationes eum haud probe cognovisse patet, quod in minori cum nostris astronomis commercio ægre feret nemo. Cavendum præterea hic est, ne cum nonnullis illas imprimis observationes seligamus, quæ nostræ sententiæ de parallaxi Solis magis favent. Concedamus quoque observationibus hujus ordinis eam incertitudinem, quam inter plures ejusdem loci observatores cum diversis instrumentis regnare satis evictum est. Equidem scio moram transitus Veneris a me observatam minorem esse, quam quæ convenit distantiae centrorum ope micrometri Dollondiani a me determinatæ; sed unam vel alteram rejicere incongruum puto, nisi ex peractis disquisitionibus, (quarum insignia exempla dedit D. Short) quantum officiant errores nunquam

fatis evitandi pateat : ex illis enim iusto demum colligitur cuinam observationum generi major fides habenda sit. Norunt astronomi accuratiores, & aperte docuit D. Bouguer in Essai d'Optique, quod margines Solis minus fulgeant, quam partes Solis centro propiores : forte hinc mirum non erit, margines Solis in contactibus interioribus diutius obscurari, quam par est, & moram inter eosdem nimium diminui pro ratione distantiae centrorum Veneris & Solis, in cujus determinatione levior ex allata ratione metuendus est error, quod ex indole observationum intelligitur, & ex precedentibus uberius colligitur. Taceo vires radios luminis reflectendi limitatas in utroque speculo telescopii, nec non vitrorum tam objectivi in micrometro, quam ocularium & coloratorum in telescopio non perfectam pelluciditatem, ex quibus determinatur raritas vel densitas radiorum a quovis puncto ad oculum pertinentium ; hæc autem omnia in censum revocari posse ex eo probo, quod diametrum Solarem majorem soleo invenire, adhibitis vitris coloratis gracilioribus quam magis opacis ; minorem quoque si levior intercedat nebula. Subsistit itaque irregularitas paucorum secundorum, quam ex observationum circumstantiis rite explicatis unice dijudicare debemus : in eadem vero consideratis patet observationem motus transitus a me factam debuisse differre ab illa D. Wargentin, si utraque genuina fuerit, contra vero hujus observationes, excepto contactu interiore emersionis, Upsaliæ observato, debuisse coincidere cum illa Bergmanni, ob instrumenta & apparatus proxime similia.

Concludam, parallaxin Solis ex observata distantia minima centrorum Veneris atque Solis sequi minorem  
9 secun-

secundis, ut supra monui; sed nolle me hoc opus suscipere, ut litem inter astronomos ortam de vera Solis parallaxi, nisi post longum rationum examen, dijudicem: etenim observationes circa Martem institutæ annis 1750 & 1751 uno quasi ore clamitant, parallaxin Solis esse 9 sec. majorem, quod ex suis quoque observationibus monet D. Pingré. Contra vero observari merentur correctiones momentorum contactuum a D. Pingré, sine ullis adjectis rationibus, introductæ, ex quibus de illis controversi potest, nisi ex reliquis ejus observationibus per calculum evincatur, ultimo proposita momenta esse solum genuina. Favent quoque minori parallaxi Solis observationes contactuum a Maso peractæ, si cum Europæis fere omnibus comparentur, ut abunde demonstravit D. Planman professor physices Aboensis, in actis Stockh. 1762: restat vero, ut earum fidem ulterius urgeat D. Maso ex distantis centrorum observatis, ne quid in opere calculi desit, quod ad observationum veritatem probandam facit. Perpendendum insuper credo, quousque extendatur certitudo observationum parallacticarum Martis, vel in quam partem pronior sit labendi occasio, quæ ex indole instrumentorum & methodorum observandi optime intelligi potest. His enim rationibus, ut arbitror, veram certamque Solis parallaxin ex jam factis observationibus colligere licebit, vel saltem earum irregularitati & certitudini medelam adferre in proximo futuro Veneris transitu A. 1769. Dabam Upsalæ 28. Maii 1765.

Fredericus Mallet,

Astron. obs. reg. Ups.

Received Feb. 24, 1766.

XIII. *A Hepatitis, with unfavourable Symptoms, treated by Robert Smith, Surgeon at Edinburgh, now at Leicester.*

Read April 24,  
1766. **M**RS Morton, aged 26, of a spare habit and a small size, in the summer of the year 1750, frequently complained of a pain in her right side. About the middle of July, the same year, a violent vomiting and pain in her stomach introduced an acute fever, accompanied with a constant deep-seated pain in the right side, under the costæ nothæ, a little lower than where the usual pleuritic symptom in either side is generally felt.

Copious bleedings, to sixty ounces and upwards, with a vesicatory on the affected part, and pectoral medicines internally, afforded some temporal relief.

July 27th, Upon the account of an intolerable pain at her stomach, a very uncommon uneasiness, and gripings in her belly, a medicine was exhibited, composed of pulv. stann. corralin. &c. and the day following, she voided by stool a large annular worm, about half an inch broad, in length six yards and a half.

August 1st, This day, upon examination, was discovered a considerable tumor, suspected, from its situation, and previous symptoms, to be formed in the anterior part of the liver, of an oblong figure, and  
extended

extended its longest diameter across the epigastrium about seven inches.

The patient, by this time, greatly debilitated by the large evacuations and fever, which still existed, became so low and dispirited, that she had given over all thoughts of recovery, her husband and relations being of the same opinion. To Dr. J. Dundas, an eminent physician, who had occasionally attended, I proposed making an incision into the tumor; though the event, under the present circumstances, had but an indifferent aspect. This proposal was, however, approved of by the doctor, our patient, and her relations, under the following terms, viz. to have the opinion of the principal surgeon or surgeons in that city on the expediency of the operation, in order that, should the experiment prove unsuccessful, there might be no blame imputed afterwards.

Strong suppuratives, in the form of cataplasms, were now used, whereby the tumor became more prominent in two days; a very deep fluctuation being felt, a large caustic was applied on the most depending posterior side, thereby to avoid hurting the stomach or its appendages by an incision, which was made several hours after, from whence issued a copious discharge, at first purulent, at last glutinous, resembling the white of an egg: no adhesion to the peritoneum could be felt, though accurately tried all round with the finger.

Great attention and care were used in the proper applications, bandage, &c. particularly in the posture of the patient; ivory and silver flat cannulas, kept in the aperture, were materially beneficial, as well for the conveyance of balsamic injections, as to facilitate the



the exit of the putrid contents. The 3d night after the operation, she turned delirious; this symptom, with an increased fever and excessive cough, afforded little or no hopes of a recovery, the more especially, as the discharge was now turned excessively thin, of a dusky colour and very foetid: for these reasons, I dressed her twice a day, throwing in large quantities of a warm injection, composed of a decoct. ficuum, and rad. alth. wherein was dissolved bals. capiv. to which was added, when the fever abated, some calomel ppt. In the mean time medicines internally, to allay her fever and cough, were not neglected; and she afterwards took daily, as her stomach and other symptoms would admit, a light infusion of cort. peruv.

By these means strictly followed, about the 21st day from that of the incision, a laudable pus was obtained; but on the 23d, a thin sanious discharge in great quantities burst out, worse than the former, and extremely foetid.

Towards the end of the month, it began once more to assume a benign aspect, but broke out a 3d and 4th time, on the 1st and 15th of September, every time the discharge growing more and more acrid, so as to excoriate and inflame the external parts; notwithstanding these threatenings, by a close perseverance in the forementioned method, at the end of ten weeks, a callous cicatrix was obtained upon the external wound, and the recovery compleated soon after by the use of a few alterative mercurial pills.

The woman is still alive, now (1766) in London, and enjoys a middling state of health; only has been liable to complaints of gripes and indigestion, every three,

three, four or five months. Her last complaint was generally relieved by a few saponaceous pills.

I am,

S I R,

Your obedient humble Servant,

Leicester, Feb. 19,  
1766.

Robert Smith.

Received March, 1766.

XIV. *Experiments on the Peruvian Bark, by*  
Arthur Lee, M. D.

Read May 1,  
1766. **T**HE great object of experiments is to establish principles, on which practice may be conducted in the most expeditious and unerring manner. The intention, therefore, with which the following experiments on the Peruvian bark were made, was to confirm the pharmaceutic treatment of this medicine where it was just, to correct it where it was erroneous, or to improve it where it was defective.

Experiment I.

In the first experiment, I infused two ounces of the powder of Peruvian bark, in a pound and a half of distilled water; after 24 hours, it was filtered, and the filtered

filtered liquor subjected to distillation in B. M. till about half came over, which was limpid and of a disagreeable slightly aromatic flavour: what remained in the retort deposited, on cooling, half a dram of a resinous substance, perfectly dissoluble in rectified spirit of wine: the liquor being then evaporated in B. M. left about three drams of a gummy substance intensely bitter and acerb.

Ob. It seems to appear from this, that the aqueous infusion contains the aromatic part of the bark, with a little of the resinous and a considerable quantity of the gummy substance.

### Experiment II.

The residuum of bark, from the former experiment, after extraction for two hours, in B. M. with eight ounces of rectified spirit of wine, produced a tincture, which, when decomposed with water, left three drams and eighteen grains and a half of resinous matter. The water that was filtered from it was slightly bitter.

Ob. The bark therefore seems to contain three soluble parts, namely, the aromatic part, to be extracted by cold watery infusion, a gummy part, chiefly dissoluble in water, and a resinous part dissolved plentifully by spirit of wine. The quantity of dissolved matter obtained in the above experiments is in much greater proportion than in the experiments of Mr. Boulduc and Mr. Neumann. Dr. Lewis observes, in his notes upon Neumann, that different sorts of Peruvian bark differ considerably in their yield of extract, which, together with the heat used in making the spiritous extract, may account for this disparity  
in

in my experiments and those of the gentlemen just mentioned.

### Experiment III.

Two drams of a tincture obtained from one ounce of powdered Peruvian bark, with four ounces of rectified spirit, and filtered without pressure, gave two grains of resinous matter; but the same, when the filtration was finished, with pressure on the top, gave six grains of the same substance.

Ob. It may therefore seem advisable to use pressure always in filtering this tincture; and perhaps all the other tinctures would be rendered stronger by the same means.

### Experiment IV.

Two drams of the powder of the external lamina of the bark, digested in the cold with rectified spirit of wine, afforded two grains of resinous matter.

### Experiment V.

Two drams of the internal lamina, treated as above, gave a grain and half of resinous matter.

Ob. Though this single trial cannot determine the comparative quantity of resinous matter in each lamina; yet it may shew, that each of them contains it, which, as far as I know, has not yet been proved by experiment.

### Experiment VI.

Two tinctures were prepared from the same quantities of the same bark, and of the same rectified spirit of wine; but the one was digested two hours, in B. M. the other 24 hours in the cold: a dram of the former yielded three grains, and the same quantity of the latter one grain of resinous substance.

Ob. Were this experiment sufficiently authenticated, it would establish a great preference in favour of the method of extracting tinctures in B. M. not only from its producing them more strong, but from the much greater expedition with which the process is executed.

#### Experiment VII.

Half a dram of the powder of bark was infused for 24 hours in caustic volatile alkali; and the tincture produced did not effervesce with acids, but retained a volatile-alkaline smell.

Ob. Is it therefore probable that the caustic volatile-alkali does not attract fixable air from the bark?

#### Experiment VIII.

When some of an aqueous infusion of the bark was poured to some caustic volatile-alkali; a white cloud appeared at the bottom of the phial, which soon disappeared, and the liquor became of a high reddish colour, without shewing any effervescence with acids.

#### Experiment IX.

Lime water and powdered bark, being macerated in the proportion of ten ounces of the former to one of the latter, in a well stopped phial, and then filtered; neither affected the colour of violet paper, nor precipitated the corrosive sublimate from its solution in water.

Ob. This experiment was made a year before Mr. McBride had published his very ingenious essay on the dissolving power of quick lime. The view with which it was made, was to determine, whether, after infusion,

infusion, the lime water remained unaltered in its alkaline properties. And though I then conceived the reduction of the quick-lime to arise from the attraction of fixable air, yet it was by no means with that clearness and certainty, which arise from Mr. McBride's experiments. This experiment was repeated several times, with the same event.

#### Experiment X.

Lime water being poured to a watery infusion of the bark, gave it a high colour, and they remained pellucid; they tinged violet, green; and were distinctly alkaline and bitter. These things were observable for some hours after the mixing them.

Ob. This experiment seems to shew, that the air of the bark in substance, which reduces the quick-lime, does not enter into the infusion, or else adheres to it so firmly, that it cannot be similarly attracted by the quick-lime.

#### Experiment XI.

A cold infusion in common water produced no change on the syrup of violets.

Ob. I have somewhere seen it asserted, that the infusion turned violet, green; from whence an alkaline quality was inferred; to examine the truth of which, this experiment was made.

#### Experiment XII.

Half an ounce of powdered Peruvian bark was infused for 24 hours, in six ounces of a solution of sal ammoniac; when the menstruum had received a slight colour, but still remained clear and saline as before.

## Experiment XIII.

Six ounces of a solution of common salt, infused for 24 hours, with half an ounce of the powder of bark, received from it a deep red colour, but retained its saltish taste.

Ob. It was intended, that the two above experiments should shew, whether the salt was precipitated from the water, during the solution of the bark.

## Experiment XIV.

A well-saturated tincture of bark, with rectified spirit of wine, being added to lime water in the proportion of one third, suffered an immediate decomposition of its resin, as by common water; and in an hour, it made no change on the colour of violet paper.

I was in doubt here, whether the decomposition might not arise from an affinity between lime water and spirit of wine, as in common water; to determine which, I made the following experiment.

## Experiment XV.

The pure resinous substance of the bark, put into lime water, was immediately dissolved, and the lime water reduced.

Ob. There seems therefore to have been a double elective attraction in the fourteenth Experiment. That is, the quick-lime, attracting fixable air, was reduced; and the water, uniting with the spirit, the resin was precipitated. Yet it is an objection to this supposition, that Mr. M<sup>c</sup>Bride's experiments prove the solution of resins to be attended with the loss of their fixable air.

Experiment XVI.

Vitriolic acid dropped into the solution (Exp: 15.) precipitated its resin.

Experiment XVII.

Common water dropped into the solution (Exp. 15.) united with it uniformly.

Experiment XVIII.

Five grains of the resinous part were agitated with one ounce of water, and one grain was dissolved.

Experiment XIX.

Five grains of the resinous part, being rubbed with an equal quantity of fresh quick-lime, and agitated with an ounce of water, were all dissolved to one grain.

Ob. The very ingenious discovery of Mr. M<sup>r</sup>Bride, regarding the abstraction of fixable air in the solution of resinous bodies, sufficiently explains these experiments.

Experiment XX.

A dram of bark was infused for 24 hours in an ounce and an half of spirit of wine, then filtered, and the tincture decomposed with water, and again filtered, so as to leave the resinous part in the filtre; the gummy part which passed the filtre, turned a solution of green vitriol, black. The resinous part was agitated with water, to purify it from any adhering gum; and what passed the filtre a second time, gave a very light tinge of black to the solution of vitriol.

Experiment XXI.

The resin, obtained as above, was rubbed with quick-lime, and then dissolved in water, which solution, when



when filtered, gave a manifest black tinge to the solution of vitriol.

#### Experiment XXII.

The officinal decoction, when cold, gives an evident black colour to the solution of vitriol.

Ob. Dr. Lewis observes, that the decoctions of the bark affect the solution of vitriol in a much slighter manner than the cold infusion; from whence he infers, that the latter is more fully impregnated with the vegetable gummy matter than the former. I cannot say that I have observed any difference in the colour they strike with the solution of vitriol, and am inclined to think, the resinous as well as the gummy part possesses this power.

#### Experiment XXIII.

The matter, which had subsided from an officinal decoction, and which appeared to be purely resinous, dissolved in rectified spirit of wine, changed the solution of vitriol to a black colour.

Ob. The spirit of wine alone produced no change in the solution of vitriol.

#### Experiment XXIV.

Three parcels of bark, of half an ounce each, were infused in equal quantities of the proof spirit of the shops; after 24 hours one portion was filtered; another after 48 hours; and the third after 72 hours, or three days. The same quantity of each filtered liquor was united with equal quantities of the solution of vitriol; and after the decomposed matter had perfectly subsided, the united liquors were each passed through three filters of the same weight, which, when dried, were weighed

weighed again, and found to be perfectly equal, each having gained six grains of additional weight.

Ob. The common practice of prolonging the infusion to three days at least, is always tedious, frequently inconvenient, and, if we may trust this experiment, not necessary, because not useful, since the menstruum appears to have been as fully impregnated after one day's infusion, as after three.

#### Experiment XXV.

Three parcels of the same powdered bark were put, with six ounces of water to each, to infusion at the same time. After twelve hours the first was filtered, the second after twenty four, and the third after six and thirty hours infusion. The filtered liquors were not discernibly different in colour and taste, nor in a residuum obtained from each, as in Experiment 24, was there any perceptible inequality on the scale.

Ob. This experiment may direct our practice in preparing the cold watery infusion with more precision than is commonly known. I apprehend too, that it may further serve to amend our pharmaceutical practice in many other similar points, in which our veneration for the antients has induced us to acquiesce in their forms, which they did not found upon experiment, the only admissible test of their propriety.

These are all the pharmaceutic experiments I have hitherto made on the Peruvian bark; they were intended as a part of a compleat history of this medicine, which, though almost finished, an unexpected and indispensable call into my own country, prevents me from making public. I will just beg leave to subjoin a remark, concerning the tincture of the bark  
with

with rectified spirit of wine, prepared by heat. I found the filtered water, made use of to precipitate the resin, so strongly impregnated, as to be more intensely bitter than the watery infusions; from whence I conclude, that spirit dissolves not only the resinous, but the gummy part, more powerfully than water; and as it is a more expeditious way than common decoction or infusion, it might be more eligible for preparing the officinal extract. I have remarked too, that, after one such extraction, the remaining bark is almost wholly insipid, which shows how great the extracting power of spirit is, when aided by heat. In making this tincture, it is necessary that the stopper be taken out of the phial, a little after it has been in the heat, to let the extricated air escape, so that it may afterwards continue stopped without any danger.



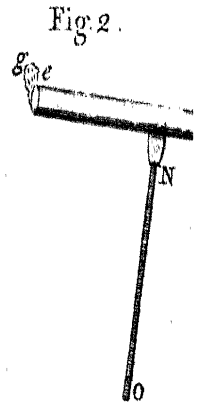
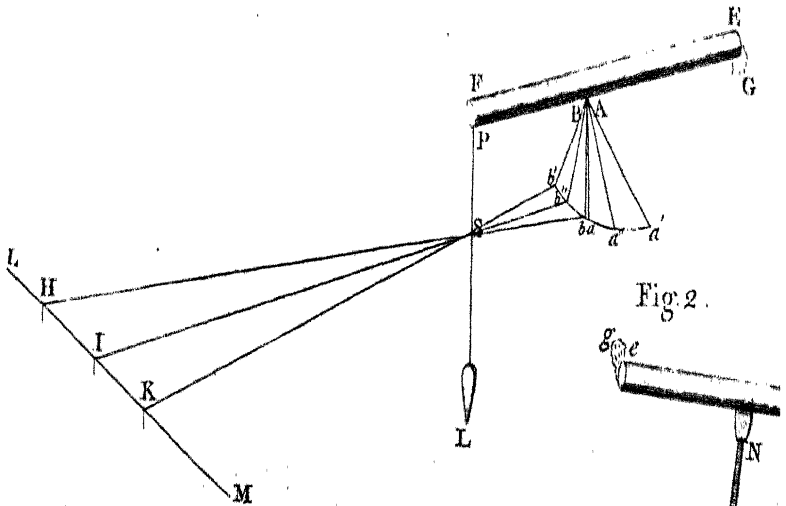


Fig. 4.



Fig. 5.



Fig. 6.



Fig. 3.

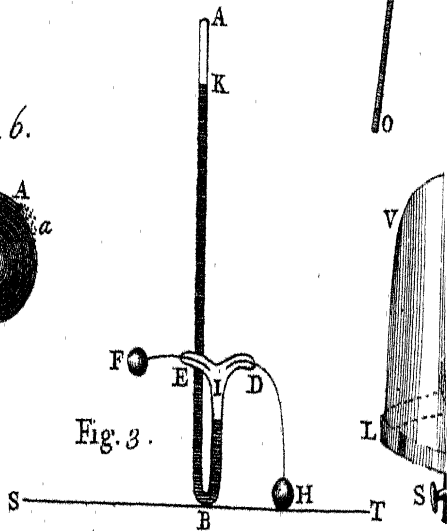


Fig. 7.

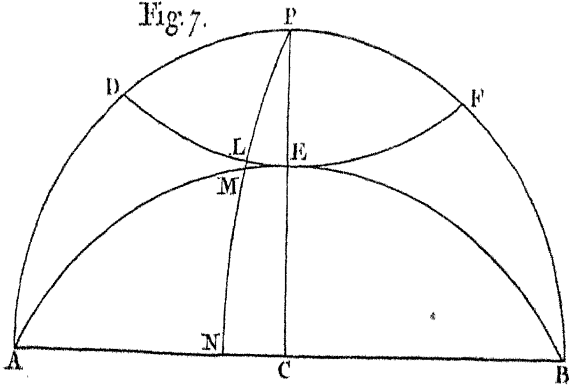


Fig. 8.

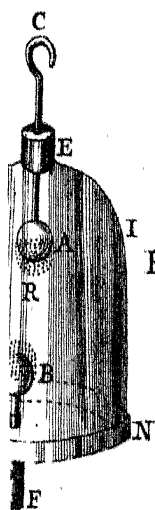
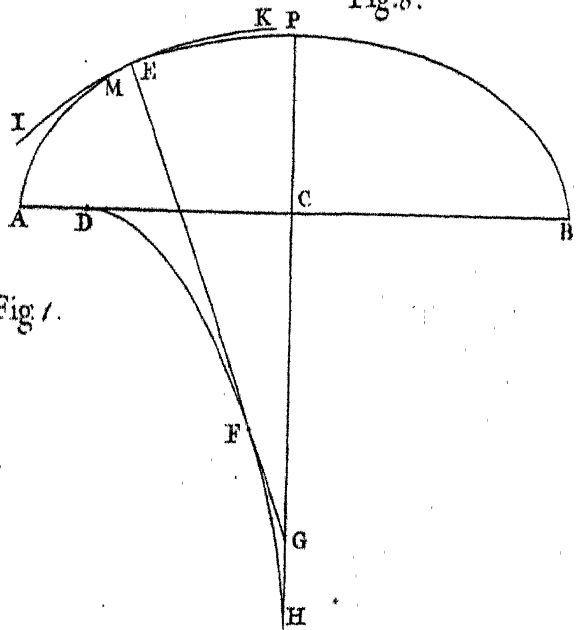


Fig. 1.



XV. *Novorum quorundam in re electrica experimentorum Specimen, quod Regiæ Londinenſi Societati mittebat die 14 Januarii, Anni 1766. Joannes Baptiſta Beccaria ex Scholis Piis, & R. S. Soc.*

EXPERIMENTUM PRIMUM.

Read May 1,  
1766. **N**EGO ceram ſignatoriam, ſulphur, dum fricantur, orbari ſemper electrico igne ſuo. Vultis in hæc corpora ignem congeram alienum? Frico charta inaurata. Equidem ut igne exuam ſuo, ſatis eſt, ſi chartam invertam, & facie fricem nuda; ſed ideo plane eloquentius videtur mihi experimentum; demonſtrat enim a tantula ſuperficiæ craſſitie pendere electricitatis contrarietatem, quantulam facit bracteola metallica.

Quod attinet ad contrarietatis veritatem, hæc experior. Quum alternatim utor facie inaurata, & nuda, penicillus & ſtellula mutant loca ſua omnia in machina, in catena, in globo ipſo, ſive ex ſulphure iſ conſtet, ſive ex cera ſignatoria, &, quæ diſceſſerunt, fricando una facie chartæ, ſtamina lini catenæ nexa, vel machinæ, accedunt, fricando facie ejuſdem chartæ altera\*.

\* Quum hoc ſpecimen optime merito de re electrica ſocio mitterem; ceræ ſignatoriæ, ſcribebam, annulum valde craſſum advolutum cilindro ligneo, dum hic rotatur in machina electrica, cultro rotundo expolio, abrado, ceu faciunt tornatores, & ea ipſa in abraſione catena ſit exceſſu electrica, machinæ defectu; ſi facie frico



Si frico simul globos ex cera signatoria duos eidem nexos catenæ, alterum nuda charta, alterum inaurata, perit electricitas altera vi alterius contraria: neque aliam a Frankliniana rei causam confingi oportet; scilicet quantum ignem immittit in catenam globus is, qui fricatur chartæ facie inaurata, tantum haurit alius, qui fricatur nuda.

Etenim Roberti Simmerii conjectationes de potentiis geminis, quas appellat antagonistas, miror ego, non moror. Profecto nihilo res adversatur maxime, excessus defectui. Excessus elasticitatis aërem movet; quidni similis causa (quæ est Frankliniana Theoria) moveat ignem electricum? Una autem directione, quod rem conficit, moveri ignem electricum demonstratur

#### EXPERIMENTO ALTERO.

Tab. V. Fig. 1. In vitrum pneumaticum VF admodum capax, per collum E, init virga metallica CA, definens in sphæram metallicam A pollicarem politissimam. Sphæræ huic occurrit, ad aptum quoddam intervallum, æqualis sphæra B, existens ex lance machinæ pneumaticæ LN. Subducto aëre ex vitro, converfoque epistomio S, disjungo a machina pneumatica lancem

nuda, electricitates eunt in contrarias; si facie frico ejusdem chartæ inauratæ, iterum catena excessu, machina defectu electricæ fiunt, & sic deinceps. Cl. Wilsonus respondet experimentum se dudum antea cepisse, quo ceram signatoriam dudum per aliquot horas intactam fecit electricam defectu, semel ipsam fricando laevi argenti manu, deinde eandem iterum tranquillam per horas totidem fecit electricam excessu, fricando iterum semel, & similiter blanda pressione lævi eadem argenti facie, sed paululum inclinata, ut ipsius limes ceram premeret. Ad sulphur quod attinet, globo experiebar, in quo sulphuri admixtum fuerat colophonium, ne facile fatisceret.

cum

cum vitro; prehendo manu lancem LN, tum unicum C virgæ AC ineuntis per collum vitri admoveo catenæ (pono hanc fieri electricam frictione vitri) atque circa imum hemisphærium A virgæ ejusdem micat atmosphæra electrica, qua nihil in re electrica pulchrius vidi, aut ordinatius. Etenim tenui quidem ipsa est luce, luce tamen distinguitur usque & usque vividiore versus imum suum locum R. Forma hemisphærium lucis diceret majore diametro, existens ex imo sphæræ metallicæ hemisphærio. Interea vero circa sphæram B, quæ ad lancem pertinet, & cum solo communicat, atmosphæra nulla, nulla omnino deprehenditur lux. Pergo ad machinam constanter electricam, admoveo ipsi similiter unicum C; simillima atmosphæra illucescit circa sphæram B; circa sphæram A, lux cernitur omnino nulla. Generatim (uti expertus sum) quæ sphæra communicat cum corpore, in quo cum Franklino electricum ignem densiorem dicimus, atmosphæram habet electricam; quæ cum corpore, in quo dicimus ignem rariorem, habet nullam.

Monebo tamen aliqua in hoc experimento opus esse patientia, solertiaque; nam pro magnitudine electricitatis, & pro accuratatione vacui, augendum intervallum sphærarum A, B, ne electricus ignis coeat in radium, eaque forma profiliat a sphæra ad sphæram, uti in aëre admodum raro plerumque contingit. Sed sagax quisque naturæ vestigator semihora vix indigebit, quo faciat sibi satis. Quam experiendi patientiam videtur non demereri pulchrum hoc ac plane eloquens experimentum.

Etenim ipso demum oculis cernitur, qua ex parte ignis electricus proxime erumpat, unde fides fit maxima duobus, quos a principio, & primus usurpavi,

pro directione ignis electrici testibus, penicillo, & stellulæ. Scilicet constitui penicillum igne fieri erumpente, stellulam ineunte: ac plane penicillus erumpit ex iis corporibus, queis contigua sphæra metallica atmosphæra distinguitur electrica, stellula in iis corporibus fulget, queis contigua sphæra atmosphæra distinguitur nulla.

Pulcherrima itaque Roberti Simmerii experimenta circa serica tibialia huc redeunt omnia. Mutua frictione, tibiale album haurit a nigro ignem electricum nativum, qui est omnino plurimus. Intumescit seorsim tibiale utrumque, quod corpora æque electrica discedant, & partes singulæ tibialis unius sint æque electricæ; nam æque carent igne suo partes nigri, & partes albi æque redundant alieno; admota accedunt, quod accedant corpora inæqualiter electrica. Sed hæ sunt leges phænomenorum. Audebo alias experimentorum complexionem proponere, quæ causam jam plane prodere videtur discessionum, accessionum, cohæsionum omnino mechanicam.

Noletus, in suis ad experimenta Simmerii animadversionibus: Tæniæ, inquit, sericæ albæ vitro fricatæ ipsi adhærescunt, divulsæ ad ipsas redeunt præcipites. Hinc legem refellit, qua constitutum, corpora eadem prædicta electricitate se mutuo repellere. Sed, quod affert experimentum, confirmandæ erat aptius huic legi, quam convellendæ; nam tæniæ sericæ albæ exuuntur a vitro igne suo, quem adeo accipit vitrum. Quod tænia alba a nigra ignem accipiat, non fit inde, ut accipiat etiam a vitro. His fallaciis amovendis, & amplificandæ theoriæ electricæ universæ, atque novæ corporum omnium affectioni vestigandæ, utilis mihi videtur sequens tabula.

*Effecta*

*Effecta experimentorum, quibus vestigatum est, utrum duorum corporum, quæ mutuo fricantur, accipiat ignem electricum alterius, utrum det alteri ignem electricum suum.*

*Corpora aptata in machina electrica, rotata, & fricata similiter ac aptatur, rotatur, & fricatur vitrum.*

*Corpora, quibus fricavi, ceu communiter fricatur vitrum manu ac pulvillo.*

Vitrum lamellare  
aut solidum

politum  
ignem accipit

asperum  
dat

accipiunt

Pili leporis

dant

{ a corio inaurato, a charta inaurata, a manu *plurimum*; a corio nudo *multum*; a capillis, a tibiali albo, a charta nuda, a tibiali nigro *modicum*.

{ manus (uti istuc inventum) pilis leporis, felis, martis, mustelae.

{ a corio inaurato, a tibiali nigro, a vitro aspero *plurimum*, a numismate aureo, a numismate argenteo, a charta obducta bracteolis aureis, argenteis, aeneis, aut stanneis, a lamina oricalcica, aut stannea, aut ferrea, aut plumbea, a magnete, a regula ex fago, a tibiali albo, a taenia serica caerulea, a corio nudo, a manu, a charta nuda, a vitro aspero *pauculum*.

{ vitro laevi lamellari, pilis felis, sive albi, sive nigri sint, pilis aliis subtilioribus, vitro laevi solido.

			a tibiali nigro (quod est experimentum Simmerii) a numismate zureo, a lamella laevi aurea, a numismate argenteo, a lamina argenteo laevi, a panno nigro, a corio inaurato, a charta bracteolis aeneis, aurcis, argenteis, stanneis obducta, a lamina stannea, a vitro aspero, a panno serico villoso nigroque, a theca Sandaraca obducta, vulgo verniciata.
	accipit	{	chartae nudae, manui, capillis, pilis felis, leporis, mustelae, vitro laevi, panno serico villoso, sed albo.
	( album   dat	}	tibiali albo, panno serico villoso albo, pilis felis, capillis, pilis leporis, mustelae, martis, tubo vitreo laevi, magneti, oricalco, argento, ferro, manui, panno nigro ex lana.
Tibiale sericum	{	nigrum (dat	{
			a corio inaurato, a charta aureis, argenteis, stanneis, aeneis bracteolis obducta.
Cera signatoria, & Sulphur	{	accipiunt	{
		dant	{ capillis, pilis felis, mustelae, martis, manui, corio nudo, chartae nudae, tibiali nigro.

Expono paucis in uno, vel altero horum corporum, qua ratione expertus sim in omnibus. Tibiale nigrum cilindro vitreo advolve, tendo, alligo, ad suo; apto in machina, ita dum rotatur tibiale nigrum, frico manu immissa in tibiale album; atque tum filum metallicum exhibitum tibiali nigro vibrat penicillum, effundit similiter penicillum filum metallicum nexum machinae, aut exhibitum catenae. Contra filum metallicum exhibitum machinae, aut nexum catenae demonstrat stellulam.

[ III ]

Sufficio cylindrum vitreum convolutum tibiali albo, frico nigro; stellula invadit loca penicilli, penicillus stellullæ.

Sed quoniam tibialia, & pelles cilindro vitreo, aut sulphureo advolutæ corporibus quibusdam nequeunt aptissime fricari, hinc experior etiam aliter. Experimentum pono. Famulus distendit manu utraque pellem felis calentem abs igne, ne ullus præterea inhærescat humor; tubo vitreo ego frico, mox aspera ipsius parte, mox lævi; cum frico parte aspera, filum ferreum exhibitum pelli distinguitur stellula, at exhibitum vitro effundit penicillum. Effundit, inquam; repente enim, & singulari cum crepitu atque expansione explodit. Quæ adeo manifesta effusionis indicia non vidi alibi. Res fit in tenebris. Equidem cum filum ferreum exhibetur pelli, tum etiam videtur identidem emittere penicillum; sed si attendas, eum penicillum non ad pilos dirigi cernes, sed ad vitrum asperum, dum subit filo ferreo inter fricandum.

Cæterum quæ hæctenus ex tabula hac existere videntur leges, cernet quivis\*; constituo ego nihil, quamdiu ipsam omnigenorum corporum experimentis non amplificavero; cui rei maxime inhio 1. ut id ipsum certius intelligam, quæ corporum mutuæ affectiones in causa sint ignis electrici sui impertiendi, aut accipiendi alieni; 2. ut explorem, num natura corporum cohæsiōnem ordiatur aliquam, vel omnem simili vi, vel causa, qua ad tempus aliquod (quamquam ante plures annos vidi ego tenuem ex cera signatoria bracteolam tubo metallico ad plures menses adhæsisse elec-

\* Proposita experimentorum tabula eorum summam exhibet, singularia singularis cujusque experimenti adjuncti pertractationem postulant.

tricitatis vi) cohærere videmus corpora, quæ mutuo fricavimus; 3. ut pergam explorare, quanta parte illi etiam naturæ motus, quos chemicos dicimus, quando arte imitatur, igne electrico efficiantur, &c.

THEOREMA.

Chordæ semiangulorum, quibus divergunt duo fila, adeoque & vires, quibus ea fila divergunt, sequuntur directam simplicem proportionem densitatis ignis electrici redundantis in iis filis, aut raritatis ignis electrici deficientis.

EXPERIMENTI APPARATUS.

Fig. 2. I. In medio amplo experimentalis theatri funiculis suspendo, & distraho, ne commoveri possit, tubum ex lamina ferrea FE longum pedes quatuor, latum pollices tres, pendentem libras tres.

II. Imo ipsius tubo puncto P, in extrema parte,necto pendulum PL. Imo alteri puncto, quod est in medio tubo,necto duo tenuissima argentea fila Bb, Aa emollita ad ignem, & probe tensa, ut pendeant recta, proxima, & parallela. Quo facile discedant hæc fila, in BA nectuntur alterum alteri perbrevis serico stamine; quo cerni eminus possint, singulis b & a necto levissima duo chartæ frustula, prospectanda contra pannum nigrum adverso muro adfixum.

III. In abaco eminus posito solidissimo, quo prorsus non trepidet, regula lignea lineam definitio LM parallelam plano bB, Aa, in quo novi insistere fila, cum divergunt. Porro cum fila hæc sunt recte disposita, &

late disjuncta a corpore alio omni, divergunt in plano parallelo axi tubi FE.

IV. Tum regulæ adfixæ in LM adpono dioptras mobiles tres HIK. Eæ sunt acutissimæ, & rectæ tres acus infixæ cubis plumbeis.

V. Interea adfunt eminus duo homines A & B. Homo A separatus a solo, qui in tubum FE, attingendo auream bracteolam adfixam extremo ipsius G, (ne attræctatione tubi fila commoveantur) immittat electricitatem à catena, aut machina; alter homo B tenet extremum caput bacilli vitrei NO quo separatur tubus ferreus *fe* omnia similis, & æqualis tubo alteri FE instructus ipse etiam bracteola metallica g.

#### EXPERIMENTUM TERTIUM.

His ita comparatis, primo colloco dioptram H, ut radius visualis ex apice acus trajectus per filum verticale PL incurrat in duo stamina argentea Bb, Aa.

II. Tum homo A monitus eminus attingit bracteolam G; stamina divergunt ad angulum *b'B Aa'*. Vix ipsa acquiescunt a prima vibratione, studeo ego dioptram K locare ita, ut radius visualis *Kb'* incurrens in chartam *b'* fili *Bb'* jam divergentis, trajiciat per filum verticale penduli PL.

III. Tum homo B monitus bracteolam g admovet bracteolæ G; electricitas tubi FE effundit se ad æqualitatem in tubum *fe*. Angulus divergentiæ filorum *b'B, Aa'* minuitur. Sollicitus dioptram I loco, ubi radius visualis ex ipsa incurrens in *b''*, trajiciat ipse etiam per filum verticale PL.

Experimentum autem tamdiu instauro, quamdiu dioptras recte constitutas iterum atque iterum observo, atque tum pergo ad calculum.



CALCULUS THEOREMATIS DEMONSTRATIONEM  
SUPPEDITANS.

I. Metior distantias horizontales, HK, HI, HS, Sb. Tum HS inquam, ad HK, uti Sb ad quartum; atque quartus hic terminus est sinus femianguli  $b'Bb$ , est enim intervallum horizontale duorum planorum verticalium, quorum alterum tranfit per puncta H,  $b$  & per filum verticale PSL.

II. Et simili analogia; uti HS ad HI, ita  $Sb''$  ad quartum; quartus hic terminus est sinus anguli  $b''Bb$ .

III. Ex finibus pronæ sunt chordæ. Atque pluries repetitis eadem die, & diversis diebus experimentis, semper chordam  $b''b$  inveni quam proxime subduplam chordæ  $b'b$ , ut differentia fit in perpauca lineæ centesimis, & differentiis æquatis experimentorum omnium, plane evanescat.

THEOREMATIS DEMONSTRATIO.

IV. Ex mechanicis, vis suspendens grave in arcu circuli est uti chorda arcus. Itaque vires electricæ, quæ filum quodque divergens a filo socio suspendunt in arcu circuli, sunt ut chordæ angulorum, queis ipsum filum divergit a recta verticali. Quare quoniam ex experimento, chorda anguli  $b'Bb$ , existentis ex igne electrico toto, est dupla chordæ anguli  $b''Bb$  existentis, ex igne electrico subduplo, efficitur has chordas, adeoque & vires, quibus fila divergunt, sequi rationem simplicem, atque directam densitatis ignis electrici redundantis.

Pro filiis, in quibus ignis deficit, eadem est ex experimento demonstratio. Alias demonstrabo, similem

sem existere etiam causam, scilicet experimenta ostendunt: quemadmodum ignis electricus redundans in filis expandit se circa fila, quin ex ipsis ineat in aërem habentem solum ignem nativum; ita ignem nativum aëris ambientis fila, in queis ignis nativus deficit, expandere se promptissime circa fila, quin ex aëre discedat.

#### EXPERIMENTUM QUARTUM.

Fig. 3. I. Abacum rectangularem ita loco, ut facie sua plana in plano sit horizontali ST.

II. Faciei ipsius adglutino laminam ex plumbo, quæ distet undique a margine pollices tres; unus solum ejus laminæ limbus extat ex margine abaci; appello limbum communicationis.

III. Quatuor lateribus tabulæ necto regulas ligneas quatuor lineam unam prominentes supra tabulæ faciem. Ita capacitas existit parallelepipedæ, habens pro basi amplitudinem abaci, & undique lineam unam alta.

IV. In Aheno, liquo ad ignem, colofonium defæcatissimum, admiscens pondus æquale pulveris ex marmore cribrati ad summam subtilitatem, diu diligenterque calefacti, ne humor ullus reliquus insit, atque, ut omnis, qui potest, dispellatur aër.

V. Liquatam massam capacitati abaci infundo, atque ubi est necesse, cilindro æquare pergo; si alicubi fatiscat, consolidare satago ferro candente.

VI. Adglutino summæ, & mediæ hujus strati faciei laminam plumbeam distantem undique similiter pollices tres a margine abaci.

Atque ita paratum habeo abacum vere fulminantem, qui fulminantibus vitris præstat usus commoditate,

effectuumque magnitudine; etenim tempestate, etiam non siccissima, quatit validissime, quod resinæ humorem respuant, quem attrahit vitrum. Præterea parari potest amplitudine quantalibet ad effectus quantoslibet.

Abacus meus fulminans indusium habet metallicum longum pollices triginta, latum pollices quatuordecim, & aërem clausum in breviori barometri crure BI scintilla sua tanta vi disjicit, ut totam mercurii columnam deprimat in eo crure ad sesquilineam, elevetque adeo ad altitudinem æqualem, in crure longiore, columnam mercurii totam \* BK.

Experimentum III. usu venire potest in æstivandis ignis electrici pressionibus, seu viribus, uti appellant, mortuis; hoc, aut similia vires vivas possunt exponere.

#### EXPERIMENTUM QUINTUM.

Fig. 4. In globo vitreo *a b c*, qui rotatur in machina electrica, & fricatur pulvillo P, duplex communiter observatur lux ignis electrici: altera in *a*, ubi globus proxime discedit a pulvillo, altera in *c*, ubi globus proxime redit ad pulvillum; illam adeo appello lucem discessionis; hanc lucem reditus. \*

Hactenus cum Franklino opinabar, lucem discessionis existere ex igne electrico, qui a pulvillo trajiceret in vitrum, atque in ipso cumularetur, alteram ex eo igne cumulato, qui, cum globus proxime redit ad pulvillum, in hunc reflueret parte sua aliqua.

\* Quoties experimentum hoc obvenit in facie mercurii, qui aëri subest vibrato, electrica lux micat. Nonne ea vibratione aliquis ab aëre electricus ignis in mercurium vibratur?

Secundam hujus opinionis partem video constare verissimam; si enim ignem frictione congestum in globum intercipiam quoquomodo, vitro, quod jam discessit a pulvillo, objiciendo ubilibet corpus deferens, pro eo igne sublato vel omni, vel aliquo, vel continenter, vel interrupte, omnis, vel parte aliqua, continenter, vel interrupte deficit lux redivit.

At in luce discessionis explicanda erravimus; non est enim ab igne electrico, qui a pulvillo ineat in vitrum, verum ipsa etiam æque, ac lux redivit, efficitur, parte ignis in vitrum frictione congesti in proximum pulvillum refluenta.

Fig. 5. Etenim dum ceram signatoriam frico charta inaurata, existunt luces geminæ, B, A, tum quæ discessionis dicitur, tum quæ lux redivit appellatur, simillimæ ambæ inter se, simillimæ iis, quæ apparent in vitro. Fig. 6. Sed, cum frico charta nuda, geminæ luces iterum sunt Aa, Bb, similes inter se, sed iis prorsus dissimiles, quæ adparent in vitro, aut in cera signatoria perfricata a charta inaurata; illæ enim micant ad perbreve intervallum inter ceram signatoriam, & marginem chartæ inauratæ, atque juxta hunc marginem æmulantur seriem stellarum, istæ a margine chartæ nudæ, instar penicillorum, longe perfectiuntur faciem ceræ signatorię fugientem, aut redeuntem.

Nequit autem lux discessionis similis esse luci redivit in utroque experimento, & nequeunt geminæ in experimento uno esse dissimiles geminis in experimento altero, quin, quæ similes sunt in experimento uno, causa efficiantur simili, & quæ dissimiles sunt in experimentis duobus, causa efficiantur diversa. Sed lux redivit, cum fricatur globus vitreus, aut cum charta  
inaurata.

inaurata fricatur globus ex cera Hispanica, efficitur igne congesto refluxente in proximum marginem corporis fricantis; ergo & focia discessionis lux ignis ejusdem parte aliqua efficitur ex vitro, aut cera Hispanica in fricans corpus refluxentis.

Iterum lux reditus, cum charta nuda fricatur globus ex cera signatoria, efficitur igne, qui a corpore fricante init in ceram orbatam igne suo; igitur & similis discessionis lux igne efficitur in ceram signatoriam refluxente.

Forma earum lucum rem confirmat jam plane manifestam. Quæ nempe luces geminæ in margine chartæ sunt, ceu series stellarum, eas decet igne effici in proximum marginem ineunte, quæ ex eodem margine erumpunt simillimæ penicillis, eas decet effici igne prodeunte.

P. S. Addo experimentum elegans, uti videtur, commodum, neque infacundum. Zonam ex panno serico villosa nigroque advolvo cilindro vitreo, qui fricatur in machina electrica, distendo, adfuo, pilis frico, qui extant ex pelle leporis; frictione non ita valida, electricitas existit in catena vehementior, quam quæ existit ex frictione vitri, sed huic contraria. Frictio modica commoditatem facit in experiendo, vehementior electricitatis, & experiendi commoditatem auget, ut & experimentorum effecta. Electricitatis in vehementia contrarietas quæstionem finit. Uti enim video, qui electricitatem resinæ negabant contrariam electricitati vitri, abutebantur ejus electricitatis debilitate.

XVI. *Proposal of a Method for measuring Degrees of Longitude upon Parallels of the Æquator, by J. Michell, B. D. F. R. S.*

Read May 8, 1766. **T**HERE have been already several attempts made towards discovering the figure of the earth, by measuring the length of a degree of the meridian in different latitudes: now if these measures had been sufficiently accurate and numerous, and we could also depend upon the uniformity of the earth's surface, we might then immediately discover from them the form sought; but these measures, not agreeing exactly to any certain rule, leave us still in some degree at a loss. It is therefore much to be wished, that more measurements of degrees upon the meridian were to be made, in order to determine with greater accuracy a question of this importance.

But what would tend yet more to determine this matter, would be the measurement of degrees of longitude, as well as those of latitude. Astronomers have indeed expressed their wishes that this might be done; and though no attempt has been hitherto made towards it, yet as it is probable, that such measurements may some time or other take place, it will not be amiss to suggest a method, which will admit of more exactness than any I have seen proposed for this purpose, all of which, depending upon an observation of the time, are therefore liable to an error of fifteen seconds of a degree for every second of time; but the method, I mean to recommend, stands upon the same foundation with the  
measure-

measurement of a degree of the meridian, and, the instruments being equally good, and the number of miles to be measured the same, the exactness of it, to that of a degree of the meridian, will be in the proportion of the sine of the latitude to the radius very nearly.

In TAB. V. Fig. 7. let AB represent the æquator; P the pole; DLEF a parallel of the æquator; PEC a meridian passing through the station E; PLMN a meridian passing through another station M; and let AMEB be a great circle cutting the meridian PEC at right angles in the point E.

Then in the spherical triangle AMN, right angled at N, we shall have  $R : \text{Cof. AM} [\text{Sin. ME}] :: \text{Tan. MAN} : \text{Co-Tan. AMN}$ : hence  $\frac{\text{Tan. MAN}}{R} \times \text{Sin. ME} = \text{Co-Tan. AMN}$ ; but Tan. MAN, being the Tangent of the latitude of the given place E, and therefore given, the quantity  $\frac{\text{Tan. MAN}}{R}$  will likewise be given, and greater or less than unity in the proportion of the Tan. of the latitude to the R. The Co-Tan. therefore of the angle AMN, that is the Tan. of the complement of the angle AMN to  $90^\circ$ . will be greater or less than the Sine of the Arc ME, in the proportion of the Tan. of the latitude of the place, to the R. And consequently, whilst the Arc ME is small (in which case the Sine, Arc, and Tangent differ very little from each other) the angular deviation of the intersection of the meridian PLMN with the great circle AMEB, from a right angle, will contain more or fewer degrees, &c. than the Arc ME nearly in the (same) proportion of the tan. of the latitude of the place to the R.

By

By this means then, the latitude of the place and the angle  $PME$  (contained between the meridian  $PMN$  and the great circle  $AMB$ ) being given, the length of the Arc  $ME$  will likewise be given, with great exactness. But I must observe, that as the angles  $PEM$  and  $PME$  must be taken by the observation of some star near the pole, they will be less accurate, when reduced to the plain of the horizon, than at the pole, in the proportion of the Sine of the distance between the pole and zenith, that is the  $\text{Cos.}$  of the latitude, to the  $R$ , which with the proportion just mentioned of the  $\text{Tan.}$  of the latitude to the  $R$ , makes the accuracy of this method upon the whole, when compared with that of the measurement of a degree of the meridian, in the proportion of the  $\text{Tan.}$  multiplied into the  $\text{Cos.}$  of the latitude, to the square of the  $R$ . very nearly; but the  $\text{Tan.}$  of any angle into its  $\text{Cos.}$  is equal to the  $\text{Sin.}$  into the  $R$ . whence this proportion is the same as the  $\text{Sin.}$  into the  $R$ . to the square of the  $R$ . and dividing both by the  $R$ . simply as the  $\text{Sin.}$  of the latitude, to the  $R$ . as above.

Having got the length of the arc  $ME$ , of a great circle, in degrees, &c. together with the distance of the two stations  $M$  and  $E$ , it is easy to conclude from thence the length of a degree of the parallel of latitude, at the place of observation, which will be the same, without sensible error, as it would be, supposing the earth was an exact sphere, to the same scale, with the degree of a great circle just found.

For in Fig. 8. let  $APB$  represent a section of the earth through its axis  $PCH$ ;  $ACB$  an æquatorial diameter;  $AD$  the radius of curvature at the point  $A$ ; and  $PH$  the radius of curvature at the



point P; DFH the evolute of the curve AEP; EF the radius of curvature at the point E; (which we will suppose to have the same latitude with the point E in Fig. 7.) and let EF be produced till it cuts the axis PH in G: then with the radius EG and centre G, describe the arc IEK, which will be the least circle, that can touch the curve AEP at the point E, without cutting it. Let now the curve PEA, the line EG, and the arc IEK revolve about PH as an axis, and, PE being equivalent to PE in the former figure, the point E in the latter figure will describe the parallel DEF in the former; AEP at the same time describing the surface of the earth, and IK describing a portion of a sphere, which will be every where a tangent to the parallel DEF, and whose centre will be G. The curvature therefore of this sphere will be less than the curvature of the earth, in the direction of the meridian, at the point E, as the radius GE is greater than the radius FE; but this, in moderate distances, can cause no sensible error. The difference between AD the radius of curvature, at the point A, on the earth's surface, and the line AC, according to that hypothesis, which makes it the greatest, does not exceed one fiftieth part of the whole, and upon the same hypothesis, the part FG of the line EG, supposing E to be in the latitude of  $45^\circ$ , would not exceed  $\frac{1}{130}$  part of the whole. If then we take any other point upon the surface of the earth as M, at a small distance from E, the distance between that point and the sphere described by the arc IK, will be only  $\frac{1}{130}$  part of the versed sine of the arc EM, and the perpendicular standing upon the surface of the earth at M, will be inclined to the perpendicular standing

standing upon the sphere in an angle, which is equal to  $\frac{1}{130}$  part of the angle subtended by the arc EM. And in higher latitudes these quantities will be still less. Let us now return to Fig. VII. and supposing the point E to be situated in latitude  $45^\circ$ , let the arc EM cutting PE at right angles, consist of  $2^\circ$  (near 140 statute miles) then will the side PM, of the triangle PME, consist of  $45^\circ 2' 5''\frac{1}{2}$ , and consequently, if LM in Fig. 7. be supposed to correspond to EM in Fig. 8. the distance of these two points E and M, in the latter Fig. will be only  $2' 5''\frac{1}{2}$ , the  $\frac{1}{130}$  part of the versed sine of which is a little more than  $\frac{1}{3}$  of an inch, to the radius of the earth, which will therefore be the distance of the point M upon the earth's surface, and the point of the imaginary sphere, described by IK, immediately over it. Hence also, the inclination of the real perpendicular at M, and the imaginary one standing upon the arc IK, at the same place, to each other, will be something less than a second, a quantity in itself almost too small to be regarded, unless the instruments made use of are both very large and very excellent in their kinds, and which, being wholly in the plane of the meridian, will produce an error, that must be perfectly insensible, with any instruments whatsoever, in an observation of the angle PME, Fig. 7. which will therefore, to all intents and purposes, be the same, as if the curvature of the earth in the direction of the meridian, and in the direction of ME or LE were accurately the same.

I have supposed the arc ME, to stand at right angles to the meridian PE, which passes through one of the extrem stations; the method here proposed is,

however, liable to the least error, when the meridian cuts the arc to be measured at right angles in the middle of it; but this makes so very small a difference, that it is not worth regarding; nor is it indeed necessary, that the arc should not deviate two or three degrees from right angles with the meridian, at that end where it cuts it most nearly at right angles, in case the situation and circumstances of the country should make this more convenient, the errors, that would be occasioned by such a deviation, being too small to affect the conclusion. And if this deviation was still much greater, and the length of a degree of the meridian at the same place was known, it would be very easy to make the necessary corrections.

It will perhaps be objected, that the method above proposed depends, in some measure, upon time, as well as others, the finding of the meridian not being to be performed without it; but I must observe, that the motion of the pole star, by which I propose to find the meridian, being slower than that of a star at the æquator, nearly in the proportion of 30 to 1, this method will admit of an exactness greater in the same proportion (except the reduction of the Sin. to the R. before mentioned) than those observations, by which we endeavour to find the difference of the longitude of two places, by the difference of the time of the sun, or a star's coming to their respective meridians.

The method above proposed will likewise require different instruments from those commonly in use; but admitting, that instruments of equal radius are capable of equal exactness, this method will admit of the same exactness with the observations of a degree of the meridian, except the before-mentioned limita-

tion. Nor would the instruments for this purpose, if well contrived, be either less portable, or more expensive, than those for measuring a degree on the meridian; the same telescope which would be necessary for finding the meridian, would serve likewise for tracing the arc of a great circle; but it is beside my present purpose to consider any farther what kind of instruments would be most proper upon this occasion, or what would be the best manner of constructing them.

I must not, however, dismiss the present subject, without observing, that, by means of the above-mentioned method, a country not too near the æquator, nor attended with any other unfavourable circumstances, might be laid down with wonderful exactness. By running a great circle nearly East and West through the midst of it, we should get the longitude of all the places, the great circle passed over; and if, by means of the meridian telescope, we should trace meridians through a few of these places, as far North and South, as the survey was intended to be carried, we should then have a number of stations, in several parts of the country, whose longitudes, with respect to one another, would be very accurately determined, and to which other places might easily be referred, when the length of a degree of longitude in those situations was known.

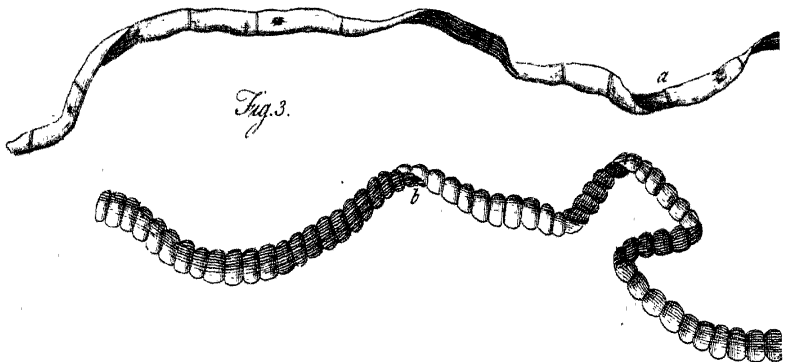
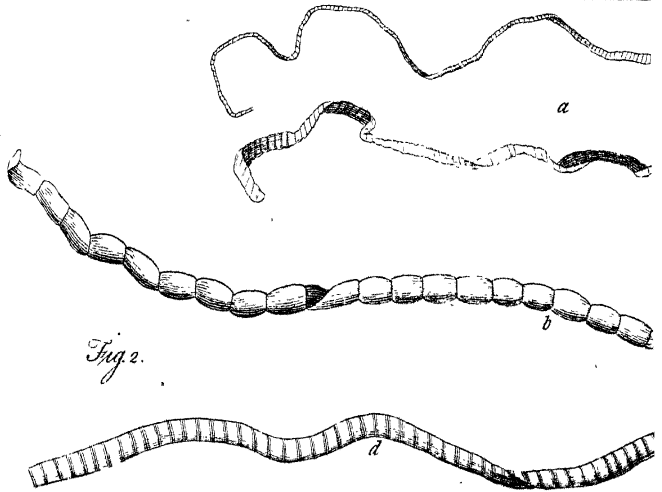
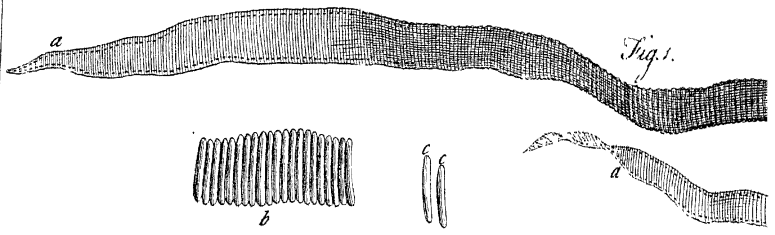
XVII. *Observationes de Ascaridibus & Cucurbitinis, & potissimum de Taenia, tam humana quam leporina.*

Read May 15, 1766. **S**I omnes pervolvantur auctores, qui de vermibus corporis humani tractaverunt; vix de plerisque, scilicet de taenia, ascaridibus & cucurbitinis, aliquid certe inde intelligitur. Non enim concipitur an taenia ex ascaridibus, juxta Couletum, vel ex cucurbitinis, juxta plerosque, agglutinatis & concatenatis constet; num potius sit vermis suae & singularis speciei, isque vel solitarius, vel multiplex; tuncque an cucurbitini non sint totidem taeniae annuli singulares, ab ea separati, vel alia vermiculorum species; praeter quas, aliae restant difficultates non leviores, unde, exempli gratia, originem ducant, imo quinam sit singulorum character naturalis, aut figura externa, quod postremum dubium vel a descriptionibus, figurisve erroneis, vel a varietate naturali ipsorum vermium pendet.

Quae, ut dilucidentur, missis fabulis, & opinionum commentis, necesse est horum animalculorum descriptionem unice a natura depromere, & super iis observata multiplicare; unde tandem eorum doctrina nitebit.

Cum mihi, quod non multis contigit, omnes vermium corporis humani species, vel saltem taeniarum frustula videre licuerit, & praesertim cum e leporis intestinis taeniarum binarum extremitates obtinuerim, inde occasione fruor singulari nonnulla de hisce vermium speciebus exhibendi, quæ ad taeniae historiam naturalem





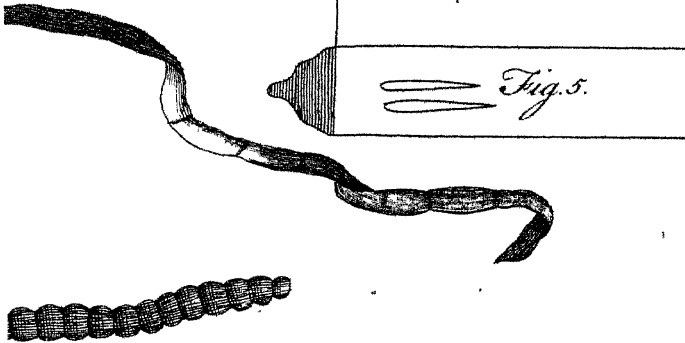
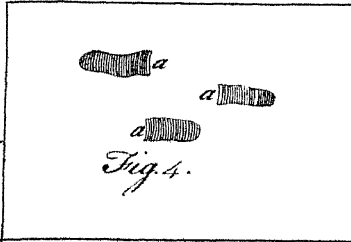
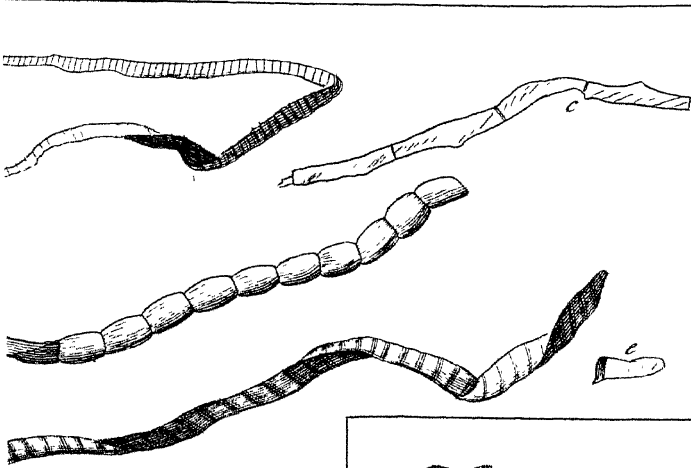
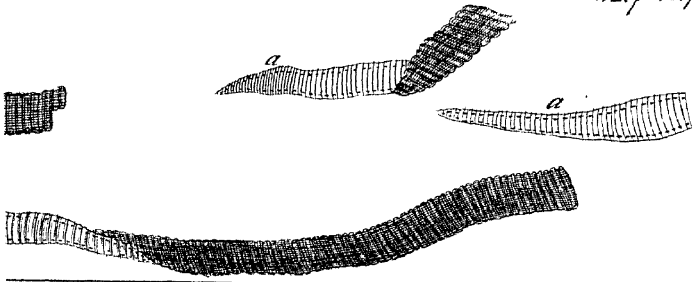




Fig. 2. exhibet frustula taeniae, quae, assumpto, vesperi, mercurii dulcis cum saccharo triti scrupulo uno, expulit postridie pulvis purgans, mulieri nostrati exhibitus. Superior portio *a*, extremum lineare exhibet. Mediae portionis *b* segmenta triplo, alterius *c* circiter quadruplo longiora sunt quam inferiores *d*, ita ut haec videatur uni extremitatum propinquior.

In annulo, seu segmento separato *e*, species sulci seu hiatus longitudinalis ab uno extremo est, qua parte alteri jungebatur.

Fig. 3. duo alia repraesentat, alterius taeniae, a praecedenti non multum diversae, frustula; quorum superioris *a* segmenta sunt tenuiora, laeviora, fere quadruplo longiora quam inferioris *b*, cujus annuli magis longi quam lati, asperiores, speciem verrucarum, aut mammillarum, alias in lateribus, alias in parte plana, exhibent.

Vasculum, specie fili caerulei, aut purpurei, quod in medio longitudinis ab uno ad aliud extremum per taeniam decurrere dicitur, non datur in hisce taeniis; sed apparentia velut vasculosa obscurior in quolibet annulo conspicitur.

Fig. 4. exhibet vermes cucurbitinos, ita ab Hippocrate, ob quamdam, ipsi visam, sed minime realem, cum seminis cucumerini aut cucurbitini similitudinem; horum extremum latus *a*, seu basis, speciem sulci seu hiatus habet.

Haec species magnam affinitatem habet cum segmentis taeniae separatis, ut videre est comparando in Fig. 2. litt. *e*.

Tales habeo quamplurimos, ut a juveni jamdudum excernuntur.

Fig. 5. ascarides repræsentat, quales vidi innumeros. Hic vermis, circiter dimidium pollicem longus, corpore donatus est terete & laevi, opaco, candido, in duo extrema desinente, quorum aliud tenuius quam medium corpus, obtusum, substantiæ candidæ, opacæ; aliud sensim magis acuminatum & tandem capillare, pellucidum, excolor.

His observatis, nonnullæ animadversiones sequentur.

Primo nil ascaridibus cum taenia aut cucurbitinis commune videtur; sed singularis vermium species censendi sunt.

Qui vero ascarides Couleto audiunt, ii sunt, aliorum cucurbitini; quorum excretio, ut taeniæ signum, ab omnibus habetur. Inde vel ex hisce coalitis formari taeniam, quæ communior sententiæ est, vel eos non nisi taeniæ segmenta separata esse, apparet.

Prior opinio, maxime speciosa, ipso figurarum intuitu evanescit. 1. Enim ipse ordo & diversitas annulorum, & præsertim exilitas annulorum extremitatis, *a* (Fig. 2.) formationem ex cucurbitinis concatenatis renuit; hos itaque potius non veros vermes, sed taeniæ annulos separatos arguit. 2. Comparatio taeniæ leporinae hoc valde confirmat. In ea omnes annuli sunt longitudinales; atque cum sulcus *a* cucurbitinorum pro eorum ore accipiatur, repugnat segmenta Taeniæ leporinae fuisse vermes, cucurbitinis nostris affines, quorum os tota longitudine decurreret. 3. Puncta illa, in segmentis extremitatum taeniæ leporinae, eorum mechanismum testantur diversum quam segmentorum intermediorum, in quibus talia puncta non cernuntur, ita ut non eadem sit eorum fabrica originalis, nec per

consequens communis origo a consimili verme.  
 4. Divisio annularis in aliis vermium speciebus visa est ; nec a tali concatenatione hos ortos suspicatum est, nec per consequens taeniam oriri suspiciendum ; actuum Edinburg. Vol. II. prostat exemplum vermis rotundi pariter annularis. Linnaei (in Faunâ Suec.) taenia articulata teres, aliud est similis constructionis exemplum. Haec, aliaque, non ab insueta naturae lege, qualis plurimum animalium redactio in unum esset, sed naturalem esse constructionem testantur.

De taeniae capite bipartiuntur auctores. Jam celebris Tulpus (1) reprobravit taeniae descriptoribus, quod eam semper mutilam & tantum capite tenus descriperint. Caput alii etiam agnoverunt, ut Andry (2), Raulin (3), Thomas (4); caput Andryanum, speciem porri aut verrucae; Tulpianum velut caput colubrinum; Raulinum specie rostri, cum duobus punctis nigris ad duo latera; Thomas, ut punctum nigrum, quale in quorundam fructuum infectis apparet.

Quoad Tulpium, notandum quod non propria autopfia, sed aliorum medicorum testimonio, & sculpsoris delineatione id admittat.

Tot diversitates in capite pingendo & describendo, an non ejus realitatem minus certam faciunt? an non praeoccupatio potuit dilaceratae forsan extremitatis appendices, specie rostri aut capitis, figurare? maxime quum Raulin & Thomas, nonnisi microscopio adjuti,

(1) Obs. Med. lib. ii. cap. 42.

(2) Gen. des vers.

(3) Maladies par les variations de l'air.

(4) Journal de Medecine, Juillet, 1765.

caput depinxerint, quom Tulpii, & Andry, summe diversa capita videantur oculis nudis conspecta fuisse.

Alii observatores, inter quos ego, nil capiti simile vidimus; truncatum forte potuit esse ab extremo, quale in Fig. 2. Fateor tamen leporis taeniarum extremitatibus quatuor deficiens caput mihi persuadere has taenias dicto organo destitutas fuisse visibili, quom videatur iis extremis nihil truncati fuisse. Revera hae taeniae dilaceratae erant in partibus intermediis, quod domestici incautela & initio corruptionis viscerum contingere potuit, quom is lepus jam a decem diebus affervatus fuerit. Sed extremitates visae sunt sanae & integrae, nec capitis, vel etiam separati, vestigium reperire potuimus. Accedit quod Linnaeus, de taenia intestinorum agens (1), referat se taeniam invenisse in ochra acidulari Jaernensi; unde concludit, ex ovis vermium, una cum aqua haustis, tales & alios vermes in corpus pervenire; nec de capite mentionem facit hic egregius historiae naturalis scrutator. Haec rem indecisam faciunt, novaque requirunt exactiora observata.

Alia quaestio est num solitaria sit taenia, vel multiplex? Quatuor extremitates (Fig. 1.) arguunt duas saltem fuisse in lepore. Duae pariter repertae sunt in fele, altera in stomacho, altera in duodeno (2); Tulpius (l. c.) ter integram a muliere quadam excretam asserit.

Alia discussio est num multiplices sint taeniarum species? alia annulis longis, ut depinxit Coulet; alia brevioribus, ut Tulpius; alia articulis

(1) Syst. Nat. Obs. in R. Anim.

(2) Mem. de l'Acad. R. des Sciences de Paris, Vol. 17. pag. 484. Edit. d'Amsterdam.

versus extrema exilibus, in medio longioribus, quales taenia Hippocratis & Andry; quales videntur fuisse taeniae hic depictae, tam humanae quam leporinae. An forte duae priores species stabilitae sunt, secundum frustula, quae prodiderunt, extrema aliis, aliis media? quod probabile videtur. An non etiam aliae aliis latiores, quod a diversitate pabuli nutritii pendere posset? & hoc verisimile videtur. Interim leporinae & humanae taeniae diversitas species diversas ostendit.

Unde tandem originem ducunt taeniae? Certe, ut jam ex Linnaeo deduxi, tam taeniae, quam alii vermes intestinales, videntur ex similibus vermium ovis, cum aqua aut edulibus deglutitis in intestina pervenire, ubi proprium alimentum nacti crescunt & videntur, probabiliterque speciem propagant. Quae ovulorum taeniae deglutitio haud difficilis videtur, considerando taeniam ex praedictis auctoribus reperiendam esse in ovibus, canibus, tinca pisce, fele, lepore, atque in ipsa ochra; ex quibus facilis communicatio, seu per aquam, seu per alimenta, quibus deposita sint ova, deducitur.

Ut veri limites non excedam, his superfluo congruum duxi, donec plura horum vermium, praesertim integrorum, observata omnibus difficultatibus satis faciant.

Haec interim, quantumvis mediocria, sperans aliquantum lucis huic materiae afferre posse, perillustri Regiae Scientiarum Societati offert submittitque,

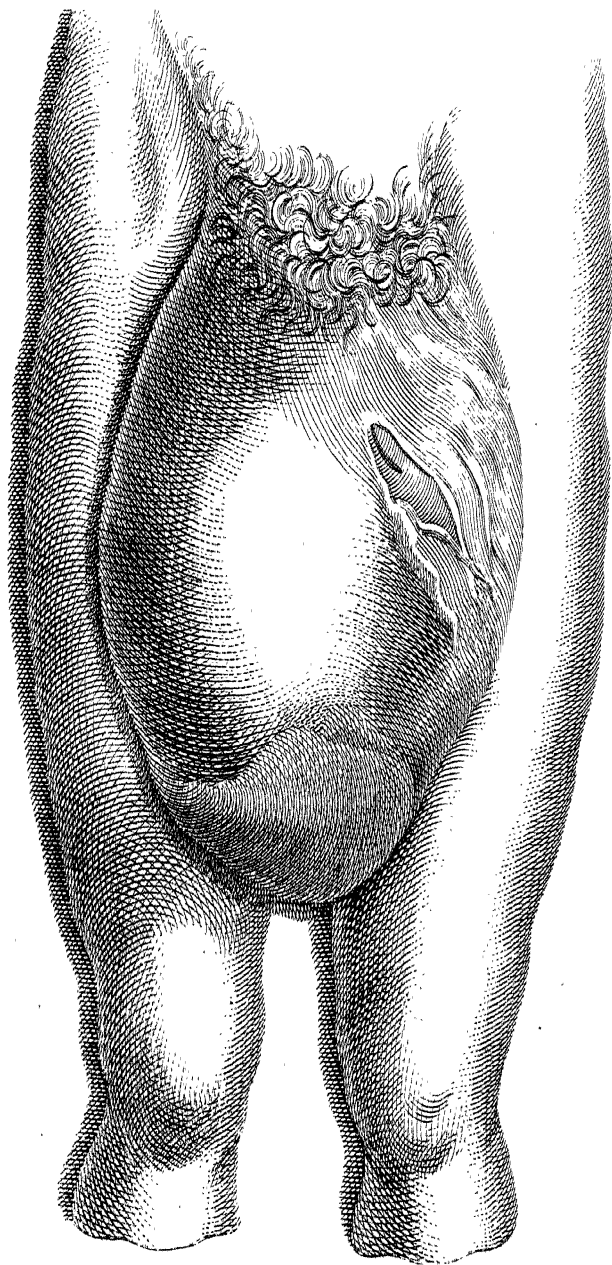
Joannes Philippus de Limbourg, M. D.

Et Regiae Scientiarum Societatis

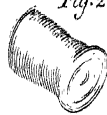
Monspeliensis Correspondens.



*Fig. 1.*



*Fig. 2.*









Received April 24, 1766.

XVIII. *An Account of an uncommon large Hernia, in a Letter from Dr. George Carlisle, to the Right Rev. the Lord Bishop of Carlisle, F. R. S.*

Carlisle, April 18, 1766.

My Lord,

Read May 14,  
1766. **W**HEN I shewed you the drawing of an uncommon large hernia at Rose, you were pleased to say, you should be glad to have the history of it, and of what occurred in examining the body after death, in order to communicate it to the Royal Society: from that time I determined to draw out the case, but have been prevented by various other engagements, till now, that I take the liberty to present it to your Lordship; and shall be extremely rejoiced if it prove agreeable to you, and the learned body; with it I inclose an outline of the drawing, TAB. VII. Fig. 1. and an explanation, which may make the description more intelligible. I was sorry, that for want of a proper draftsman, my good friend the captain being out of town, I could not have the situation of the stomach, with the other parts left in the abdomen, taken; but my painter was so squeamish, it was with difficulty we got the outward appearance taken from the dead body.

We

We have had the driest season here I have known; thro' the whole of last year, only 19.705 inches of rain fell; and in the three last months of this, only 1.689 inches, including what melted from the great fall of snow on the 11th and 12th of February, into my receiver: the snow lay then 9 inches perpendicular, upon a level bed in my garden. I am, with great respect,

My Lord,

Your Lordship's much obliged,

and most obedient Servant,

George Carlisle.

**J**OHAN HALLOWDAY, an out-pensioner of Chelsea, aged near 80, having entered very young into the army, and undergone several hardships in the campaigns under the Duke of Marlborough, upon his return to England from Flanders, at the conclusion of the war, first perceived a small tumor in the right side of the scrotum, and groin. This he carefully concealed, to avoid the scoffs of his companions, and lest it might be the occasion of his discharge, which he dreaded, and wanted to avoid; as he found no other inconvenience from it, but what its bulk occasioned, nor ever had pain, vomiting, obstructions to stools, or any other symptoms of a strangulated hernia. From that time, however, it continued to increase in bulk; and from that, and its weight, grew daily more inconvenient to him, insomuch, that about the year 1725, being

unable to go through the duty of a soldier, he was admitted to the out-pension of Chelsea hospital. Its size was then such, that he was obliged to have a particular bag made in the forepart of his breeches, to enable him to carry about its weight, and always wore a leathern apron to conceal its figure. For six or seven years before his death, the weight and bulk of the hernia had made such an alteration, in the outward appearance of the parts about the scrotum, that the penis was entirely buried in the tumor; a small oval opening only was left, out of which the urine was discharged: this opening was sometimes excoriated, from the acrimony of the urine, as the penis could not be extracted to throw it off, nor the glans be made to appear by any endeavours: after death, it could be protruded no farther outwards, than as it is shewn in Fig. 2. A year or two before his death, after a cold, and fretting the part by too much walking, the urine had brought on a considerable inflammation, which mortified to a large extent, one considerable eschar, formed upon the anterior and most depending part of the bag, one less on the right side where it touched the thigh, and a third behind; yet all cast off and healed kindly, by the help of the bark, warm dressings, &c. Except from this accident, in the latter years of his life, he was not subject to any other complaints than are common at his years; such as dimness of sight, catarrhus coughs, shortness of breath upon motion, swellings of his legs occasionally: and he wore off at last by a gentle decay, having all along had as good an appetite, and digestion, as could be expected at his time of life; regular discharges, both by stool and urine; very rarely vomitings, except from overloading.

loading his stomach; purgatives, and every other medicine, operated as regularly upon him, as upon any other person. He was a well-made man, rather above the middle size; was as corpulent, and had as much strength, as most of his years, until within a very little time of his death.

His case having nothing particular in it, but as far as the contents of the abdomen and scrotum were concerned; it was not thought necessary to carry the examination of his body farther than through them.

The large hernial bag I had measured, as exactly as I could, about a year before his death; and found its length, from the os pubis to the most depending point, 15 inches; its greatest breadth, while it lay supported by the thighs,  $17\frac{1}{2}$  inches; and its greatest circumference 34 inches: but in the body, the day after death, its length, from the pubis to the most depending part, was only 13 inches; its breadth, to the part where it fell in between the thighs, 12 inches; its circumference round the thick, or smallest part, where it descended from the pubis, 19 inches; and round its large circumference, 27 inches. It was covered with the common integuments of the scrotum; but at its lower and posterior parts, the cellular membrane, or dartos, was reduced to an almost cartilaginous hardness, where the weight, both in sitting, standing, and lying, had the greatest effect: the cicatrices also, where gangrenous sloughs had been cast off, were of an equal firmness, and hardness under the knife. The testicle of the left side was plainly to be felt, at the prominent part, above, and to one side of the opening for the penis, not far from its natural situation: the right testicle was obscurely to be felt, a little above the  
lowest

lowest and anterior point of the bag. Besides what appeared upon the front view of the bag, a large portion of it, like a ridge, extended backwards, where the space betwixt the thighs allowed it more room; they being rather more concave than usual, inwardly, towards each other; and more distant, from the constant pressure they sustained. The colour of the bag was the same as that of the other parts, except where the mortified sloughs had been cast off; where it was of a shining white. Upon opening the abdomen, the liver appeared rather large, and farther extended over the left side than usual. The gall-bladder was small, with a little diluted bile in it. None of the intestines appeared, but a portion of the colon, towards the anterior edge of the pelvis, on the left side; where it made two inflections, much in the way as the lowest turns of the intestines are shewn to do, below the omentum, in Eustach. TAB. IX, from these it went downwards, and backwards, into the pelvis, to make its last curve, and be continued into the rectum: which, with that last curve of the colon, was in its natural place and direction. The stomach, instead of an horizontal, had a longitudinal position; its large, and here upper extremity, being placed behind the left lobe of the liver, close to the diaphragm, and its large convex side lay along the left side of the abdomen; it descended to nigh the crest of the os ilium, from whence it turned over the inflection of the colon, before-mentioned, across the pelvis, to the large hernial aperture, in the right side; within the verge of which, it ran downwards about an inch, then ascended, and made a semicircular turn to the pylorus, which mounted towards the abdomen; from

thence the beginning of the duodenum made another turn, to descend into the hernial bag; immediately below which, viz. just within the opening of the hernial bag, the ductus communis choledochus entered it; and seemed the cause which kept it from falling further into the sac. From this, the remainder of the duodenum, and all the other intestines, were entirely contained in the hernial bag, to nigh the extremity of the colon, before-mentioned. The duodenum, after entering the sac, first ran a little downwards, and backwards, then horizontally, and lastly upwards, to within the edge of the sac, towards the abdomen; from thence the tegumen proceeded backwards and downwards, and then formed, with the ileum, pretty nigh their usual convolutions, about the middle of the tumor, as they should have done in the abdomen. The caecum had a very small appendix, but was itself very large; as was the colon through its whole length, while contained in the sac; that part of it, which returned into the pelvis again, being much smaller, even only of the dimensions of the smaller intestines: the length of the colon too seemed more than usual. The caecum began in the lower part of the bag, and from thence the colon kept pretty nigh the course it should have kept, if the bag had been the abdomen, for a great part of its length; running up, from the caecum, along the right side of the bag, to nigh the pubis, and then crossing over towards the left side, before the duodenum, to the left edge of the hernial aperture; at which place, slipping behind the lower extremity of the stomach, it appeared in the pelvis, crossing over to its left side; from thence to follow the course before described. The pancreas lay in a longitu-

longitudinal direction, along the concave arch of the stomach, through its whole course; and was placed before the bodies of the vertebrae. The ductuscholedochus, besides its great length from the liver, to within the hernial bag, was of such a width, as easily to admit a middle-sized finger, being about  $2\frac{1}{2}$  inches in circumference: in some parts of its course, it was little inferior in width to the gall bladder, in this same subject. The kidneys were rather small, in general found, except that some few hydatides were here and there fixed upon their outward surface, and that two or three steatomatous tumors, of about the size of a pea, and white, were in the substance of each; but not rising above their surface: they were each in their proper situation; the left lay behind the stomach, and was less, probably from the pressure it was exposed to. The ureters and bladder were in their usual situation; the bladder was no way engaged in the hernia, and a catheter was pretty easily introduced, through the concealed penis, into it. The spleen was small in its natural situation, and found. The mesenteric glands were numerous, large, hardened, and surrounded with a fat of a deep yellow, as was the pancreas; no omentum appeared; its place seemed supplied by the fat, interspersed among the glands, and pancreas. The testes were of a natural size, but loose and flabby, and had many varicose veins upon their surface: the right, which was so much out of its proper place, was the least, and laxer of the two: the spermatic vessels belonging to it were large, through the great length they ran. The sac and intestines were adherent, almost at every point of their contact; in some places so firmly, that they were with great difficulty

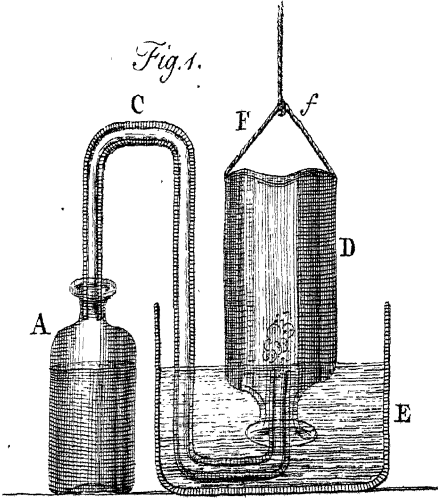


difficulty separated, and often not without danger of tearing: the intestines also adhered in the same manner, to one another; all, by means of a firm cellular membrane. The containing bag was very firm, thick and strong, as observed before. Its aperture, at the right ring from the abdomen, was so wide, as readily to allow a middle-sized hand to pass through it, from the abdomen, for a small space, betwixt its anterior edge, and the convolutions of the lower extremity of the stomach, and the semi-circular turn it made to the pylorus, with the beginning of the duodenum from thence, and the other extremity of the duodenum, before the jejunum commenced, and that part of the colon which returned into the pelvis; all of which were lodged in the very aperture: so that the space left unoccupied by these parts could not be much less than 8 inches in circumference: notwithstanding which, very little of a watery fluid was found in the sac: indeed it would not have had a very easy admittance, from the many adhesions formed betwixt the sac and its contained parts, a little below the opening from the abdomen.

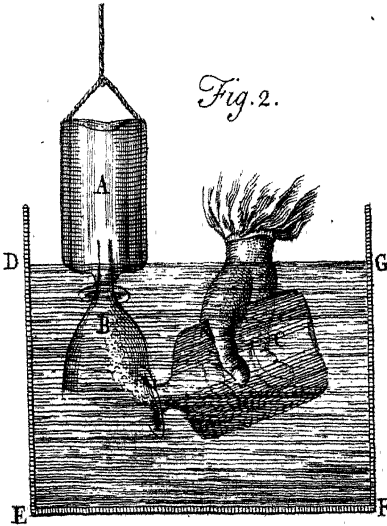
The above is a tedious perhaps, but a circumstantial, and just representation of this extraordinary case: which I shall not, at present, lengthen by deductions or reasonings; farther, than to admire the exquisite composition of that most admirably formed machine, which could bear so great an alteration in its parts, without a manifest impediment to its most material actions: seeing here, life, and even health, went happily on, through a great length of years, though the whole system almost of the intestines had been, for many of these years, without the reach of the  
 action



*Fig. 1.*



*Fig. 2.*



*Fig. 3.*

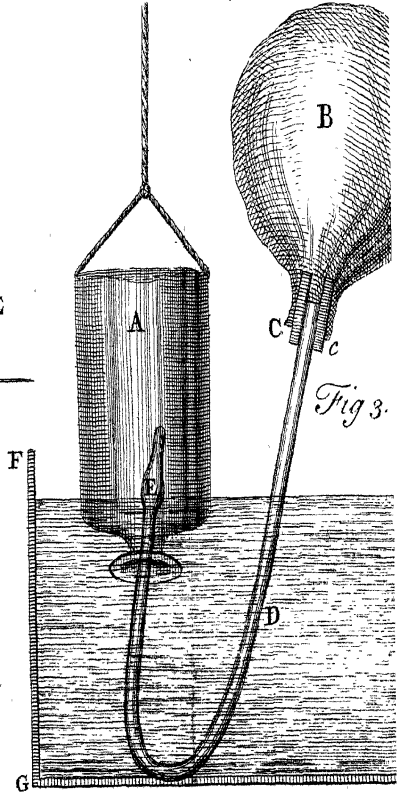
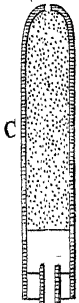
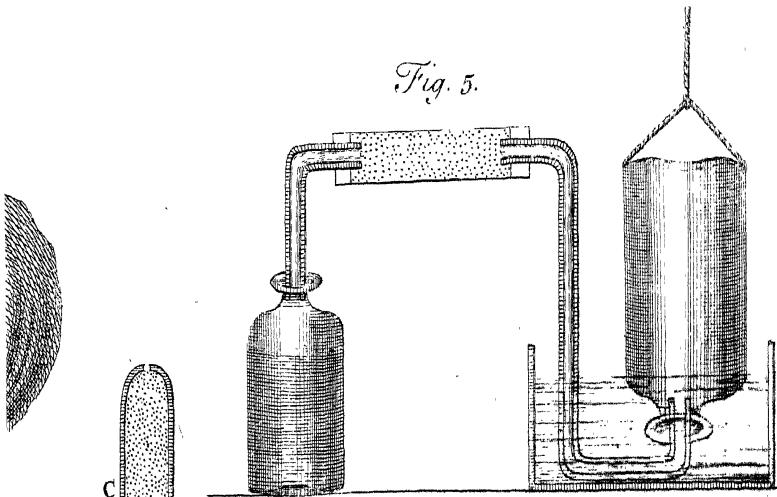


Fig. 5.



B

Fig. 4.

A

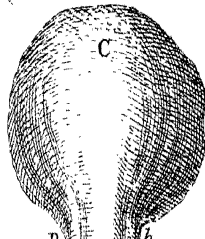
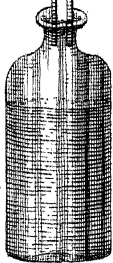


Fig. 6.

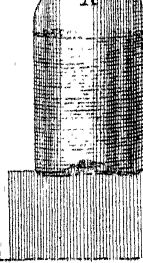
B

b

D

A

E





action of the diaphragm and abdominal muscles, and of the fatus of their generally neighbouring parts: requisites, as has been imagined, towards the carrying on their several functions, for the benefit of the animal oeconomy.

George Carlisle.

Received May 12, 1766.

XIX. *Three Papers, containing Experiments on factitious Air, by the Hon. Henry Cavendish, F. R. S.*

Read May 29, Nov. 6.  
and Nov. 13, 1766.

**B**Y factitious air, I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art.

By fixed air, I mean that particular species of factitious air, which is separated from alkaline substances by solution in acids or by calcination; and to which Dr. Black has given that name in his treatise on quicklime.

As fixed air makes a considerable part of the subject of the following papers; and as the name might incline one to think, that it signified any sort of air which is contained in other bodies in an unelastic form; I thought it best to give this explanation before I went any farther.

Before

Before I proceed to the experiments themselves, it will be proper to mention the principal methods used in making them.

In order to fill a bottle with the air discharged from metals or alkaline substances by solution in acids, or from animal or vegetable substances by fermentation, I make use of the contrivance represented in TAB. VII. Fig. 1. where A represents the bottle, in which the materials for producing air are placed; having a bent glass tube C ground into it, in the manner of a stopper. E represents a vessel of water. D the bottle to receive the air, which is first filled with water, and then inverted into the vessel of water, over the end of the bent tube. Ff represents the string, by which the bottle is suspended. When I would measure the quantity of air, which is produced by any of these substances, I commonly do it by receiving the air in a bottle, which has divisions marked on its sides with a diamond, shewing the weight of water, which it requires to fill the bottle up to those divisions: but sometimes I do it by making a mark on the side of the bottle in which I have received the air, answering to the surface of the water therein; and then, setting the upright, find how much water it requires to fill it up to that mark.

In order to transfer the air out of one bottle into another, the simplest way, and that which I have ofteneft made use of, is that represented Fig. 2. where A is the bottle, into which the air is to be transferred: it is supposed to be filled with water and inverted into the vessel of water DEFG, and suspended there by a string: the line DG is the surface of the water: B represents a tin funnel held under the mouth of the bottle: C represents the inverted bottle, out of which  
the

the air is to be transferred; the mouth of which is lifted up till the air runs out of it into the funnel, and from thence into the bottle A.

In order to transfer air out of a bottle into a bladder, the contrivance Fig. 3. is made use of. A is the bottle out of which the air is to be transferred, inverted into the vessel of water FGHK: B is a bladder whose neck is tied fast over the hollow piece of wood Cc, so as to be air-tight. Into the piece of wood is run a bent pewter pipe D, and secured with lute\*. The air is then pressed out of the bladder as well as possible, and a bit of wax E stuck upon the other end of the pipe, so as to stop up the orifice. The pipe, with the wax upon it, is then run up into the inverted bottle, and the wax torn off by rubbing it against the sides. By this means, the end of the pipe is introduced within the bottle, without suffering any water to get within it. Then, by letting the bottle descend, so as to be totally immersed in the water, the air is forced into the bladder.

The weights used in the following experiments, are troy weights, 1 ounce containing 480 grains. By an ounce or grain measure, I mean such a measure as contains one ounce or grain Troy of water.

\* The lute used for this purpose, as well as in all the following experiments, is composed of almond powder, made into a paste with glue, and beat a good deal with a heavy hammer. This is the strongest and most convenient lute I know of. A tube may be cemented with it to the mouth of a bottle, so as not to suffer any air to escape at the joint, though the air within is compressed by the weight of several inches of water.



## EXPERIMENTS ON FACTITIOUS AIR.

## PART I.

*Containing Experiments on Inflammable Air.*

**I** Know of only three metallic substances, namely, zinc, iron and tin, that generate inflammable air by solution in acids; and those only by solution in the diluted vitriolic acid, or spirit of salt.

Zinc dissolves with great rapidity in both these acids; and, unless they are very much diluted, generates a considerable heat. One ounce of zinc produces about 356 ounce measures of air: the quantity seems just the same whichever of these acids it is dissolved in. Iron dissolves readily in the diluted vitriolic acid, but not near so readily as zinc. One ounce of iron wire produces about 412 ounce measures of air: the quantity was just the same, whether the oil of vitriol was diluted with  $1\frac{1}{2}$ , or 7 times its weight of water: so that the quantity of air produced seems not at all to depend on the strength of the acid.

Iron dissolves but slowly in spirit of salt while cold: with the assistance of heat it dissolves moderately fast. The air produced thereby is inflammable; but I have not tried how much it produces.

Tin was found to dissolve scarce at all in oil of vitriol diluted with an equal weight of water, while cold: with the assistance of a moderate heat it dissolved slowly, and generated air, which was inflammable: the quantity was not ascertained.

Tin dissolves slowly in strong spirit of salt while cold: with the assistance of heat it dissolves moderately fast.

fast. One ounce of tinfoil yields 202 ounce measures of inflammable air.

These experiments were made, when the thermometer was at  $50^{\circ}$  and the barometer at 30 inches.

All these three metallic substances dissolve readily in the nitrous acid, and generate air; but the air is not at all inflammable. They also unite readily, with the assistance of heat, to the undiluted acid of vitriol; but very little of the salt, formed by their union with the acid, dissolves in the fluid. They all unite to the acid with a considerable effervescence, and discharge plenty of vapours, which smell strongly of the volatile sulphureous acid, and which are not at all inflammable. Iron is not sensibly acted on by this acid, without the assistance of heat; but zinc and tin are in some measure acted on by it, while cold.

It seems likely from hence, that, when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are dissolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, produced by uniting these metallic substances by heat to the undiluted vitriolic acid, shew plainly, that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid

united to phlogiston\*. It is highly probable too, that the same thing happens in dissolving these metallic substances in the nitrous acid; as the fumes produced during the solution appear plainly to consist in great measure of the nitrous acid, and yet it appears, from their more penetrating smell and other reasons, that the acid must have undergone some change in its nature, which can hardly be attributed to any thing else than its union with the phlogiston. As to the inflammable air, produced by dissolving these substances in spirit of salt or the diluted vitriolic acid, there is great reason to think, that it does not contain any of the acid in its composition; not only because it seems to be just the same whichever of these acids it is produced by; but also because there is an inflammable air, seemingly much of the same kind as this, produced from animal substances in putrefaction, and from vegetable substances in distillation, as will be shewen hereafter; though there can be no reason to suppose, that this kind of inflammable air owes its production to any acid. I now proceed to the experiments made on inflammable air.

I cannot find that this air has any tendency to lose its elasticity by keeping, or that it is at all absorbed, either by water, or by fixed or volatile alcalies; as I have kept some by me for several weeks in a bottle inverted into a vessel of water; without any sensible

\* Sulphur is allowed by chymists, to consist of the plain vitriolic acid united to phlogiston. The volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur. A circumstance which I think shews the truth of this, is that if oil of vitriol be distilled, from sulphur, the liquor, which comes over, will be the volatile sulphureous acid.

decrease of bulk; and as I have also kept some for a few days, in bottles inverted into vessels of sope leys and spirit of sal ammoniac, without perceiving their bulk to be at all diminished.

It has been observed by others, that, when a piece of lighted paper is applied to the mouth of a bottle, containing a mixture of inflammable and common air, the air takes fire, and goes off with an explosion. In order to observe in what manner the effect varies according to the different proportions in which they are mixed, the following experiment was made.

Some of the inflammable air, produced by dissolving zinc in diluted oil of vitriol, was mixed with common air in several different proportions, and the inflammability of these mixtures tried one after the other in this manner. A quart bottle was filled with one of these mixtures, in the manner represented in Fig. 2. The bottle was then taken out of the water, set upright on a table, and the flame of a lamp or piece of lighted paper applied to its mouth. But, in order to prevent the included air from mixing with the outward air, before the flame could be applied, the mouth of the bottle was covered, while under water, with a cap made of a piece of wood covered with a few folds of linnen; which cap was not removed till the instant that the flame was applied. The mixtures were all tried in the same bottle; and, as they were all ready prepared, before the inflammability of any of them was tried, the time elapsed between each trial was but small: by which means I was better able to compare the loudness of the sound in each trial. The result of the experiment is as follows.

With one part of inflammable air to 9 of common air, the mixture would not take fire, on applying the lighted paper to the mouth of the bottle; but, on putting it down into the belly of the bottle, the air took fire, but made very little sound.

With 2 parts of inflammable to 8 of common air, it took fire immediately, on applying the flame to the mouth of the bottle, and went off with a moderately loud noise.

With 3 parts of inflammable air to 7 of common air, there was a very loud noise.

With 4 parts of inflammable to 6 of common air, the sound seemed very little louder.

With equal quantities of inflammable and common air, the sound seemed much the same. In the first of these trials, namely, that with one part of inflammable to 9 of common air, the mixture did not take fire all at once, on putting the lighted paper into the bottle; but one might perceive the flame to spread gradually through the bottle. In the three next trials, though they made an explosion, yet I could not perceive any light within the bottle. In all probability, the flame spread so instantly through the bottle, and was so soon over, that it had not time to make any impression on my eye. In the last mentioned trial, namely, that with equal quantities of inflammable and common air, a light was seen in the bottle, but which quickly ceased.

With 6 parts of inflammable to 4 of common air, the sound was not very loud: the mixture continued burning a short time in the bottle, after the sound was over.

With.

With 7 parts of inflammable to 3 of common air, there was a very gentle bounce or rather puff: it continued burning for some seconds in the belly of the bottle.

A mixture of 8 parts of inflammable to 2 of common air caught fire on applying the flame, but without any noise: it continued burning for some time in the neck of the bottle, and then went out, without the flame ever extending into the belly of the bottle.

It appears from these experiments, that this air, like other inflammable substances, cannot burn without the assistance of common air. It seems too, that, unless the mixture contains more common than inflammable air, the common air therein is not sufficient to consume the whole of the inflammable air; whereby part of the inflammable air remains, and burns by means of the common air, which rushes into the bottle after the explosion.

In order to find whether there was any difference in point of inflammability between the air produced from different metals by different acids, five different sorts of air, namely, 1. Some produced from zinc by diluted oil of vitriol, and which had been kept about a fortnight; 2. Some of the same kind of air fresh made; 3. Air produced from zinc by spirit of salt; 4. Air from iron by the vitriolic acid; 5. Air from tin by spirit of salt; were each mixed separately with common air in the proportion of 2 parts of inflammable air to  $7\frac{7}{10}$  of common air, and their inflammability tried in the same bottle, that was used for the former experiment, and with the same precautions. They each went off with a pretty loud noise, and without any difference in the sound that I could

could be sure of. Some more of each of the above parcels of air were then mixed with common air, in the proportion of 7 parts of inflammable air to  $3\frac{1}{2}$  of common air, and tried in the same way as before. They each of them went off with a gentle bounce, and burnt some time in the bottle, without my being able to perceive any difference between them.

In order to avoid being hurt, in case the bottle should burst by the explosion, I have commonly, in making these sort of experiments, made use of an apparatus contrived in such manner, that, by pulling a string, I drew the flame of a lamp over the mouth of the bottle, and at the same time pulled off the cap, while I stood out of the reach of danger. I believe, however, that this precaution is not very necessary; as I have never known a bottle to burst in any of the trials I have made.

The specific gravity of each of the above-mentioned sorts of inflammable air, except the first, was tried in the following manner. A bladder holding about 100 ounce measures was filled with inflammable air, in the manner represented in Fig. 3. and the air pressed out again as perfectly as possible. By this means the small quantity of air remaining in the bladder was almost intirely of the inflammable kind. 80 ounce measures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the same manner: after which, the pewter pipe was taken out of the wooden cap of the bladder, the orifice of the cap stopt up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air pressed out as perfectly as possible, and the bladder weighed again. It was found to have increased

creased in weight  $40\frac{3}{4}$  grains. Therefore the air pressed out of the bladder weighs  $40\frac{3}{4}$  grains less than an equal quantity of common air: but the quantity of air pressed out of the bladder must be nearly the same as that which was forced into it, *i. e.* 80 ounce measures: consequently 80 ounce measures of this sort of inflammable air weigh  $40\frac{3}{4}$  grains less than an equal bulk of common air. The three other sorts of inflammable air were then tried in the same way, in the same bladder, immediately one after the other. In the trial with the air from zinc by spirit of salt, the bladder increased  $40\frac{1}{2}$  grains on forcing out the air. In the trial with the air from iron, it increased  $41\frac{1}{2}$  grains, and in that with the air from tin, it increased 41 grains. The heat of the air, when this experiment was made, was  $50^{\circ}$ ; the barometer stood at  $29\frac{3}{4}$  inches.

There seems no reason to imagine, from these experiments, that there is any difference in point of specific gravity between these four sorts of inflammable air; as the small difference observed in these trials is in all probability less than what may arise from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce measures of inflammable air weigh 41 grains less than an equal bulk of common air. Therefore, if the density of common air, at the time when this experiment was tried, was 800 times less than that of water, which, I imagine, must be near the truth\*, inflam-

\* Mr. Hawksbee, whose determination is usually followed as the most exact, makes air to be more than 850 times lighter than water; *vid.* Hawksbee's experiments, p. 94, or Cotes's Hydrostatics, p. 159. But his method of trying the experiment must in all probability make it appear lighter than it really is. For



mable air must be 5490 times lighter than water, or near 7 times lighter than common air. But if the density of common air was 850 times less than that of water, then would inflammable air be 9200 times

having weighed his bottle under water, both when full of air and when exhausted, he supposes the difference of weight to be equal to the weight of the air exhausted; whereas in reality it is not so much: for the bottle, when exhausted, must necessarily be compressed, and on that account weigh heavier in water than it would otherwise do. Suppose, for example, that air is really 800 times lighter than water, and that the bottle is compressed  $\frac{1}{2000}$  part of its bulk; which seems no improbable supposition: the weight of the bottle in water will thereby be increased by  $\frac{1}{2000}$  of the weight of a quantity of water of the same bulk, or more than  $\frac{1}{25}$  of the weight of the air exhausted: whence the difference of weight will be not so much as  $\frac{4}{5}$  of the weight of the air exhausted: and therefore the air will appear lighter than it really is in the proportion of more than 15 to 14, *i. e.* more than 857 times lighter than water: whereas, if the ball had been weighed in air in both circumstances, the error arising from the compression would have been very trifling.

It appears, from some experiments that have been made by weighing a ball in air, while exhausted, and also after the air was let in, that air, when the thermometer is at  $50^{\circ}$ , and the barometer at  $29\frac{3}{4}$ , is about 800 times lighter than water. Though the weight of the air exhausted was little more than 50 grains, no error could well arise near sufficient to make it agree with Hawksbee's experiment. Air seems to expand about  $\frac{1}{3000}$  part by  $1^{\circ}$  of heat, whence its density in any other state of the atmosphere is easily determined. The density here assumed agrees very well with the rule given by the gentlemen, who measured the length of a degree in Peru, for finding the height of mountains barometrically, and which is given in the *Connoissance des mouvemens celestes, année 1762*. To make that rule agree accurately with observation, the density of air, whose heat is the same as that of the places where these observations were made, and which I imagine we may estimate at about  $45^{\circ}$ , should be 798 times less than that of water, when the barometer stands at  $29\frac{3}{4}$ .

lighter than water, or  $10\frac{1}{10}$  lighter than common air.

This method of finding the density of factitious air is very convenient and sufficiently accurate, where the density of the air to be tried is not much less than that of common air, but cannot be much depended on in the present case, both on account of the uncertainty in the density of common air, and because we cannot be certain but what some common air might be mixed with the inflammable air in the bladder, notwithstanding the precautions used to prevent it; both which causes may produce a considerable error, where the density of the air to be tried is many times less than that of common air. For this reason, I made the following experiments.

I endeavoured to find the weight of the air discharged from a given quantity of zinc by solution in the vitriolic acid, in the manner represented in Fig. 4. A is a bottle filled near full with oil of vitriol diluted with about six times its weight of water: B is a glass tube fitted into its mouth, and secured with lute: C is a glass cylinder fastened on the end of the tube, and secured also with lute. The cylinder has a small hole at its upper end to let the inflammable air escape, and is filled with dry pearl-ashes in coarse powder. The whole apparatus, together with the zinc, which was intended to be put in, and the lute which was to be used in securing the tube to the neck of the bottle, were first weighed carefully; its weight was 11930 grains. The zinc was then put in, and the tube put in its place. By this means, the inflammable air was made to pass through the dry pearl-ashes; whereby it must have been pretty effectually deprived of any acid

or watery vapours that could have ascended along with it. The use of the glass tube B was to collect the minute jets of liquor, that were thrown up by the effervescence, and to prevent their touching the pearl-ashes; for which reason, a small space was left between the glass-tube and the pearl-ashes in the cylinder. When the zinc was dissolved, the whole apparatus was weighed again, and was found to have lost  $11\frac{3}{4}$  grains in weight\*; which loss is principally owing to the weight of the inflammable air discharged. But it must be observed, that, before the effervescence, that part of the bottle and cylinder, which was not occupied by other more solid matter, was filled with common air; whereas, after the effervescence, it was filled with inflammable air; so that, upon that account alone, supposing no more inflammable air to be discharged than what was sufficient to fill that space, the weight of the apparatus would have been diminished by the difference of the weight of that quantity of common air and inflammable air. The whole empty space in the bottle and cylinder was about 980 grain measures, there is no need of exactness; and the difference of the weight of that quantity of common and inflammable air is about one grain: therefore the true weight of the inflammable air discharged, is  $10\frac{3}{4}$  grains. The quantity of zinc used was 254 grains, and consequently the weight of the air discharged is  $\frac{2}{23}$  or  $\frac{1}{24}$  of the weight of the zinc.

\* As the quantity of lute used was but small, and as this kind of lute does not lose a great deal of its weight by being kept in a moderately dry room, no sensible error could arise from the drying of the lute during the experiment.

It was before said, that one grain of zinc yielded 356 grain measures of air: therefore 254 grains of zinc yield 90427 grain measures of air; which we have just found to weigh  $10\frac{3}{4}$  grains; therefore inflammable air is about 8410 times lighter than water, or  $10\frac{1}{2}$  times lighter than common air.

The quantity of moisture condensed in the pearl-ashes was found to be about  $1\frac{1}{4}$  grains.

By another experiment, tried exactly in the same way, the density of inflammable air came out 8300 times less than that of water.

The specific gravity of the air, produced by dissolving zinc in spirit of salt, was tried exactly in the same manner. 244 grains of zinc being dissolved in spirit of salt diluted with about four times its weight of water, the loss in effervescence was  $10\frac{3}{4}$  grains; the empty space in the bottle and cylinder was 914 grain measures; whence the weight of the inflammable air was  $9\frac{3}{4}$  grains, and consequently its density was 8910 times less than that of water.

By another experiment, its specific gravity came out 9030 times lighter than water.

A like experiment was tried with iron.  $250\frac{1}{2}$  grains of iron being dissolved in oil of vitriol diluted with four times its weight of water, the loss in effervescence was 13 grains, the empty space 1420 grain measures. Therefore the weight of the inflammable air was  $11\frac{3}{4}$  grains *i. e.* about  $\frac{1}{2}$  of the weight of the iron, and its density was 8973 times less than that of water. The moisture condensed was  $1\frac{1}{4}$  grains.

A like experiment was tried with tin. 607 grains of tin-foil being dissolved in strong spirit of salt, the loss in effervescence was  $14\frac{3}{4}$  grains, the empty space 873 grain

grain measures: therefore the weight of the inflammable air was  $13\frac{3}{4}$  grains *i. e.*  $\frac{1}{4}$  of the weight of the tin, and its density 8918 times less than that of water. The quantity of moisture condensed was about three grains.

It is evident, that the truth of these determinations depend on a supposition, that none of the inflammable air is absorbed by the pearl-ashes. In order to see whether this was the case or no, I dissolved 86 grains of zinc in diluted acid of vitriol, and received the air in a measuring bottle in the common way. Immediately after, I dissolved the same quantity of zinc in the same kind of acid, and made the air to pass into the same measuring bottle, through a cylinder filled with dry pearl-ashes, in the manner represented in Fig. 5. I could not perceive any difference in their bulks.

It appears from these experiments, that there is but little, if any, difference in point of density between the different sorts of inflammable air. Whether the difference of density observed between the air procured from zinc, by the vitriolic and that by the marine acid is real, or whether it is only owing to the error of the experiment, I cannot pretend to say. By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or eleven times lighter than common air.

In order to see whether inflammable air, in the state in which it is, when contained in the inverted bottles, where it is in contact with water, contains any considerable quantity of moisture dissolved in it, I forced 192 ounce measures of inflammable air, through a cylinder filled with dry pearl-ashes, by means of the same apparatus, which I used for filling the bladders with inflam-

inflammable air, and which is represented in Fig. 3. The cylinder was weighed carefully before and after the air was forced through; whereby it was found to have increased 1 grain in weight. The empty space in the cylinder was 248 grains, the difference of weight of which quantity of common and inflammable air is  $\frac{1}{4}$  of a grain. Therefore the real quantity of moisture condensed in the pearl-ashes is  $1\frac{1}{4}$  grain. The weight of 192 ounce measures of inflammable air deprived of its moisture appears from the former experiments to be  $10\frac{1}{2}$  grains; therefore its weight when saturated with moisture would be  $11\frac{3}{4}$  grains. Therefore inflammable air, in that state in which it is in, when kept under the inverted bottles, contains near  $\frac{1}{9}$  its weight of moisture; and its specific gravity in that state is 7840 times less than that of water.

I made an experiment with design to see, whether copper produced any inflammable air by solution in spirit of salt. I could not procure any inflammable air thereby: but the phenomena attending it seem remarkable enough to deserve mentioning. The apparatus used for this experiment was of the same kind as that represented in Fig. 1. The bottle A was filled almost full of strong spirit of salt, with some fine copper wire in it. The wire seemed not at all acted on by the acid, while cold; but, with the assistance of a heat almost sufficient to make the acid boil, it made a considerable effervescence, and the air passed through the bent tube, into the bottle D, pretty fast, till the air forced into it by this means seemed almost equal to the empty space in the bent tube and the bottle A: when, on a sudden, without any sensible alteration of the heat, the water rushed violently through

through the bent tube into the bottle A, and filled it almost intirely full.

The experiment was repeated again in the same manner, except that I took away the bottle D, and let out some of the water of the cistern: so that the end of the bent tube was out of water. As soon as the effervescence began, the vapours issued visibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A small empty phial was then inverted over the end of the bent tube, so that the mouth of the phial was immersed in the water, the end of the tube being within the body of the phial and out of water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to shake the inverted phial a little, the water suddenly rushed in, and filled it almost full; from thence it passed through the bent tube into the bottle A, and filled it quite full. It appears likely from hence that copper, by solution in the marine acid, produces an elastic fluid, which retains its elasticity as long as there is a barrier of common air between it and the water, but which immediately loses its elasticity, as soon as it comes in contact with the water. In the first experiment, as long as any considerable quantity of common air was left in the bottle containing the copper and acid, the vapours, which passed through the bent tube, must have contained a good deal of common air. As soon therefore as any part of these vapours came to the farther end of the bent tube, where they were in contact with the water, that part of them, which consisted of the air from copper, would be immediately condensed, leaving

ing the common air unchanged; whereby the end of the tube would be filled with common air only; by which means the vapours, contained in the rest of the tube and bottle A, seem to have been defended from the action of the water. But when almost all the common air was driven out of the bottle, then the proportion of common air contained in the vapours, which passed through the tube, seems to have been too small to defend them from the action of the water. In the second experiment, the narrow space left between the neck of the inverted phial and the tube would answer much the same end, in defending the vapours within the inverted phial from the action of the water, as the bent tube in the first experiment did in defending the vapours within the bottle from the action of the water.

## EXPERIMENTS ON FACTITIOUS AIR.

### PART II.

*Containing Experiments on Fixed Air, or that Species of Factitious Air, which is produced from Alcaline Substances, by Solution in Acids or by Calcination.*

#### EXPERIMENT I.

**T**HE air produced, by dissolving marble in spirit of salt, was caught in an inverted bottle of water, in the usual manner. In less than a day's time, much the greatest part of the air was found to be absorbed. The water contained in the inverted bottle was found to precipitate the earth from lime-water; a sure sign that it had absorbed fixed air\*.

\* Lime, as Dr. Black has shewn, is no more than a calcarious earth rendered soluble in water by being deprived of its fixed



## EXPERIMENT II.

I filled a Florence flask in the same way with the same kind of fixed air. When full, I stopt up the mouth of the flask with my finger, while under water, and removed it into a vessel of quicksilver, so that the mouth of the flask was intirely immersed therein. It was kept in this situation upwards of a week. The quicksilver rose and fell in the neck of the flask, according to the alterations of heat and cold, and of the height of the barometer; as it would have done if it had been filled with common air. But it appeared, by comparing together the heights of the quicksilver at the same temper of the atmosphere, that no part of the fixed air had been absorbed or lost its elasticity. The flask was then removed, in the same manner as before, into a vessel of sope leys. The fixed air, by this means, coming in contact with the sope leys, was quickly absorbed.

I also filled another Florence flask with fixed air, and kept it with its mouth immersed in a vessel of quicksilver in the same manner as the other, for upwards of a year, without being able to perceive any air to be absorbed. On removing it into a vessel of sope leys, the air was quickly absorbed like the former.

It appears from this experiment, that fixed air has no disposition to lose its elasticity, unless it meets with

air. Lime water is a solution of lime in water: therefore, on mixing lime water with any liquor containing fixed air, the lime absorbs the air, becomes insoluble in water, and is precipitated. This property of water, of absorbing fixed air, and then making a precipitate with lime water, has been taken notice of by Mr. M'Bride.

water,

with water or some other substance proper to absorb it, and that its nature is not altered by keeping.

### EXPERIMENT III.

In order to find how much fixed air water would absorb, the following experiment was made. A cylindrical glass, with divisions marked on its sides with a diamond, shewing the quantity of water which it required to fill it up to those marks, was filled with quicksilver, and inverted into a glass filled with the same fluid. Some fixed air was then forced into this cylindrical glass, in the same manner that it was into the inverted bottles of water, in the former experiments; except that, to prevent any common air from being forced into the glass along with the fixed, I took care not to introduce the end of the bent tube within the cylindrical glass, till I was well assured that no common air to signify could remain within the bottle. This was done by first introducing the end of the bent tube within an inverted bottle of water, and letting it remain there, till the air driven into this bottle was at least 10 times as much as would fill the empty space in the bent tube, and the bottle containing the marble and acid. By this means one might be well assured, that the quantity of common air remaining within the bent tube and bottle must be very trifling. The end of the bent tube was then introduced within the cylindrical glass, and kept there till a sufficient quantity of fixed air was let up. After letting it stand a few hours, the division answering to the surface of the quicksilver in the cylinder was observed and wrote down, by which it was known how much fixed air had been let up. A little rain water

was then introduced into the cylindrical glass, by pouring some rain water into the vessel of quicksilver, and then lifting up the cylindrical glass so as to raise the bottom of it a little way out of the quicksilver. After having suffered it to stand a day or two, in which time the water seemed to have absorbed as much fixed air as it was able to do, the division answering to the upper surface of the water, and also that answering to the surface of the quicksilver, were observed: by which it was known how much air remained not absorbed, and also how much water had been introduced: the division answering to the surface of the water telling how much air remained not absorbed, and the difference of the two divisions telling how much water had been let up. More water was then let up in the same manner, at different times, till almost the whole of the fixed air was absorbed. As all water contains a little air, the water used in this experiment was first well purged of it by boiling, and then introduced into the cylinder while hot. The result of the experiment is given in the following table; in which the first column shews the bulk of the water let up each time; the second shews the bulk of air absorbed each time; the third the whole bulk of water let up; the fourth the whole bulk of air absorbed; and the fifth column shews the bulk of air remaining not absorbed. In order to set the result in a clearer light, the whole bulk of air introduced into the cylinder is called 1, and the other quantities set down in decimals thereof.

Bulk of air let up = 1.

Bulk of water let up each time.	Bulk of air absorbed each time.	Whole bulk of water let up.	Whole bulk of air absorbed.	Whole bulk of air remaining.
.322	.374	.322	.374	.626
.481	.485	.803	.859	.141
.082	.048	.885	.907	.093
.145	.079	1.030	.986	.014

I imagine that the quantities of water let up and of the air absorbed could be estimated to about three or four 1000th parts of the whole bulk of air introduced. The height of the thermometer, during the trial of this experiment, was at a medium  $55^{\circ}$ .

This experiment was tried once before. The result agreed pretty nearly with this; but, as it was not tried so carefully, the result is not set down.

It appears from hence, that the fixed air contained in marble consists of substances of different natures, part of it being more soluble in water than the rest: it appears too, that water, when the thermometer is about  $55^{\circ}$ , will absorb rather more than an equal bulk of the more soluble part of this air.

It appears, from an experiment which will be mentioned hereafter, that water absorbs more fixed air in cold weather than warm; and, from the following experiment, it appears, that water heated to the boiling point is so far from absorbing air, that it parts with what it has already absorbed.

## EXPERIMENT IV.

Some water, which had absorbed a good deal of fixed air, and which made a considerable precipitate with lime water, was put into a phial, and kept about  $\frac{1}{4}$  of an hour in boiling water. It was found when cold not to make any precipitate, or to become in the least cloudy on mixing it with lime water.

## EXPERIMENT V.

Water also parts with the fixed air, which it has absorbed by being exposed to the open air. Some of the same parcel of water, that was used for the last experiment, being exposed to the air in a saucer for a few days, was found at the end of that time to make no clouds with lime water.

## EXPERIMENT VI.

In like manner it was tried how much of the same sort of fixed air was absorbed by spirits of wine. The result is as follows.

Bulk of air introduced = 1.

Spirit let up each time.	Air absorbed each time.	Whole bulk of spirit let up.	Whole bulk of air absorbed.	Bulk of air remaining.
.207	.453	.207	.453	.547
.146	.274	.353	.727	.273
.074	.103	.427	.830	.170
.046	.030	.473	.860	.140

The mean height of the thermometer, during the trial of the experiment, was  $46^{\circ}$ . Therefore spirit of wine, at the heat of  $46^{\circ}$ , absorbs near  $2\frac{1}{4}$  times its bulk of the more soluble part of this air.

#### EXPERIMENT VII.

After the same manner it was tried how much fixed air is absorbed by oil. Some olive oil, equal in bulk to  $\frac{1}{3}$  part of the fixed air in the cylindrical glass, was let up. It absorbed rather more than an equal bulk of air; the thermometer being between 40 and 50. The experiment was not carried any farther. The oil was found to absorb the air very slowly.

#### EXPERIMENT VIII.

The specific gravity of fixed air was tried by means of a bladder, in the same manner which was made use of for finding the specific gravity of inflammable air; except that the air, instead of being caught in an inverted bottle of water, and thence transferred into the bladder, was thrown into the bladder immediately from the bottle which contained the marble and spirit of salt, by fastening a glass tube to the wooden cap of the bladder, and luting that to the mouth of the bottle containing the effervescing mixture, in such manner as to be air-tight. The bladder was kept on till it was quite full of fixed air: being then taken off and weighed, it was found to lose 34 grains, by forcing out the air. The bladder was previously found to hold 100 ounce measures. Whence if the outward air, at the time when this experiment was tried, is supposed to have been 800 times lighter than water, fixed air is 511 times lighter than water, or  $1\frac{1}{4}\frac{2}{5}$  times heavier

heavier than common air. The heat of the air during the trial of this experiment was  $45^{\circ}$ .

By another experiment of the same kind, made when the thermometer was at  $65^{\circ}$ , fixed air seemed to be about 563 times lighter than water.

#### EXPERIMENT IX.

Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminished by the mixture of a small quantity of fixed air; as appears from hence.

A small wax candle burnt  $80''$  in a receiver, which held 190 ounce measures, when filled with common air only.

The same candle burnt  $51''$  in the same receiver, when filled with a mixture of one part of fixed air to 19 of common air, *i. e.* when the fixed air was  $\frac{1}{20}$  of the whole mixture.

When the fixed air was  $\frac{2}{40}$  of the whole mixture, the candle burnt  $23''$ .

When the fixed air was  $\frac{1}{10}$  of the whole, it burnt  $11''$ .

When the fixed air was  $\frac{6}{33}$  or  $\frac{1}{9}$  of the whole mixture, the candle went out immediately.

Hence it should seem, that, when the air contains near  $\frac{1}{9}$  its bulk of fixed air, it is unfit for small candles to burn in. Perhaps indeed, if I had used a larger candle and a larger receiver, it might have burnt in a mixture containing a larger proportion of fixed air than this; as I believe that large flaming bodies will burn in a fouler air than small ones. But this is insufficient to shew, that the power, which common air

has

has of keeping fire alive, is very much diminished by a small mixture of fixed air.

This experiment was tried, by setting the candle in a large cistern of water, in such manner that the flame was raised but a little way above the surface; the receiver being inverted full of water into the same cistern. The proper quantity of fixed air was then let up, and the remaining space filled with common air, by raising the receiver gradually out of water; after which, it was immediately whelmed gently over the burning candle.

*Experiments on the Quantity of Fixed Air, contained in Alcaline Substances.*

EXPERIMENT X.

The quantity of fixed air contained in marble was found by dissolving some marble in spirit of salt, and finding the loss of weight, which it suffered in effervescence, in the same manner as I found the weight of the inflammable air discharged from metals by solution in acids, except that the cylinder was filled with shreds of filtering paper instead of dry pearl ashes; for pearl ashes would have absorbed the fixed air that passed through them. The weight of the marble dissolved was  $311\frac{1}{2}$  grains. The loss of weight in effervescence was  $125\frac{1}{2}$  grains. The whole empty space in the bottle and cylinder was about 2700 grain measures: the excess of weight of that quantity of fixed, above an equal quantity of common, air is  $1\frac{3}{4}$  grains. Therefore the weight of the fixed air discharged is  $127\frac{1}{4}$  grains. The cylinder with the filtering paper was found to have increased  $1\frac{3}{4}$  grains in weight during the effervescence. The empty space in



in the cylinder was about 1160 grain measures: the excess of weight of which quantity of fixed air above an equal bulk of common air is  $\frac{3}{4}$  grains. Therefore the quantity of moisture condensed in the filtering paper is one grain, or about  $\frac{1}{1125}$  part of the weight of the air discharged.

As water has been already shewn to absorb fixed air, it seemed not improbable, but what there might be some fixed air contained in the solution of marble in spirit of salt; in which case the air discharged, during the effervescence, would not be the whole of the fixed air in the marble. In order to see whether this was the case, I poured some of the solution into lime water. It made scarce any precipitate; which, as the acid was intirely saturated with marble, it would certainly have done if the solution had contained any fixed air. It appears therefore from this experiment,

first, that marble contains  $\frac{127\frac{1}{2}}{311\frac{1}{2}} = \frac{407}{1000}$  of its weight of fixed air; and secondly, that the quantity of moisture, which flies off along with the fixed air in effervescence, is but trifling; as I imagine that the greatest part of what did fly off must have been condensed in the filtering paper.

By another experiment tried much in the same way, marble was found to contain  $\frac{408}{1000}$  of its weight of fixed air.

#### EXPERIMENT XI.

Volatile sal ammoniac dissolves with too great rapidity in acids, and makes too violent an effervescence, to allow one to try what quantity of fixed  
air

air it contains in the foregoing manner: I therefore made use of the following method.

Three small phials were weighed together in the same scale. The first contained some weak spirit of salt, the second contained some volatile sal ammoniac in moderate sized lumps without powder, corked up to prevent evaporation, and the third, intended for mixing the acid and alcali in, contained only a little water, and was covered with a paper cap, to prevent the small jets of liquor, which are thrown up during the effervescence, from escaping out of the bottle. In order to prevent too violent an effervescence, the acid and alcali were both added by a little at a time, care being taken that the acid should always predominate in the mixture. Care was also taken always to cover the bottle with the paper cap, as soon as any of the acid or alcali were added. As soon as the mixture was finished, the three phials were weighed again; whereby the loss in effervescence was found to be 134 grains. The weight of the volatile salt made use of was 254 grains, and was pretty exactly sufficient to saturate the acid. The solution appeared, by pouring some of it into lime water, to contain scarce any fixed air. Therefore 254 grains of the volatile sal ammoniac contain 134 grains of fixed air, *i.e.*  $\frac{528}{1000}$  of their weight. It appeared from the same experiment, that 1680 grains of the volatile salt saturate as much acid as 1000 grains of marble.

By another experiment, tried with some of the same parcel of volatile salt, it was found to contain  $\frac{533}{1000}$  of its weight of fixed air, and 1643 grains of it saturated as much acid as 1000 grains of marble. By a medium, the salt contained  $\frac{533}{1000}$  of its weight

of fixed air; and 1661 grains of it saturated as much acid as 1000 grains of marble.

One thousand grains of marble were found to contain  $407\frac{1}{2}$  grains of air, and 1661 grains of volatile fal ammoniac contain 885 grains. Therefore this parcel of volatile fal ammoniac contains more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 885 to  $407\frac{1}{2}$ , or of 217 to 100.

N. B. It is not unlikely, that the quantity of fixed air may be found to differ considerably in different parcels of volatile fal ammoniac; so that any one, who was to repeat these experiments, ought not to be surprized if he was to find the result to differ considerably from that here laid down. The same thing may be said of pearl ashes.

#### EXPERIMENT XII.

This serves to account for a remarkable phenomenon, which I formerly met with, on putting a solution of volatile fal ammoniac in water into a solution of chalk in spirit of salt. The earth was precipitated hereby, as might naturally be expected: but what surprized me, was, that it was attended with a considerable effervescence; though I was well assured, that the acid in the solution of chalk was perfectly neutralized. This is very easily accounted for, from the above-mentioned circumstance of volatile fal ammoniac containing more fixed air in proportion to the quantity of acid that it can saturate, than calcareous earths do. For the volatile alkali, by uniting to the acid, was necessarily deprived of its fixed air. Part of this air united to the calcareous earth, which  
was

was at the same time separated from the acid ; but, as the earth was not able to absorb the whole of the fixed air, the remainder flew off in an elastic form, and thereby produced an effervescence.

### EXPERIMENT XIII.

The same solution of volatile sal ammoniac made no precipitate, when mixed with a solution of Epsom salt ; though a mixture thereof with a little spirit of sal ammoniac, made with lime, immediately precipitated the magnesia from the same solution of Epsom salt ; as it ought to do according to Dr. Black's account of the affinity of magnesia and volatile alcalies to acids. This experiment is not so easily accounted for as the last ; but I imagine, that the magnesia is really separated from the acid by the volatile alcali ; but that it is soluble in water, when united to so great a proportion of fixed air, as is contained in a portion of volatile sal ammoniac, sufficient to saturate the same quantity of acid. The reason, why the mixture of the solution of volatile sal ammoniac, with the spirits of sal ammoniac made with lime, precipitates the magnesia from the Epsom salt, is that, as the spirits made with lime contain no fixed air, the mixture of these spirits with the solution of volatile sal ammoniac contains less air in proportion to the quantity of acid which it can saturate, than the solution of volatile sal ammoniac by itself does.

Volatile sal ammoniac requires a great deal of water to dissolve it, and the solution has not near so strong a smell as the spirits of sal ammoniac made with fixed alcali ; the reason of which is, that the latter contain much less fixed air. But volatile sal

ammoniac dissolves in considerable quantity in weak spirits of sal ammoniac made with lime, and the solution differs in no respect from the spirits made with fixed alcali. This is a convenient way of procuring the mild spirits of sal ammoniac, as those made with fixed alcali are seldom to be met with in the shops.

#### EXPERIMENT XIV.

The quantity of fixed air contained in pearl ashes was tried, by mixing a solution of pearl ashes with diluted oil of vitriol, in the same manner as was used for volatile sal ammoniac. As much of the solution was used as contained  $328\frac{1}{2}$  grains of dry pearl ashes. The loss of effervescence was 90 grains. The mixture, which was perfectly neutralized, being then added to a sufficient quantity of lime water, in order to see whether it contained any fixed air, a precipitate was made, which being dried weighed  $8\frac{1}{2}$  grains. Therefore, if we suppose this precipitate to contain as much fixed air as an equal weight of marble, which I am well assured cannot differ very considerably from the truth, the fixed air therein is  $3\frac{1}{2}$  grains, and consequently the air in  $328\frac{1}{2}$  grains of the pearl ashes, is  $93\frac{1}{2}$  grains, *i. e.*  $\frac{2884}{10000}$  of their weight.

By another experiment tried in the same way, they appeared to contain  $\frac{2887}{10000}$  of their weight of fixed air.

1558 grains of the pearl ashes were found to saturate as much acid as 1000 grains of marble. Therefore this parcel of pearl ashes contains more air in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 109 to 100.

EXPERI-

## EXPERIMENT XV.

Dr. Black says, that, by exposing a solution of salt of tartar for a long time to the open air, some crystals were formed in it, which seemed to be nothing else than the vegetable alcali united to more than its usual proportion of fixed air. This induced me to try, whether I could not perform the same thing more expeditiously, by furnishing the alcali with fixed air artificially; which I did in the manner represented in Fig. 6: where A represents a wide-mouthed bottle, containing a solution of pearl ashes; Bb represents a round wooden ring fastened over the mouth of the bottle, and secured with luting; C is a bladder bound tight over the wooden ring. This bladder, being first pressed close together, so as to drive out as much of the included air as possible, was filled with fixed air, by means of the bent tube D, one end of which is fixed into the wooden ring, and the other fastened into the mouth of the bottle E, containing marble and spirit of salt. By this means the fixed air thrown into the bladder mixed with the air in the bottle, and came in contact with the fixed alcali. The fixed air was by degrees absorbed, and crystals were formed on the surface of the fixed alcali, which were thrown to the bottom by shaking the bottle. When the alcali had absorbed as much fixed air as it would readily do, the crystals were taken out and dried on filtered paper, and the remaining solution evaporated; by which means some more crystals were procured.

N. B. It seemed, as, if not all the air discharged from the marble was of a nature proper to be absorbed by the alcali, but only part of it; for when the alcali had  
absorbed

absorbed somewhat more than  $\frac{1}{4}$  of the air first thrown into the bladder, it would not absorb any more: but, on pressing the remaining air out of the bladder, and supplying its place with fresh fixed air, a good deal of this new air was absorbed. I cannot, however, speak positively as to this point; as I am not certain whether the apparatus was perfectly airtight\*.

These crystals do not in the least attract the moisture of the air; as I have kept some, during a whole winter, exposed to the air in a room without a fire, without their growing at all moist or increasing in weight.

Being held over the fire in a glass vessel, they did not melt as many salts do, but rather grew white and calcined.

They dissolve in about four times their weight of water when the weather is temperate, and dissolve in greater quantity in hot water than cold.

It was found, by the same method, that was made use of for the volatile sal ammoniac, that these crystals contain  $\frac{423}{1000}$  of their weight of fixed air, and that 2035 grains of them saturate as much acid as 1000 grains of marble. Therefore these crystals contain more air in proportion to the quantity of acid they

\* Pearl ashes deprived of their fixed air, *i. e.* sops leys, will absorb the whole of the air discharged from marble; as I know by experience. But yet it is not improbable, but that the same alkali, when near saturated with fixed air, may be able to absorb only some particular part of it. For as it has been already shewn, that part of the air discharged from marble is more soluble in water than the rest; so it is not unlikely, but that part of it may have a greater affinity to fixed alkali, and be absorbed by it in greater quantity than the rest.

faturate, than marble does, in the ratio of 211 to 100.

### EXPERIMENT XVI.

As these crystals contain about as much fixed air in proportion to the quantity of acid, that they can faturate, as volatile sal ammoniac does, it was natural to expect, that they should produce the same effects with a solution of Epsom salt, or a solution of chalk in spirit of salt; as those effects seemed owing only to the great quantity of fixed air contained in volatile sal ammoniac. This was found to be the real case: for a solution of these crystals in five times their weight of water, being dropt into a solution of chalk in spirit of salt, the earth was precipitated, and an effervescence was produced. No precipitate was made on dropping some of the same solution into a solution of Epsom salt, though the mixture was kept upwards of twelve hours. But, upon heating this mixture over the fire, a great deal of air was discharged, and the magnesia was precipitated.

### EXPERIMENTS ON FACTITIOUS AIR.

#### P A R T III.

*Containing Experiments on the Air, produced by Fermentation and Putrefaction.*

**M**R. M<sup>c</sup>Bride has already shewn, that vegetable and animal substances yield fixed air by fermentation and putrefaction. The following experiments were made chiefly with a view of seeing, whether they yield any other sort of air besides that.

EXPERI-



## EXPERIMENT I.

The air produced from brown sugar and water, by fermentation, was caught in an inverted bottle of sops in the usual manner, and which is represented in Fig. 1. As the weather was too cold to suffer the sugar and water to ferment freely, the bottle containing it was immersed in water, which, by means of a lamp, was kept constantly at about  $80^{\circ}$  of heat. The quantity of sugar put into the bottle was 931 grains: it was dissolved in about  $6\frac{1}{2}$  times its weight of water, and mixed with 100 grains of yeast, by way of fermentation. The empty space left in the fermenting bottle and tube together measured 1920 grains. The mixture fermented freely, and generated a great deal of air, which was forced up in bubbles into the inverted bottle, but was absorbed by the sops, as fast as it rose up. It frothed greatly; but none of the froth or liquor ran over. In about ten days, the fermentation seeming almost over, the vessels were separated. The bottle with the fermented liquor was found to weigh 412 grains less than it did, before the fermentation began. As none of the liquor ran over, and as little or no moisture condensed within the bent tube, I think one may be well assured, that the loss of weight was owing intirely to the air forced into the inverted bottle; for the matter discharged, during the fermentation, must have consisted either of air, or of some other substance, changed into vapour: if this last was the case, I think it could hardly have failed, but that great part of those vapours must have condensed in the tube. The air remaining unabsorbed in the inverted bottle of sops was measured, and was

found to be exactly equal to the empty space left in the bent tube and fermenting bottle. It appears therefore, that there is not the least air of any kind discharged from the sugar and water by fermentation, but what is absorbed by the sope leys, and which may therefore be reasonably supposed to be fixed air. It seems also, that no part of the common air left in the fermenting bottle was absorbed by the fermenting mixture, or suffered any change in its nature from thence: for a small phial being filled with one part of this air, and two of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the same appearances, as far as I could perceive, as when the phial was filled with the same quantities of common and inflammable air.

The sugar used in this experiment was moist, and was found to lose  $\frac{228}{1000}$  parts of its weight by drying gently before a fire. Therefore the quantity of dry sugar used was 715 grains; and the weight of the air discharged by fermentation appears to be near 412 grains, *i. e.* near  $\frac{57}{100}$  parts of the weight of the dry sugar in the mixture.

The fermented liquor was found to have intirely lost its sweetness; so that the vinous fermentation seemed to be completed; but it was not grown at all four.

#### EXPERIMENT II.

The air, discharged from apple-juice by fermentation, was tried exactly in the same manner. The quantity set to ferment was 7060 grains, and was mixed with 100 grains of yeast. Some of the same parcel of

apple-juice, being evaporated gently to the consistence of a moderately hard extract, was reduced to  $\frac{1}{7}$  of its weight; so that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor fermented much faster than the sugar and water. The loss of weight during the fermentation was 384 grains. The air remaining unabsorbed in the inverted bottle of sops leys was lost by accident, so that it could not be measured; but, from the space it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty space in the bent tube and fermenting bottle, if it did at all. Therefore there is no reason to think that the apple-juice, any more than the sugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near  $\frac{384}{1009}$  of the weight of the extract contained in the apple-juice. The fermented liquor was very sour; so that it had gone beyond the vinous fermentation, and made some progress in the acetous fermentation.

In order to compare more exactly the nature of the air produced from sugar by fermentation, with that produced from marble by solution in acids, I made the three following experiments.

### EXPERIMENT III.

I first tried in what quantity the air from sugar was absorbed by water, and at the same time made a like experiment on the air discharged from marble, by solution in spirit of salt. This was done exactly in the same way as the former experiments of this kind. The result is as follows, beginning with the air from sugar and water.

Air

Air from sugar and water let up = 1000.

Bulk of water let up each time.	Bulk of air absorbed each time.	Wholebulk of water let up.	Wholebulk of air absorbed.	Bulk of air remaining.	Height of thermometer when observ. was made.
375	517	375	517	483	40
143	164	518	681	319	45
153	164	673	845	154	45
82	103	755	948	52	46

Air from marble let up = 1000.

391	473	391	473	527	40
143	133	534	606	394	45
284	115	818	811	189	45
194	80.	1.012	891	109	46

The apparatus used in this experiment was suffered to remain in the same situation till summer, when the thermometer stood at 65°. The bulk of the air from sugar, not absorbed by the water, was then found to be 287; so that the matter had remitted 235 parts of air. The bulk of the air from marble not absorbed, was 194; so that 85 parts were remitted; which is therefore a proof, that water absorbs less fixed air in warm weather than cold.

It appears from this experiment, that the air produced from sugar by fermentation, as well as that discharged from marble by solution in acids, consists of substances of different nature: part being absorbed by water in greater quantity than the rest. But, in

general, the air from sugar is absorbed in greater quantity than that from marble.

In forcing the air from sugar into the cylindrical glass, no sensible quantity of moisture was found to condense on the surface of the quicksilver, or sides of the glass; which is a proof that no considerable quantity of any thing except air could fly off from the sugar and water in fermentation.

#### EXPERIMENT IV.

The specific gravity of the air produced from sugar was found in the same way as that produced from marble. A bladder holding 102 ounce measures, being filled with this kind of air, lost  $29\frac{1}{3}$  grains on forcing out the air, the thermometer standing at  $62^{\circ}$ , and the barometer at  $29\frac{1}{2}$  inches. Whence, supposing the outward air during the trial of this experiment to be 826 times lighter than water, as it should be, according to the supposition made use of in the former parts of this paper, the air from sugar should be 554 times lighter than water. Its density therefore appears to be much the same as that of the air contained in marble; as that air appeared to be 511 times lighter than water, by a trial made when the thermometer was at  $45^{\circ}$ ; and 563 times lighter, by another trial when the thermometer was at  $65^{\circ}$ .

This air seems also to possess the property of extinguishing flame, in much the same degree as that produced from marble; as appears from the following experiment.

#### EXPERIMENT V.

A small wax candle burnt 15'' in a receiver filled with  $\frac{1}{10}$  of air from sugar, the rest common air.

In a mixture containing  $\frac{6}{33}$  or  $\frac{1}{9\frac{1}{2}}$  of air from sugar, the rest common air, the candle went out immediately. When the receiver was filled with common air only, the same candle burnt 72''.

The receiver was the same as that used in the former experiment of this kind, and the experiment tried in the same way, except that the air from sugar was first received in an empty bladder, and thence transferred into the inverted bottles of water, in which it was measured: for the air is produced from the sugar so slowly, that, if it had been received in the inverted bottles immediately, it would have been absorbed almost as fast as it was generated.

It appears from these experiments, that the air produced from sugar by fermentation, and in all probability that from all the other sweet juices of vegetables, is of the same kind as that produced from marble by solution in acids, or at least does not differ more from it than the different parts of that air do from each other, and may therefore justly be called fixed air. I now proceed to the air generated by putrefying animal substances.

#### EXPERIMENT VI.

The air produced from gravy broth by putrefaction, was forced into an inverted bottle of slope leys, in the same way as in the former experiment. The quantity of broth used, was 7640 grains, and was found, by evaporating some of the same to the consistence of a dry extract, to contain 163 grains of solid matter. The fermenting bottle was immersed in water kept constantly to the heat of about 96°. In  
about

about two days the fermentation seemed intirely over. The liquor smelt very putrid, and was found to have lost  $11\frac{1}{2}$  grains of its weight. The sops leys had acquired a brownish colour from the putrid vapours, and a musty smell. The air forced into the inverted bottle, and not absorbed by the sops leys, measured 6280 grains: the air left in the bent tube and fermenting bottle was 1100 grains; almost all of which must have been forced into the inverted bottles: so that this unabsorbed air is a mixture of about one part of common air and  $4\frac{7}{10}$  of factitious air.

This air was found to be inflammable; for a small phial being filled with 109 grain measures of it, and 301 of common air, which comes to the same thing as 90 grains of pure factitious air, and 320 of common air, it took fire on applying a piece of lighted paper, and went off with a gentle bounce, of much the same degree of loudness as when the phial was filled with the last mentioned quantities of inflammable air from zinc and common air. When the phial was filled with 297 grains of this air, and 113 of common air, *i. e.* with 245 of pure factitious air, and 165 of common air, it went off with a gentle bounce on applying the lighted paper; but I think not so loud as when the phial was filled with the last-mentioned quantities of air from zinc and common air.

5500 grain measures of this air, *i. e.* 4540 of pure factitious air, and 960 of common air, were forced into a piece of ox-gut furnished with a small brass cock, which I find more convenient for trying the specific gravity of small quantities of air, than a bladder: the gut increased  $4\frac{1}{2}$  grains in weight on

forcing out the air. A mixture of 4540 grains of air from zinc and 960 of common air being then forced into the same gut, it increased  $4\frac{3}{4}$  grains on forcing out the air. So that this factitious air should seem to be rather heavier than air from zinc; but the quantity tried was too small to afford any great degree of certainty.

N. B. The weight of 4540 grain measures of inflammable air, is  $\frac{5^8}{1^8}$  grains, and the weight of the same quantity of common air is  $5\frac{7}{10}$  grains.

On the whole it seems that this sort of inflammable air is nearly of the same kind as that produced from metals. It should seem, however, either to be not exactly the same, or else to be mixed with some air heavier than it, and which has in some degree the property of extinguishing flame, like fixed air.

The weight of the inflammable air discharged from the gravy appears to be about one grain, which is but a small part of the loss of weight which it suffered in putrefaction. Part of the remainder, according to Mr. M'Bride's experiments, must have been fixed air. But the colour and smell, communicated to the sops leys, shew, that it must have discharged some other substance besides fixed and inflammable air.

Raw meat also yields inflammable air by putrefaction, but not in near so great a quantity, in proportion to the loss of weight which it suffers, as gravy does. Four ounces of raw meat mixed with water, and treated in the same manner as the gravy, lost about 100 grains in putrefaction; but it yielded hardly more inflammable air than the gravy. This air  
seemed.



seemed of the same kind as the former; but, as the experiments were not tried so exactly, they are not set down.

I endeavoured to collect in the same manner the air discharged from bread and water by fermentation, but I could not get it to ferment, or yield any sensible quantity of air; though I added a little putrid gravy by way of ferment.

Received May 21, 1766.

XX. *A farther Account of the Polish Cochineal: from Dr. Wolfe, of Warsaw. Communicated by Henry Baker, F. R. S.*

Read June 5, 1766. **I**N the LIVth volume of the Philosophical Transactions, for the year 1764, Art. XV. the Royal Society has been pleased to publish two curious papers, communicated by Mr. Baker, from Dr. Wolfe of Warsaw, describing the Polish Cochineal, the plants on whose roots it is found, the manner of collecting and curing it, the method of dying therewith, and also the doctor's own experiments on these curious insects; the figures whereof are there given as engraven on a copper plate.

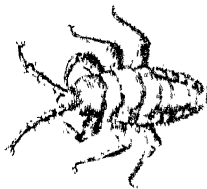
Since that time, the doctor has been very industrious in breeding and observing these insects, and has thereby discovered the male fly, about which he was before uncertain; and has sent to Mr. Baker an elegant picture



2.



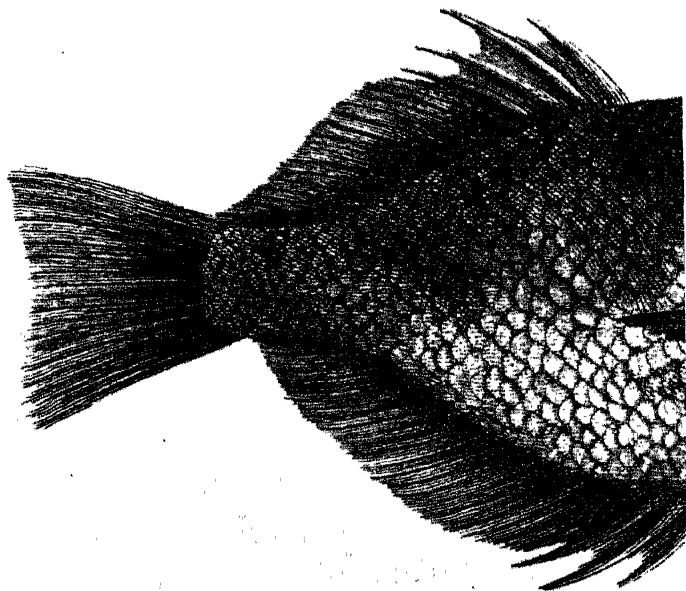
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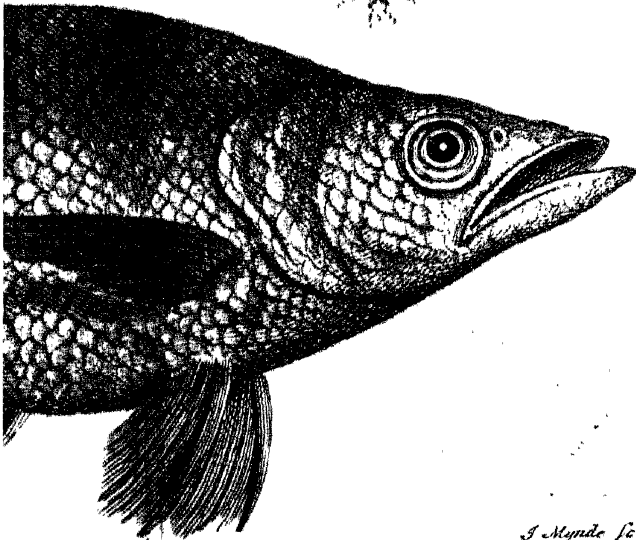
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6



*S. Mynde sc.*



picture thereof, painted from the life in its natural colours, and also of the young female just crept from the egg, both in their natural size, and as magnified by glasses; together with a drawing and description of the polygonum minus of Casper Bauhine, or scleranthus perennis of Linnaeus, which is the plant, adhering to the roots of which, this insect is chiefly found in Podolia and the Ukrain.

All these Mr. Baker takes the liberty to lay before the Royal Society, to compleat Dr. Wolfe's account of this insect; and as this plant is common in England, as well as the Potentilla and Fragraria, at the roots of which these insects are likewise found, he is in hopes, that such gentlemen, as have opportunity, will seek them in the months of June, July and August; the time they seek for, and collect, them in the above-mentioned countries. The curious will receive pleasure and information from comparing the male fly of this Polish Cochineal, with the male fly of the Cochineal of South America; communicated some time ago by Mr. John Ellis, Fellow of this Society, and published in the Transactions, Vol. LII. p. 664.

TAB. VIII. Fig. 1. The Polish Cochineal male insect, just come out of the egg, of its natural size\*.

Fig. 2. The same magnified.

Fig. 3. The female insect, just crept out of the egg, of the natural size. Its colour a brownish crimson.

Fig. 4. The same magnified.

Fig. 5. Polygonum minus IV<sup>um</sup> C. Bauh. or, Scleranthus perennis calycibus clausis, Linnæi.

\* The body and head of this beautiful little fly have several tints of a brownish crimson: the wings are white and transparent, except the darker parts in the plate, which are of a lively crimson colour.

The root fibrous; when old, woody. The young stalks of a grey green; in the second year, red. They have knots at different intervals. Each knot has two sharp-pointed leafy narrow stipulae. The stalks are dichotomi: and near the umbella there is, at every bifurcation, a flower twice as big as the others, having its seeds more ripe and perfect.

The calyx grows almost woody, and is five-pointed. The petala are small, oval-pointed, white, in number ten: the five stamina short: the antherae yellow: the pistilla two, very short.

The seeds egg-shaped, one or two strongly adhering to the calyx.


The whole plant, when old, has stalks ten inches in length, procumbent by the weight of the flowers, and making a sort of convex bush round about the root.

*XXI. Some further Intelligence relating to the Jaculator Fish, mentioned in the Philosophical Transactions for 1764, Art. XIV. from Mr. Hommel, at Batavia, together with the Description of another Species, by Dr. Pallas, F. R. S. in a letter to Mr. Peter Collinson, F. R. S. from John Albert Schloffer, M. D. F. R. S.*

Amsterdam, Feb. 15, 1766.

Read June 5,  
1766.

**W**HEN the Jaculator fish intends to catch a fly or any other insect, which is seen at a distance, it approaches very slowly and cautiously, and comes as much as possible perpendicularly

dicularly under the object: then the body being put in an oblique situation, more or less in this manner,  and the mouth and eyes being near the surface of the water, the Jaculator stays a moment quite immoveable, having its eyes directly fixed on the insect, and then begins to shoot, without ever shewing its mouth above the surface of the water, out of which the single drop, shot at the object, seems to rise.

With the closest attention I never could see any part of the mouth out of water, though I have very often seen the Jaculator fish shoot a great many drops one after another, without leaving its place and fixed situation.

No more than two different species of this fish are found here.

The first and rarest kind is that which I sent before; and to the description published in the LIVth volume of the Philosophical Transactions, the foregoing account may be added. You now will receive from me, a specimen of the second species, which is the most common here.

Batavia, October 30,  
1764.

Hommel.

TAB. VIII. Fig. 6. *Sciæna Jaculatrix*, quinque maculata, pinnis ventralibus adnatis, maxilla inferiore longiore.

Locus: Mare Indicum.

Magnitudo cyprini rutili.

B b 2

Corpus



Corpus macrolepidotum (*covered with large scales*), dorso convexo, versus pinnam ascendente, abdomine tumidulo, antice carinato.

Caput undique squamosum, corpore crassius, supra planiusculum.

Os ascendens, maxilla inferiore multo longiore, utriusque marginibus subtiliter scabris.

Palatum osseum, concavo carinatum, subtilissime scabrum.

Lingua, plana, acuta, supra basin in medio gibba.

Oculi magni, laterales, iride fulvo-aurea.

Pinnae pectorales, acuminatae, radiorum XII.

Pinnae ventrales radiorum VI, primo brevior aculeato, triangulares, fere sub pectoralibus, paulove posterius, inter se membranula connexae, quae abdomini frenulo adhaeret.

Squama lanceolata, frenulo pinnarum ventralium utrinque accubans.

Pinna dorsalis dimidia dorsi longitudine, caudae proxima, ascendens basi subcarnosa, radiorum XIII in uno, XV in altero specimine, quorum IV priores maximi, aculeati, primo brevior, membranis intersectis, unde pinna antierius ferrata.

Pinna ani, dorsali opposita, basi carnosa, squamataque, seu carinae caudae productae insidens, radiorum XVIII, quorum tres priores aculeati.

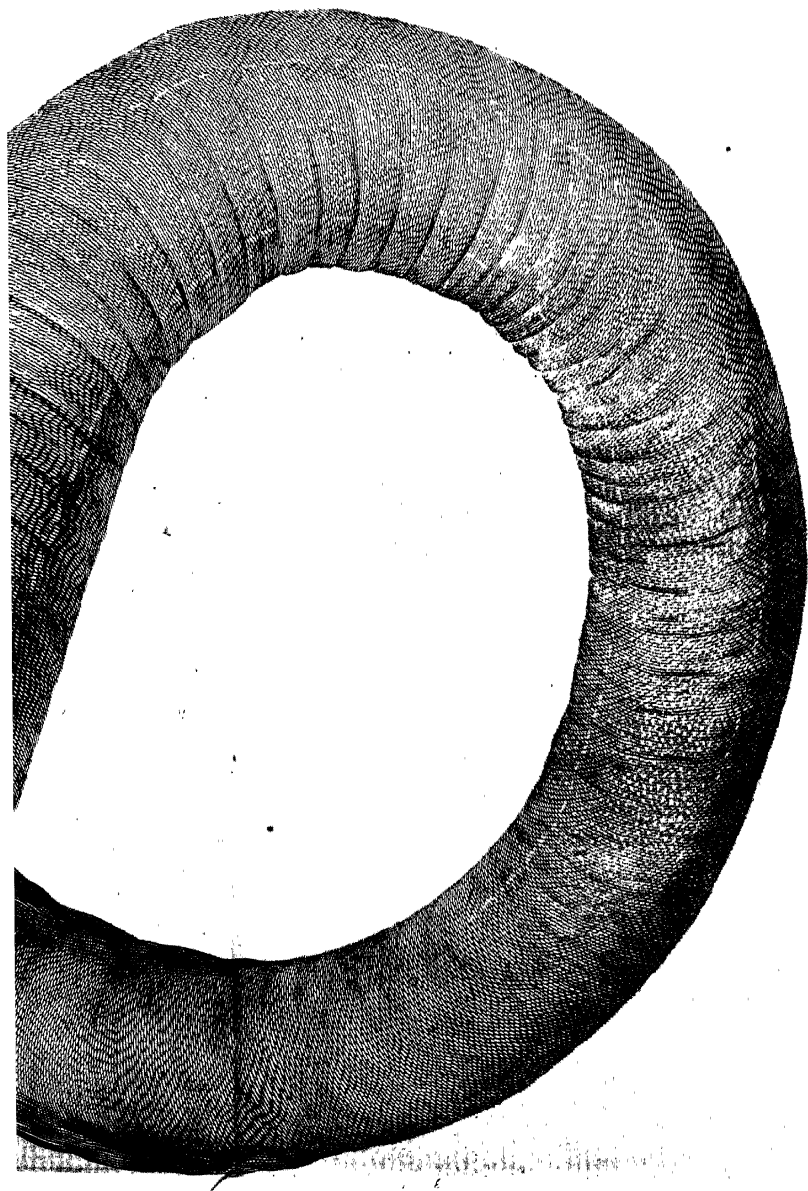
Pinna caudae subaequalis, radiorum XVII.

Color piscis aureus, in dorso fuscens; maculae fuscae, transversae, ad dorsum utrinque quinque, aequidistantes, prima ad tempora, ultima prope caudam.

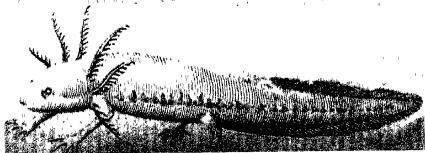


SIREN of *Linnaeus* or *Mud-Inguana* from *S. Carolina*





D





Received June 5, 1766.

XXII. *An Account of an Amphibious Bipes;*  
by John Ellis, Esq; F. R. S. To the  
*Royal Society.*

Read June 5, 1766. THESE two specimens of a remarkable kind of animal, which I have the honour to lay before this Royal Society, I received last summer from Dr. Alexander Garden, of Charles-town South Carolina, who says, it is evidently a new genus not yet taken notice of by naturalists, and that it appears to him, to come between the *Muraena* and the *Lacerta*.

The natives call it by the name of *Mud-Inguana*.

It is found in swampy and muddy places, by the sides of pools, under the trunks of old trees that hang over the water.

TAB. IX. The lesser one B, which is preserved in spirits, measures about nine inches in length; and appears to be a very young state of the animal, as we may observe from the fin of the tail and the opercula or coverings of the gills being not yet extended to their full size. These opercula, in their present state, consist each of three indented lobes, hiding the gills from view, and are placed just above the two feet. These feet appear like little arms and hands, each furnished with four fingers, and each finger with a claw.

In the specimen A, which is about 31 inches long, the head is something like an eel, but more compressed: the eyes are small and placed as those of the eel are, in this they are scarce visible: this smallness of the  
eye.

eye best suits an animal that lives so much in mud. The nostrils are very plainly to be distinguished; these, with the gills and the remarkable length of the lungs, shew it to be a true amphibious animal. The mouth is small in proportion to the body; but its palate and inside of the lower jaw (see Fig. C) are well provided with many rows of pointed teeth; with this provision of nature, added to the sharp exterior bony edges of both the upper and under jaw, the animal seems capable of biting and grinding the hardest kind of food. The skin, which is black, is full of small scales, resembling chagrin. These scales are of different sizes and shapes according to their situation, but all appear sunk into its gelatinous surface: those along the back and belly are of an oblong oval form, and close set together: in the other parts, they are round and more distinct. Both the sides are mottled with small white spots, and have two distinct lines composed of small white streaks, continued along from the feet to the tail. The fin of the tail has no rays, and is no more than an adipose membrane like that of the eel; this fin appears more distinctly in the dry animal than in those that have been preserved in spirits.

The opercula or coverings to the gills in dry specimens appear shrivelled up, but yet we may plainly see they have been doubly pennated. Under these coverings, are the openings to the gills, three on each side, agreeable to the number of the opercula. In the plate at Fig. F, the fins are represented as they appear when just taken out of the water and put into spirits of wine.

The form of these pennated coverings approach very near to what I have some time ago observed, in  
the

the larva or aquatic state of our English lacerta, known by the name of eft or newt (see Fig. D and E) which serve them for coverings to their gills, and for fins to swim with during this state; and which they lose, as well as the fin of their tails, when they change their state and become land animals; as I have observed by keeping them alive for some time myself.

Recollecting these observations on the changes of our lizard, and at the same time the many remarkable changes in frogs, I began to suspect whether this animal might not be the larva state of some large kind of lizard; and therefore requested the favour of Dr. Solander, to examine with me the lacertas in the British Museum; that we might see whether any of the young ones had only two feet; but, after carefully going through many kinds, we could plainly discover four feet perfectly formed, even in those that were just coming out of their eggs.

During this state of uncertainty, I forwarded to Dr. Linnæus of Upsal, at Dr. Garden's request, his account of the largest specimen, and, at the same time, sent him one of the smaller specimens preserved in spirits; desiring his opinion, for Dr. Garden's, as well as my own, satisfaction.

About the latter end of January last, I was favoured with an answer from the Professor, dated Upsal, December 27, 1765, wherein he says,

“ I received Dr. Garden's very rare two-footed animal with gills and lungs. The animal is probably the larva of some kind of lacerta, which I very much desire that he will particularly enquire into.  
 “ If it does not undergo a change, it belongs to the order of *Nantes*, which have both lungs and gills;  
 “ and



“ and if so, it must be a new and very distinct genus,  
 “ and should most properly have the name of *Siren*.

“ I cannot possibly describe to you how much this  
 “ two-footed animal has exercised my thoughts; if it  
 “ is a larva, he will no doubt find some of them with  
 “ four feet.

“ It is not an easy matter to reconcile it to the larva  
 “ of the lizard tribe, its fingers being furnished with  
 “ claws; all the larvas of lizards, that I know, are  
 “ without them (*digitis muticis*).

“ Then also the branchiæ or gills are not to be  
 “ met with in the aquatic salamanders, which are  
 “ probably the larvas of lizards.

“ Further, the croaking noise or sound it makes  
 “ does not agree with the larvas of these animals;  
 “ nor does the situation of the anus.

“ So that there is no creature that ever I saw, that I  
 “ long so much to be convinced of the truth, as what  
 “ this will certainly turn out to be.”

I am, with the greatest respect,  
 the Royal Society's  
 most obedient humble servant,

Gray's Inn,  
 June 5, 1766.

John Ellis.

P. S. In a letter lately received from Dr. Garden, he mentions one remarkable property in this animal, which is, that his servant endeavouring to kill one of them, by dashing it against the stones, it broke into three or four pieces: he further says, that he has had an opportunity of seeing many of them lately of a much larger size, and that he never saw one with more than two feet; so that he is fully convinced, that it is quite a new genus of the animal kingdom.

Received

Received May 28, 1766.

XXIII. *Observations upon Animals, commonly called Amphibious by Authors. Presented by Dr. Parsons, F. R. S.*

Read June 26, 1766. **T**HE following remarks, which I have the honour to lay before this learned Society, were occasioned by a conversation that passed between me and a gentleman well acquainted with natural history, however mistaken in the subject before us. His opinion was, that amphibious animals lived more in the water than on the land: but I believe the contrary will appear by the sequel of this treatise.

If we consider the words ἀμφι and βιῶν, from which the term amphibious is derived; we should understand that animals, having this title, should be capable of living as well by land or in the air, as by water, or of dwelling in either constantly at will; but it will be difficult to find any animal that can fulfil this definition, as being equally qualified for either; and in classing creatures of this kind, authors are much divided and sometimes mistaken.

Now if any natural historian should deduce his distinction of this class, from the structure or characteristic of any part of the animal, I think he would be a little out of the way; because the term comprehends nothing but what regards its living in both air and water at discretion; however, since the word

amphibious is adopted by the writers of the history of animals, let us retain it still, and examine some of this class, and, by considering their natural œconomy respectively, endeavour to range them according to that standard in the following manner. They are such as :

1. Enjoy their chief functions by land, but occasionally go into the water.

2. Such as chiefly inhabit the water, but occasionally go a shore. Of the latter there are but very few species. And although none of the winged tribe are to be ranged under this class, yet as many of them remain long upon the water, in search of their proper food, we shall enumerate some peculiar advantages, which have been allowed to several of them by the bountiful wisdom of the Creator, in order to render them the more able to obtain it; and this will make one curious part of my present purpose, not generally known.

The dispute mentioned between my friend and me, turned upon the class of the phocæ, which consists of a very numerous tribe of different species: I shall therefore endeavour to shew that none of them can live chiefly in the waters, but that their chief enjoyment of the functions of life is on shore.

These animals are really quadrupeds; but, as their chief food is fish, they are under a necessity of going out to sea to hunt their prey, and to great distances from shore; taking care that, how ever great the distance, rocks or small islands are at hand, as resting places when they are tired, or their bodies become too much macerated in the water; and they return to the places of their usual resort to sleep, copulate, and bring

bring forth their young, for the following reasons, viz. It is well known that the only essential difference (as to the general structure of the heart) between amphibious and meer land animals, or such as never go into the water, is that in the former the oval hole remains always open. Now, in such as are without this hole, if they were to be immersed in water for but a little time, respiration would cease, and the animal must die; because a great part of the mass of blood passes from the heart, by the pulmonary artery, through the lungs, and by the pulmonary veins returns to the heart; while the aorta is carrying the greater part of the mass to the head and extremities, &c.

Now the blood passes through the lungs in a continual uninterrupted stream, while respiration is gentle and moderate; but when it is violent, then the circulation is interrupted, for inspiration and expiration are now carried to their extent; and in this state the blood cannot pass through the lungs either during the total inspiration or total expiration of the air in breathing; for in the former case the inflation compresses the returning veins, and in the latter, by the collapse of the lungs, these veins are interrupted also, so that it is only between these two violent actions that the blood can pass: and hence it is that the lives of animals are shortened, and their health impaired, when they are subjected to frequent violent respiration; and thus it is that in animals who have once breathed, they must continue to respire ever after; for life is at an end when that ceases.

There are three necessary and principal uses of respiration in all land animals, and in these kinds that are counted amphibious; the first is that of promoting

the circulation of the blood through the whole body and extremities; in real fishes, the force of the heart is alone capable of sending the blood to every part, as they are not furnished with limbs or extremities; but in the others mentioned, being all furnished with extremities, respiration is an assistant force to the arteries in sending blood to the extremities, which, being so remote from the heart, have need of such assistance; otherwise the circulation would be very languid in these parts; thus we see, that in persons subject to asthmatic complaints, the circulation grows languid, the legs grow cold and oedematous, and other parts suffer by the defect in respiration.

A second use of breathing is that, in inspiration, the variety of particles of different qualities, which float always in the air, might be drawn into the lungs, to be insinuated into the mass of blood, being highly necessary to temperate and cool the agitated mass, and to contribute refined pabulum to the finer parts of it, which, meeting with the daily supply of chyle, serves to assimilate and more intimately mix the mass, and render its constitution the fitter for supporting the life of the animal. Therefore it is, that valetudinarians, by changing foul or unwholesome air for a free, good, open air, often recover from lingering diseases.

And a third principal use of respiration is, to promote the exhibition of a voice in animals; which all those that live on the land do according to their specific natures.

From these considerations it appears, beyond contradiction, that the phocæ of every kind are under an absolute necessity of making the land their principal residence; but there is another very convincing argument

argument why they reside on shore the greatest part of their time, and that is, that the flesh of these creatures is analogous to that of other land animals; and therefore, by over-long maceration, added to the fatigue of their chasing their prey, they would suffer such a relaxation as would destroy them. It is well known that animals, which have lain long under water, are reduced to a very lax and even putrid state; and the phoca must bask in the air on shore; for while the solids are at rest, they acquire their former degree of tension, and the vigour of the animal is restored; and while he has an uninterrupted placid respiration, his blood is refreshed by the new supply of air, as I have explained it above, and he is rendered fit for his next cruise: for action wastes the most exalted fluids of the body, more or less, according to its duration and violence; and the restorative rest must continue a longer or shorter time, according to the quantity of the previous fatigue.

Let us now examine by what power these animals are capable of remaining longer under water than land animals.

All these have the oval hole open, between the right and left auricles of the heart, and, in many, the canalis arteriosus also: and while the phoca remains under water, which he may continue an hour or two more or less, his respiration is stopped, and the blood, not finding the passage through the pulmonary artery free, rushes through the hole from the right to the left auricle, and partly through the arterial canal, being a short passage to the aorta, and thence to every part of the body, maintaining the circulation: but, upon rising

to come ashore, the blood finds its passage again through the lungs the moment he respire.

Thus the foetus in utero, during his confinement, having the lungs compressed, and consequently the pulmonary arteries and veins impervious, has the circulation of the blood carried on through the oval hole and the arterial canal; now so far the phoca in the water and the foetus in utero are analogous; but they differ in other material circumstances: one is, that the foetus, having never respired, remains sufficiently nourished by the maternal blood circulating through him, and continues to grow till the time of his birth, without any want of respiration during nine months confinement; the phoca, having respired the moment of his birth, cannot live very long without it, for the reasons given before; and this hole and canal would be closed in them, as it is in land animals, if the dam did not, very soon after the birth of the cub, carry him into the water to teach him, so very frequently; by which practice these passages are kept open during life; otherwise they would not be capable of attaining the food designed for them by providence.

Another difference is, that the phoca, as I said before, would be relaxed by maceration in remaining too long in the water; whereas the foetus in utero suffers no injury from continuing its full number of months in the fluid he swims in: the reason is; that water is a powerful solvent, and penetrates the pores of the skins of land animals, and in time can dissolve them; whereas the liquor amnii is an insipid soft fluid, impregnated with particles more or less mucilaginous, and utterly incapable of making the least alteration in the cutis of the foetus.

Otters, beavers, and some kinds of rats, go occasionally into the waters for their prey, but cannot remain very long under water; I have often gone to shoot otters, and watched all their motions; I have seen one of them go softly from a bank into the river, and dive down, and in about two minutes rise, at ten or fifteen yards from the place he went in, with a midling salmon in his mouth, which he brought on shore; I shot him, and saved the fish whole. Now, as all foetuses have these passages open, if a whelp of a true water-spaniel was, immediately after its birth, served as the phoca does her cubs, immersed in water, to stop respiration for a little time every day, I make no doubt but the hole and canal would be kept open, and the dog be made capable of remaining as long under water as the phoca.

Frogs, how capable soever of remaining in the water, yet cannot avoid living on land, for they respire; and if, as I have often done, a frog be thrown into a river, he makes to the shore as fast as he can.

The lizard kind, such as may be called water lizards, or *lacertæ aquaticæ*, all are obliged to come to land and deposit their eggs, rest, and sleep; even the crocodiles, who dwell much in rivers, sleep and lay their eggs on shore; and, while in the water, are compelled to rise to the surface to breathe; yet, from the texture of his scaly covering, he is capable of remaining in the water longer by far than any species of the phocæ, whose skin is analogous to that of a horse or cow.

The hippopotamus, who wades into the lakes or rivers, is a quadruped, and remains under the water a considerable



considerable time; yet his chief residence is upon land, and he must come on shore for respiration.

The testudo, or sea-tortoise, though he goes out to sea, and is often found far from land; yet, being a respiring animal, cannot remain long under water. He has indeed a power of rendering himself specifically heavier or lighter than the water, and therefore can let himself down to avoid an enemy or a storm; yet he is under a necessity of rising frequently to breathe, for reasons given before: and his most usual situation, while at sea, is upon the surface of the water, feeding upon the various substances that float in great abundance every where about him; these animals sleep securely upon the surface, but not under water, and can remain longer at sea than any others of this class, except the crocodile, because, as it is with the latter, his covering is not in danger of being too much macerated; yet they must go on shore to copulate and lay their eggs.

The consideration of these is sufficient to inform us of the nature of the first order of the class of amphibious animals; let us now see what is to be said of the second in our division of them, which are such as chiefly inhabit the waters, but occasionally go on shore.

These are but of two kinds: the eels and water-serpents, or snakes of every kind. It is their form that qualifies them for loco-motion on land, and they know their way back to the water at will; for by their structure they have a strong peristaltic motion, by which they can go forward at a pretty good rate, whereas, all other kinds of fish, whether vertical or horizontal, are incapable of a voluntary loco-motion on

shore ; and therefore, as soon as such fish are brought out of the water, after having flounced a while, they lye motionless, and soon die.

Let us now examine into the reason why these vermicular fish, the eel and serpent kinds, can live a considerable time on land, and the vertical and horizontal kinds die almost immediately, when taken out of the water : and, in this research, we shall come to know what analogy there is between land animals and those of the waters. All land animals have lungs, and can live no longer than while these are inflated by the ambient air, and alternately compressed for its expulsion ; that is, while respiration is duly carried on, by a regular inspiration and expiration of air.

In like manner, the fish in general have, instead of lungs, gills, or branchiæ ; and, as in land animals, the lungs have a large portion of the mass of blood circulating through them, which must be stopped if the air has not a free ingress and egress into and from them ; so, in fish, there is a great share of blood vessels that pass through the branchiæ, and a great portion of their blood circulates through them, which must in like manner be totally stopped, if the branchiæ are not kept perpetually wet with water ; so that, as the air is to the lungs, in land animals, a constant assistant to the circulation, so is the water to the branchiæ of those of the rivers and seas ; for when these are out of the water, the branchiæ very soon grow crisp and dry, the blood vessels are shrunk, and the blood is obstructed in its passage ; so, when the former are immersed in water, or otherwise prevented having respiration, the circulation ceases, and the animal dies.

Again, as land animals would be destroyed by too much maceration in water, so fishes would, on the other hand, be ruined by too much exsiccation; the latter being, from their general structure and constitution, made fit to bear, and live in, the water; the former, by their constitution and forms, to breathe, and dwell, in the air.

But it may be asked, why eels and water snakes are capable of living longer in the air than the other kinds of fish? this is answered, by considering the providential care of the great Creator for these and every one of his creatures: for, since they were capable of locomotion by their form, which they need not be if they were never to go on shore, it seemed necessary that they should be rendered capable of living a considerable time on shore, otherwise their locomotion would be vain. How is this provided for? why in a most convenient manner; for this order of fishes have their branchiæ well covered from the external drying air, and are also furnished with a slimy mucus, which hinders their becoming crisp and dry for many hours, and their very skins always emit a mucous liquor, which keeps them supple and moist for a long time; whereas the branchiæ of other kinds of fish are much exposed to the air, and want the slimy matter to keep them moist. Now, if, when any of these is brought out of the water, it was laid in a vessel without water, he might be kept alive a considerable time, by only keeping the gills and surface of the skin constantly wet, even without any water to swim in.

Before I dismiss the first part of my discourse, I must beg your patience, while I mention something that relates to a family among the fish kinds, which is  
of

of a middle nature between the phocæ, and the real fishes of the sea, in one peculiar respect. This is the class of the phocenæ, or porpeffes, of which there are several species; and these have lungs, and therefore are forced to come up to the surface to breathe at very short intervals; but, when brought on shore, have no progressive loco-motion. So that, having lungs, they resemble the phocæ, and, in every other respect, the real fishes of the sea.

Blasius, in his *Anatome Animalium*, page 288, gives an account of one of these taken and brought on shore alive; the people let him lye, to see how long he could live out of the water; and he continued alive only about seven or eight hours, and exhibited a kind of hissing voice.

From what has been said, it will, I hope, appear rational, that these are the only two orders, that can properly be deduced from the class of Amphibious animals; and that the genus's of either order are very few in the animal world.

Received May 28, 1766.

XXIV. *An Account of some peculiar Advantages in the Structure of the Asperæ Arteriæ, or Wind Pipes, of several Birds, and in the Land-Tortoise.*

Read June 9, 1766. **H**AVING, in my former discourse, given an account of some particular phænomena, in amphibious animals, which rendered them more happy and perfect in their animal œconomy towards their preservation; I shall now lay before this learned Society, certain advantages in some birds, towards assisting them in the acquisition of their food, which they seek for in the water; and some of these swim upon the water and dive down occasionally; others only wade into the water, in shallow places, as far as their long legs will carry them, without touching the water with their feathers, in search of their nourishment.

The natural history of four of them is very well set forth by authors; the other two are not mentioned, that I know of, but barely by their names; and, although the author has not described them, yet he knew the structures of their asperæ arteriæ, and was a person who made many observations in natural history, of whom I shall speak in his turn.

These birds are; the wild swan, colum, feras, crane, Indian cock, demoiselle.

The structure of the wind-pipe of all these is so singular and so little known, that I thought a proper notice of the subject would be agreeable to the Society, and very fit to be recorded together in the Transactions, if it be approved of (and will be a suitable sequel to those considerations upon amphibious animals); which I shall illustrate with drawings, to render their description the more intelligible.

The wild swan is somewhat smaller than the tame one; this inhabits fresh rivers in land, while the wild one always resorts great lakes and arms of the sea. These are two distinct species, the river swan, and the sea or wild swan; and yet it has been suggested, that the latter might become as tame and familiar as the others, if they were brought up young: and hence they were supposed to be the same. But this wonderful structure of the *aspera arteria* shews that they are different, for the river swan has it not; although a very modern author, who is certainly as well versed in natural history as any one whatever, has these remarkable words; "All the writers on birds; says he, have described the swan: they have called it *cygnus domesticus*, and *cygnus ferus*, distinguishing it in its wild and familiar state into two species, but this is idle and unnecessary: the bird is wholly the same in both."

It will be seen, however, by the description of the part mentioned, they cannot be the same species; for, besides this formation of the pipe, Mr. Edwards shews their heads to be very different also.

In the general run of birds, the *aspera arteria* is nearly strait; that is, having no plications, but descending directly from the epiglottis into the cavity of the  
body

body to lye upon the sternum, and terminating in the lungs; whereas, in these birds, which are the subjects of this discourse, they have certain turnings within the sternum or breast-bone, and run back again to double up into the thorax: which elongates them to double the length of those in other birds of equal, nay of greater magnitude, than the birds that have them.

In the wild swan, the wind-pipe runs down from its upper extremity under the epiglottis, in company with the oesophagus, till it comes within about four or five inches of the last vertebra of the neck; here the pipe quits the oesophagus, (which keeps its course to the intestines) and makes a convex curve forward between the ossa jugalia, in a circular sweep, till it enters into a hole formed through a strong membrane in the center between the insertions of the ossa jugalia into the sternum under the breast; and, in that circular sweep, is covered closely by the skin, so, that, in that place, a very slight blow would destroy the bird.

This hole is the beginning of a theca or cavity in the keel of the sternum, in which the pipe passes on to the end, and then returns back, forming a loop which is circular; and, passing out by another hole through the same strong membrane, makes another circular sweep within, and parallel to the exterior one, and then rises in that round direction, till it enters the cavity of the thorax, and is divided into two branchiæ, which terminate in the lungs.

When one views this structure, it is impossible to avoid being surprized at the wonderful formation of this part, especially too if we attend to the noble  
contri-

contrivance for securing these circular volutions of the pipe, from compressing one another, or from bending into angles; for, if this was the case, their long and free respiration could not be maintained, and the end, for which the pipe is so formed, would not be answered. An explanation of this contrivance will be necessary in this place: there is a strong membrane, which arises from all the clavicle, and is inserted all along the jugal bone on each side, very stiff like a drum; and as the aspera arteria makes its anterior volutions between the latter, (for the posterior turning is that loop within the keel of the sternum described,) it was necessary that the pipe should be supported by a stay in each circular ~~course~~ <sup>curve</sup>, to prevent the impediments just mentioned. Accordingly there are three strong transverse ligamentous membranes, running from one jugal drum to the other; over the outer of which, the pipe goes into the keel of the sternum through the under hole, and, in its return, rides over two others in a circular direction in its way to the thorax. These are the stays, which prevent its doubling back in an angle, in these two volutions; and in that within the theca, there was no need of such a fulcrum, being secured in its bed from any external pressure.

This wild swan was brought alive from Philadelphia, but died soon after its arrival; and I assisted in the dissection, and made these drawings from the prepared parts. I find no mention of this structure of the aspera arteria in the wild swan\*, but originally in

\* Mr. Edwards found it in the swan he describes. See his History of Birds.



Bartholin, who took delight in comparative anatomy, from whom Blasius has taken it.

It is difficult to say what may be the real use of this kind of wind-pipe in the several birds that have it, if it be not to procure them a longer retention of inspired air, (while they seek their food where they are obliged to remain some time immersed in water,) than if the pipe was strait, as in geese, ducks, and such like; for these and the river swan often dive down to feed, yet it is always in shallow places, and their continuance under water is very short; whereas the wild swan dwells upon and seeks his food in great lakes, and arms of the sea; and dives into deeper waters, and consequently requires a power of continuing longer without respiration than the others.

Poets and natural historians in great numbers have asserted that these birds sing very harmoniously; and this gave occasion to a friend of mine, to whom I shewed these drawings, to surmise that this structure might be of use to them in singing; but I never found any one who would say they ever heard either wild or river swans sing: and therefore I doubt it much. But, if they do sing, the length of the pipe contributes nothing towards it; it is the glottis, which forms the voice, and modulates it, whether the pipe be long or short: besides, none of the song, or speaking-birds, have any flexion in their pipes, that we are acquainted with.

The crane is the next I shall take occasion to mention, which has such a turning of the *aspera arteria* in the keel of the sternum; but the volution of this bird is round within the bone, and may be compared to that of a French-horn; whereas that of the wild  
swan

swan is strait within the bone, and may be compared to a trumpet; yet the entrance of this into the sternum, and its exit, and its passage into the cavity of the thorax, are similar to those of the swan.

This is a bird who cannot go upon the water, being no more capable of swimming than a common cock or hen. His feathers will not admit of it; and, having no webs to his toes, it is unable to swim. It is somewhat surprising that not one of the tribes which are similar to the crane, such as the herons, storks, bitterns, &c. has any such structure of the aspera arteria; and yet they all feed upon fish or water insects. We are, however, to consider, that the heron chiefly haunts brooks, springs, and the narrow heads of rivers, where he seeks his food, and finds it with ease: but the crane is under a necessity of immersing its head, and remains a considerable space in that situation upon strands and marshes: it is also a bird of congress; for, at certain seasons, a multitude of cranes flock together and rise upon the wing to a great height in the air, being birds of passage, and they are by many authors said to travel from most parts of Scythia to Egypt, where, for a certain season, they remain about the Nile, and the great lakes of that country. Perhaps this elongation of the windpipe in them may be also of use, in their great flight through various degrees of rarified or condensed air, in the variety of climates through which they pass.

The Indian cock, *Gallus Indicus* of Aldrovand and Longalius, and *Gallus Persicus* of Johnston, the Mutu Poranga of Margrave, is not the Coq d'Inde, or Turkey cock, but by the last author ranged among the pheasant tribe; this bird has a plication of the aspera

arteria, but not so considerable as either of the fore-mentioned, swan or crane: for it descends in a straight line, along with the oesophagus, to the middle of the jugal bone, without and above the thorax, where it is spread and fastened on each side. Then, turning backwards, being somewhat flat, it makes a fold upward to about an inch and half high, and there being made fast again, by a strong membrane, it doubles down and passes into the thorax, terminating by two bronchia in the lungs: and where it is fastened and folded, that is, in the flat parts, it is triple the circumference of any other part of the pipe. This bird and another of the same species were dissected by the Royal Academy of Sciences, and this structure of the pipe appeared in both; for which it is difficult to assign a reason in any of the pheasant kinds, if it be not to retain inspired air longer than ordinary upon some occasions, though they are not frequenters of rivers or marshy grounds; which one might reasonably suggest from the great capacity in the plicated parts of the wind-pipe.

The next I shall mention is the *Grus Numidica*, Numidian crane, or Demoiselle.

This bird has also a plication in the wind-pipe, which was likewise dissected by the Academy of Sciences, in whose account the natural history of it may be seen; and a true description and figure of it from the life, by Mr. Edwards, in his Natural history of birds. I confine myself only to the configuration of this part, in as many animals as I can find endowed with such a structure, in order to collect them and lay them here in view: and shall hereafter make farther researches and dissections, in such as I may reasonably suppose

suppose to have a different formation from the common standard of a strait wind-pipe.

In this bird, being of the crane kind, the pipe runs down in company with the oesophagus, to about a foot in length, and then turns outwards and forwards, as it does in the swan and crane, and enters into the keel of the sternum, which, like the others, forms a bony box for its reception, through a ligamentous hole for about three inches: then it returns upwards, and a round turn into the thorax terminating in bronchia and lungs.

Now, in the Indian cock mentioned, the plication is made above the sternum, in a roomy part between the jugal bones; whereas, in the others mentioned, the plication is within the keel of the sternum.

The other birds I find any account of, having the aspera arteria folded, are only two, and of these our information is very short.

In Dr. Fryers's account of India and Persia, where he treats of his description of Surat and his journey into Duccan, page 119, I find the following passage: "Fifth, oysters, soles, and Indian  
 " mackerel, the river yields very good; and the pools  
 " and lakes store of wild fowl; particularly brand  
 " geese, colum, and serafs, a species of the former;  
 " in the cold weather, they, shunning the northern  
 " rigid blasts, come yearly hither from mount Cauca-  
 " sus; what is worth taking notice of, is their aspera  
 " arteria wound up in a case on both sides their breast-  
 " bone, in manner of a trumpet, such as our waits  
 " use: when it is single, it is a serafs; when double,  
 " it is a colum, making a greater noise than a

“ bittern, being heard a great while before they can be seen, flying in armies in the air.”

From this passage, it is plain our author had examined the interior parts of this colum and serafs, and that they are different species of the same genus. We can only, however, endeavour to find what this genus is; and, by what we have heard of the crane, it is not improbable that they are of that kind. The crane, by every author, is said to take long flights in vast multitudes; and to make a great noise in the air. The colum and serafs are said to come to the rivers and lakes about Surat or Duccan from Caucasus, flying in armies, and making so great a noise, that they are heard a great while before they are seen. Again, by his short account, we may easily learn, that they are not the crane that is described in this paper, because the colum has a volution of the wind-pipe on both sides of the keel of the sternum, the crane but one; and I suppose, from the likenefs of the serafs to the colum, he says, that the former is a species of the latter; nor can we have room to suggest that these birds are of the wild goose kind; because he mentions the brand geese first, without taking the least notice of their aspera arteria; and confines the rest of the paragraph to the other two, calling the one a species of the other.

These are all the birds that have hitherto come to my notice, having this remarkable flexion in the aspera arteria; it now only remains to mention farther that of the land tortoise, which I bring in here on account of a similar formation of that organ in him.

Having never dissected a land tortoise myself, I must have recourse to those that have; and accordingly I find

find its parts examined by a celebrated physician of his time, Velchfius, from whom Blafius has taken it, and by the Academy of Sciences. Blafius has, however, made the anatomical distribution of its parts and their explanation; from whom I shall take the figure in a somewhat larger scale, for its better illustration; and although this last author quotes Severinus and Coiterus as difsectors of the tortoise, as well as Velchfius, yet this latter only mentions the wind-pipe.

In Blafius's figure, this pipe, for a few inches from the epiglottis, is single, but soon divides into two; and as it descends in company with the oesophagus, it forms a folded ring outwards on each side, and turns down again to enter the lungs: so that the animal has the advantage of a double aspera arteria, with a volution in each: which shews that this provision is intended to contain a greater portion of air than ordinary while he is under ground in winter.

The tortoise difsected by the Academy of Sciences, was a large land tortoise from the coast of Coromandel; in which was found a bifid wind-pipe; each branch is said to be six inches long, but no mention is made of the volutions in this land tortoise; which, one would think, being so remarkable a variety from the common standard, ought not to escape the notice of such able anatomists. Yet it was found, by several experiments, that respiration was very slow and unequal in this animal, as well as in the camelion; the Academicians observed several tortoises for a long time together, and have taken notice, that they sometimes cast forth a cold breath through the nostrils, but that is by long intervals and without order; and that the camelion is  
some-

sometimes half a day without one's being able to discern in him any motion for the respiration.

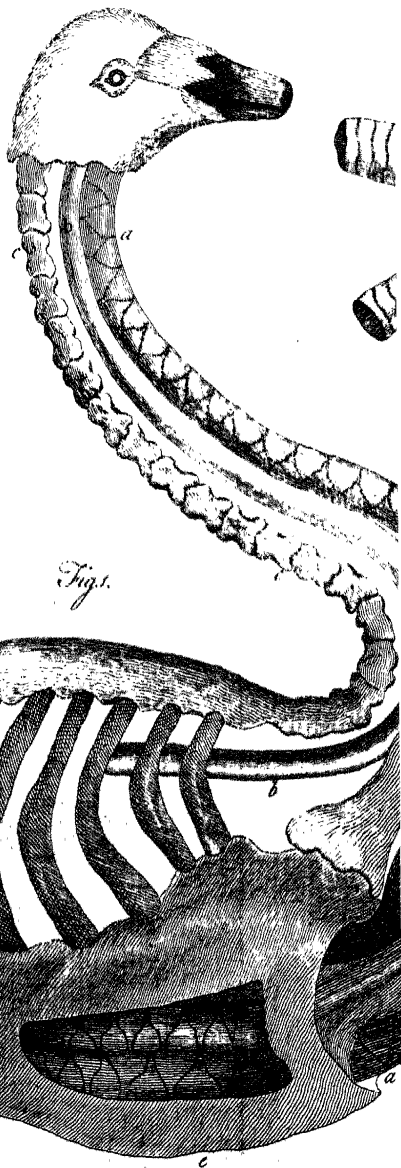
From this it is easily seen, that they can retain inspired air a long time : and the Academicians therefore think, that the principal use of the lungs in tortoises, is to render themselves specifically lighter or heavier in the water, by their inflation and compression at will, as fishes do by their swimming bladders ; indeed such a power of long inspiration seems to be as necessary in the land tortoise as in that of the sea ; because, in many countries where they breed, they are known to go into the ground and lye concealed for several months ; and it is well known, that several species of land tortoises go into ponds or canals in gardens, where they are kept, and remain long under water at pleasure. Of this my worthy friend Mr. Collinson had instances in his gardens at his country seat : and I saw two land tortoises in the bottom of a circular canal, in the gardens of the Palais Royal in Paris several times, which were very large ones, and remained under water many hours together.

The ingenious Academicians, however, in order to verify their sentiments, that one principal use of the lungs in a tortoise is to render them capable of remaining at any depth in the water at will, made the following experiment : they locked up a living tortoise in a vessel of water intirely full ; on which there was a cover exactly fastened with wax, from which there went a glass pipe : the vessel being full, so as to make the water appear at the bottom of the glass pipe ; they observed the water did sometimes ascend into the pipe, and that it sometimes descended. Now this could be done only by the augmentation and diminution





- a. The Aspera Arteria.
- b. The Oesophagus.
- c. Vertebrae of the Neck.
- d. The osia Jugalia.
- e. The Keel of the Sternum.
- f. The Membranes that Support the Aspera Arteria.



The Aspera Arteria of the Indian Cock, this Plication is made between the Jugal Bones & not in the Breast Bone, as are the others mention'd, & then the End, with the Bronchia, enters the Thorax and Lies upon the Sternum.

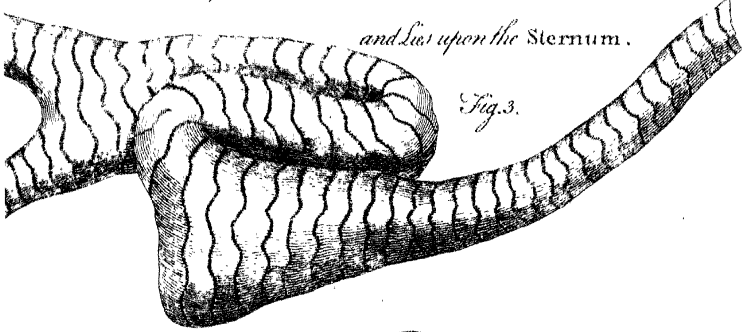


Fig. 3.

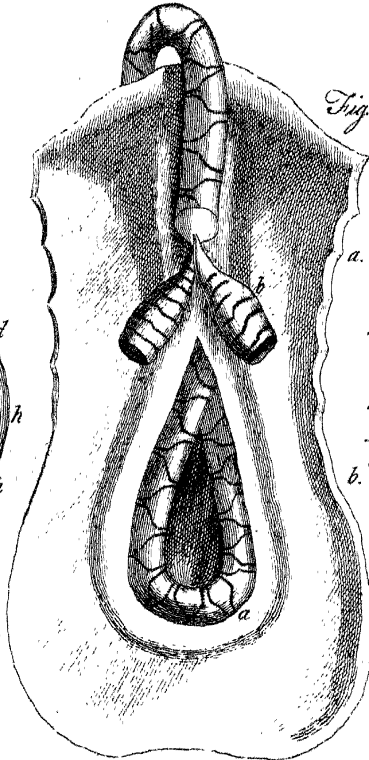
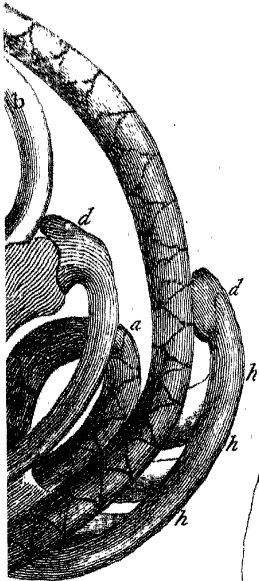
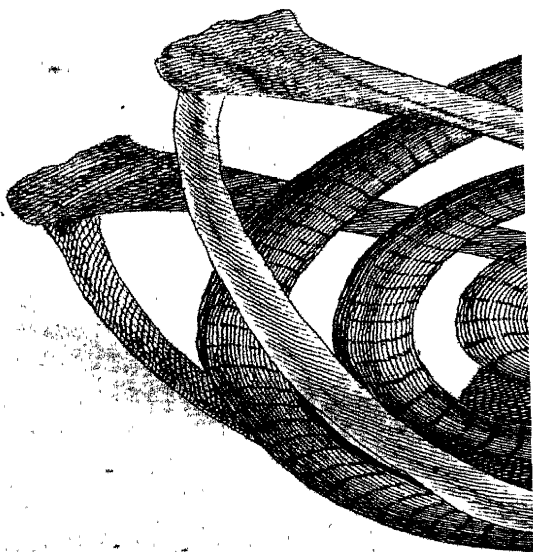


Fig. 2.

a. The Aspera Arteria passing in the Theca, doubling round to lay upon the Sternum, ending in  
b. The Bronchia.

*The Course of the Aspera*



Arteria in the Crane.

The Arteria of the Namidian Crane  
or Demasfelle. This Vein is made within  
the Keel of the Sternum.

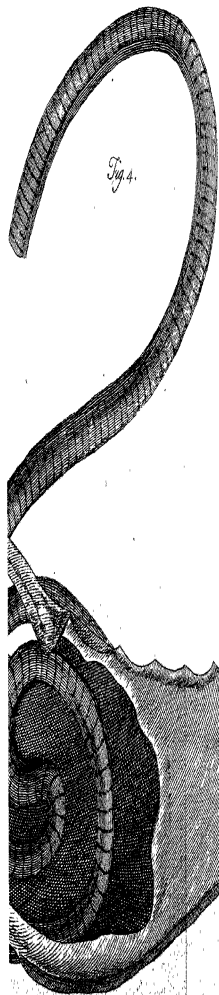


Fig. 4.

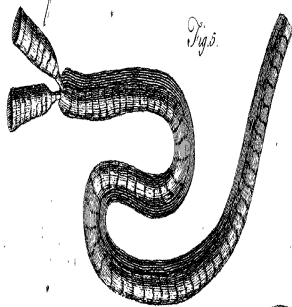


Fig. 5.

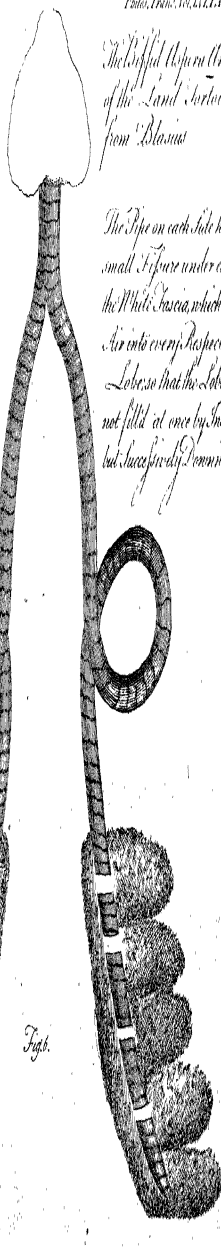


Fig. 6.

The Arteria of the Land Tortoise  
from Blasia

The Pipe on each Side has a  
small Figure under each of  
the White Arteria which admits  
Air into every respective  
Lobe, so that the Lobes are  
not fill'd at once by Inspiration,  
but successively from above.



nution of the bulk of the tortoise; and it is probable that when the tortoise endeavoured to sink to the bottom, the water fell in the pipe, because the animal lessened its bulk by the contraction of its muscles; and that the water rose by the slackening of the muscles, which, ceasing to compress the lungs, permitted it to return to its first size, and rendered the whole body of the tortoise lighter. I have, in many kinds of fish, dissected their swimming bladders, and found that in great and small these are vested with a strong muscular membrane, which they are capable of contracting and dilating at will, whereby they are able to compress or expand the column of air within very considerably; this bears some analogy to the detrusor muscle of the human urinary bladder, in contracting itself for the expulsion of the urine.

TAB. X. Fig. 1. Represents the situation and inclosure of the aspera arteria of the wild swan, in a lateral view.

2. A view of the same, with its progress within the thorax upon the sternum.

3. The aspera arteria of the Indian cock.

Tab. XI. Fig. 4. A lateral view of the aspera arteria of a crane, as situated in the cavity formed in the sternum.

5. Shews the flexion of the aspera arteria of the Numidian crane.

6. The bifid aspera arteria of the land tortoise.

Received June 6, 1766.

XXV. *A Letter from Mr. William Mountayne, F. R. S. to the Right Honourable James Earl of Morton, President of the Royal Society, containing some Observations on the Variation of the Magnetic Needle, made on Board the Montagu Man of War, in the Years 1760, 1761, and 1762, by Mr. David Ross, Surgeon.*

My Lord,

Read June 13, 1766. **T**HE following tables I have compared with the Variation Chart, published in the year 1756, and do find that they agree pretty well in general, making allowance for the time elapsed\*: it is true, that, in some few places in the Atlantic Ocean, they differ; yet this may probably arise, as is often the case, from an error in the Montagu's supposed longitude, where such observations were made. But the greatest difference (a greater than should arise, I think, according to common course) appears upon the coast of Portugal, Cape Saint Vincent, and about Gibraltar, near and within

\* By this expression, I do not mean that the variation undergoes a regular and uniform alteration, notwithstanding such appearance, but rather suspect the contrary.

fight of land, where the observations are ascertained to the spot. Hence, if mine observed about the year 1756, and those of Mr. Rois's, were both near the truth, at the respective times when\* they were taken, I know not how to account for this considerable encrease, unless those late extraordinary convulsions, in the bowels of the earth, upon those several coasts, may be found, by further experiments, to have there influenced the directions of the magnetic needle.

When these observations first came to my hands, I intended to employ them, among others, in the construction of a variation chart, at some proper period of time; but, by reason of a long and severe indisposition, I am obliged to suspend those my intentions. As, therefore, they appear to be made by a judicious and accurate observer, and, as Mr. Rois remarks, upon some local attractions in the West-Indies, mentioned at the close of his letter (which, without doubt, obtains in many other places) may induce some able philosopher, skilled in these matters, to investigate the cause of those phænomena; I beg leave to address the whole to your lordship, to be disposed of, as you shall think proper. I am,

My Lord,

Your Lordship's most obedient servant,

Gainsford-street, South-  
wark, May 18, 1766.

W. Mountaine.



*Extract of a Letter from Mr. David Ross, Surgeon of His Majesty's Ship the Montagu, to Mr. William Mountaine, F. R. S. relating to the Variation of the Magnetic Needle; with a Sett of Observations made by himself, on Board the said Ship, during the Years 1760, 1761, and 1762.*

N<sup>o</sup> 6, in Virginia-Street, April 9, 1763.

S I R,

**R**EPEATED observations of the variation of the magnetic needle, when the spot they are taken upon can be tolerably well ascertained, are of great service to philosophy in general, but particularly to navigation, as in future ages they may serve as a basis, to found its theory upon.

Having had an opportunity of observing, with all the care and accuracy in my power, the deflection of the magnetic needle, by an azimuth compass of Dr. Gowen Knight's construction, during a passage to the West-India Islands, a cruise there, and return to Britain; and likewise a cruise about the Straits of Gibraltar, I with pleasure communicate them to you, as a person deserving the highest esteem of the commercial part of Britain, by your excellent new edition of the Variation Chart of the late celebrated Dr. Halley; and I beg, Sir, your favourable acceptance of them.

I can assure you, I have endeavoured, as far as the thing would admit of, to get at the truth, having, at every observation, not trusted to a single azimuth,  
but

but generally have taken a medium of, from six to ten, or more.

I cannot help taking notice of what surprized me greatly, viz. that I should always find the variation less at anchor, in the West-India Islands, than at sea, though near the same spot. As for instance, June 18, 1760, three leagues south Prince Rupert's Bay, Dominico, I found the variation, by a medium of five azimuths and an amplitude, to be  $5^{\circ} 27'$  E. And in the Bay itself, June 20, by three azimuths and an amplitude, it was  $3^{\circ} 27'$ ; and on the 22d, by six azimuths and amplitude =  $3^{\circ} 12'$  E. The medium of these is  $3^{\circ} 20'$  E. full  $2^{\circ} 7'$  less than in the offing, though but nine miles off.

The same phenomenon appeared, upon experiments repeated March the 23d, 1761, when in the same Bay, the medium of eight azimuths gave  $3^{\circ} 19'$  E. Off of Antigua, and at anchor in Saint John's Road, the same thing was observed, though the difference was not so great. These things exercised my thoughts, though I must own I could not hit on any solution that pleased myself. I am,

S I R,

with great truth,

your very humble servant,

David Ross.

A TABLE of the Variations of the Magnetic Needle, as observed on Board His Majesty's Ship Montagu, in the Years 1760, 1761, and 1762, by Mr. David Ross; and communicated by Mr. William Mountaine, F. R. S.

Time when.	Latitude	Long. fr. Lon.	Variation.
1760 April 12	Off Bell-Isle and Grois.		19° 11' W.
	° ' N.	° ' W.	
26	43 23 N.	11 15 W.	18 51 W.
29	40 20 N.	13 39 W.	19 11 W.
May 2	40 18 N.	18 36 W.	17 0 W.
5	34 44 N.	21 42 W.	14 30 W.
6	31 38 N.	22 25 W.	14 15 W.
7	30 7 N.	23 12 W.	13 50 W.
9	28 51 N.	25 39 W.	10 4 W.
10	27 2 N.	28 40 W.	10 30 W.
11	26 10 N.	30 30 W.	10 16 W.
12	25 9 N.	32 37 W.	7 10 W.
13	24 3 N.	34 14 W.	5 59 W.
14	22 51 N.	35 51 W.	5 0 W.
15	21 40 N.	37 33 W.	4 20 W.
17	19 8 N.	41 27 W.	1 30 W.
18	18 4 N.	43 18 W.	1 0 W.
19	16 58 N.	45 9 W.	0 20 W.
20	16 7 N.	46 50 W.	0 30 E.
21	14 52 N.	48 39 W.	1 7 E.
22	13 38 N.	50 28 W.	1 59 E.
23	13 13 N.	51 48 W.	2 12 E.
25	13 30 N.	56 5 W.	3 45 E.
26	13 10 N.	57 50 W.	3 58 E.

Time when.			Variat.
*60	May 28, } 29, and 31 }	In Carlisle, Barbadoes	4 3E.
	June 1	Barbadoes S. E. 10 $\frac{1}{2}$ leagues dist.	5 20E.
	2	15° 19' N. 59° 0' W.	5 12E.
	3	16 16 N. 59 18 W.	4 24E.
	4	Antigua W. N. W. dist. 6 leagues	4 51E.
	6	Redondo E. $\frac{1}{2}$ N. 4 leagues.	4 50E.
	8	Montserrat S. by E. } Redondo S. by W. }	5 32E.
	15	In the pas. bet. Martinique and Dominico	5 6E.
	16	Off the S. W. end of Martinique	5 41E.
	17	Off the S. end of Dominico	5 21E.
	18	S. of Rupert's Bay, Dominico, dist. 3 leag.	5 27E.
	20	In Prince Rupert's Bay — 3° 27'	} 3 20E.
	22	Dominico — 3 12 }	
	July 11	Defeada S. W. by W. $\frac{1}{2}$ W. dist. 3 leag.	5 3E.
	12	15° 0' N. 59° 7' W.	5 35E.
	September 15	In Grand Courland's Bay	} 4 32E.
	16	At the South West end of Tobago,	
	18	the medium is	
	October 5	N. end of Tobago E. N. E. $\frac{1}{2}$ E. dist. 5 leag.	4 45 E.
	6	D° E. by S. dist. 4 leagues	5 0E.
	10	Island Bequia N. W. by N. $\frac{1}{2}$ W. dist. 7 leag.	4 0E.
	14	Off the N. W. end of Saint Lucia	5 14E.
	15	Off Saint Pierre's Martinico	5 17E.
	22	At the wind of the Channel between } Martinique and Dominico }	5 19E.
	23	At the East end of the same	5 36E.
	24	Saint Lucia W. $\frac{1}{2}$ N. dist. 15. leagues	5 34E.
1761	March 23	In Prince Rupert's Bay, Dominico	3 19E.
	April 4	In St. John's Road, Antigua	3 9E.
	15	Off the east end of Antigua	4 31E.
	May 4	In Carlisle Bay, Barbadoes	3 47E.
	July 27	At the Dog and Prickly Pear	4 44E.

Time when.		Latitude in		Long. fr. Lon.		Variation.	
1761	July 28	19	35 N.	62	50 W.	4	35 E.
	29	20	46 N.	62	43 W.	4	27 E.
	30	22	20 N.	62	30 W.	2	42 E.
August	31	23	42 N.	62	36 W.	2	15 E.
	1	25	5 N.	62	37 W.	1	38 E.
	2	26	20 N.	62	57 W.	1	18 E.
	3	27	26 N.	62	37 W.	0	44 E.
	4	27	56 N.	62	43 W.	0	3 E.
	5	28	5 N.	62	50 W.	0	23 E.
	6	28	11 N.	62	35 W.	0	1 E.
	7	28	40 N.	62	59 W.	0	27 W.
	8	29	23 N.	63	1 W.	0	48 W.
	9	30	1 N.	62	59 W.	1	33 W.
	10	31	1 N.	62	21 W.	1	58 W.
	11	32	41 N.	60 <sup>n</sup>	37 W.	2	45 W.
	12	33	58 N.	59	14 W.	4	13 W.
	13	35	3 N.	57	54 W.	5	0 W.
	15	36	31 N.	54	57 W.	7	4 W.
	16	37	33 N.	52	55 W.	8	20 W.
	17	37	59 N.	51	4 W.	9	40 W.
	18	38	8 N.	50	13 W.	10	20 W.
	19	38	36 N.	48	41 W.	10	12 W.
	20	38	23 N.	47	48 W.	10	57 W.
	21	38	57 N.	46	30 W.	11	52 W.
	22	39	22 N.	45	52 W.	12	0 W.
	23	40	13 N.	44	29 W.	12	0 W.
	24	41	8 N.	42	26 W.	12	0 W.
	26	41	31 N.	41	42 W.	13	0 W.
	27	42	0 N.	39	58 W.	13	35 W.
	28	42	38 N.	38	11 W.	13	36 W.
29	43	15 N.	36	21 W.	14	0 W.	
30	43	0 N.	35	53 W.	14	30 W.	
31	43	28 N.	34	40 W.	14	30 W.	
September	1	44	45 N.	31	58 W.	17	11 W.
	2	45	36 N.	29	49 W.	18	51 W.
	4	48	14 N.	25	17 W.	18	31 W.
	5	48	57 N.	23	13 W.	19	37 W.
	6	49	11 N.	20	28 W.	19	30 W.
	7	50	35 N.	18	17 W.	19	52 W.

Time when.		Variation.
1761 Sept. 10	Off Cape Clear in Ireland	19 55 W.
20	Scilly S. S. E. dist. 5 leagues	19 55 W.
21	The Start E. $\frac{1}{2}$ N. dist. 16 miles	20 16 W.
December 8	36° 25' N. 7° 45' W.	16 53 W.
19, 25, and 26	In Gibraltar Bay	17 11 W.
1762 Jan. 23	35° 46' N. 7° 2' W.	18 3 W.
Jan. and Feb.	Between Cape Saint Vincent and the Straits Mouth	17 31 W.
February 11	in Cascais Road	17 32 W.
D° 12, 23, and } March 2 }	In Lisbon River	17 32 W.
5	37° 56' N. 9° 55' W.	17 29 W.
	Between Cape Saint Mary's and Cape Spartel, the medium is	17 15 W.
From 3 to 12 leag. } E. off Gibr. Hill }	on the { European Shore Barbary Shore	17 20 W. 18 30 W.

Time when.	Latitude in	Long. fr. Lon.	Variation.
1761 July 28	19 35 N.	62 50 W.	0 35 E.
29	20 46 N.	62 43 W.	4 27 E.
30	22 20 N.	62 30 W.	2 42 E.
August 31	23 42 N.	62 36 W.	2 15 E.
1	25 5 N.	62 37 W.	38 E.
2	26 20 N.	62 57 W.	1 18 E.
3	27 26 N.	62 37 W.	0 44 E.
4	27 56 N.	62 43 W.	0 3 E.
5	28 5 N.	62 50 W.	0 23 E.
6	28 11 N.	62 35 W.	0 1 E.
7	28 40 N.	62 59 W.	0 27 W.
8	29 23 N.	63 1 W.	0 48 W.
9	30 1 N.	62 59 W.	1 33 W.
10	31 1 N.	62 21 W.	1 58 W.
11	32 41 N.	60 37 W.	2 45 W.
12	33 58 N.	59 14 W.	4 13 W.
13	35 3 N.	57 54 W.	5 0 W.
15	36 31 N.	54 57 W.	7 4 W.
16	37 33 N.	52 55 W.	8 20 W.
17	37 59 N.	51 4 W.	9 40 W.
18	38 8 N.	50 13 W.	10 20 W.
19	38 36 N.	48 41 W.	10 12 W.
20	38 23 N.	47 48 W.	10 57 W.
21	38 57 N.	46 30 W.	11 52 W.
22	39 22 N.	45 52 W.	12 0 W.
23	40 13 N.	44 29 W.	12 0 W.
24	41 8 N.	42 26 W.	12 0 W.
26	41 31 N.	41 42 W.	13 0 W.
27	42 0 N.	39 58 W.	13 35 W.
28	42 38 N.	38 11 W.	13 36 W.
29	43 15 N.	36 21 W.	14 0 W.
30	43 0 N.	35 53 W.	14 30 W.
September 31	43 28 N.	34 40 W.	14 30 W.
1	44 45 N.	31 58 W.	17 11 W.
2	45 36 N.	29 49 W.	18 51 W.
4	48 14 N.	25 17 W.	18 31 W.
5	48 57 N.	23 13 W.	19 37 W.
6	49 11 N.	20 28 W.	19 30 W.
7	50 35 N.	18 17 W.	19 52 W.

Time when.		Variation.
1761 Sept. 10	Off Cape Clear in Ireland	19 55' W.
20	Scilly S. S. E. dist. 5 leagues	19 55' W.
21	The Start E. $\frac{1}{2}$ N. dist. 16 miles	20 16' W.
December 8	36° 25' N. 7° 45' W.	16 53' W.
19, 25, and 26	In Gibraltar Bay	17 11' W.
1762 Jan. 23	35° 46' N. 7° 2' W.	18 3' W.
Jan. and Feb.	Between Cape Saint Vincent and the Straits Mouth	17 31' W.
February 11	In Cascais Road	17 32' W.
D° 12, 23, and March 2	In Lisbon River	17 32' W.
5	37° 56' N. 9° 55' W.	17 29' W.
	Between Cape Saint Mary's and Cape Spartel, the medium is	17 15' W.
	From 3 to 12 leag. } on the { European Shore	17 20' W.
	E. off Gibral. Hill } { Barbary Shore	18 30' W.



Received June 5, 1766.

XXVI. *A Letter to the President of the Royal Society, containing a new Manner of measuring the Velocity of Wind, and an Experiment to ascertain to what Quantity of Water a Fall of Snow is equal.*

Kirknewton, May 13, 1766.

My Lord,

Read June 19, 1766. **I** Should think myself most unworthy of the honour which your lordship and the Royal Society have done me, if the notice which you was pleased to take of my letter upon the late comet, did not make me more careful to observe whatever, I thought, might tend to improve the knowledge of nature, which is a capital part of the laudable design of the Society.

Your lordship knows, that my situation exposes me to every blast that blows, and affords a fair opportunity for measuring the velocity of the wind (the force of which I am, so often, obliged to feel). I have attempted to determine this by letting light downy feathers fly in the wind (the method, I understand, used by the ingenious Dr. Derham); but cannot say, in all the trials I have made (though I have let fifty of these feathers fly, one after the other, at a time), that I have ever seen above one, or two at most, upon which I could have founded a calculation. The velocity of the wind near the earth is very unequal, upon account of  
the

the frequent interruptions it meets with from hills, trees, and houses; and even in open plains, the surface of the earth, though much smoother than it commonly is, must reflect, and interrupt such a fluid as the air, and occasion great irregularity in the velocity of its current: this is the reason, when a feather is let fly with the wind, why it seldom, if ever, describes a straight line, but moves sometimes in a kind of spiral, now high, and then low, sometimes to the right, and then again to the left; and why two feathers let fly at once, seldom, if ever, keep together, or describe similar lines.

But, at some considerable distance from the earth, the velocity of the wind seems to be regular and steady: nothing can be more uniform, than the velocity of a cloud in the sky appears to be, even in the greatest storm: it is like a ship carried away insensibly by a smooth and gentle current, passing over equal spaces in equal times. This suggested the thought, that the motion of a cloud, or its shadow, over the surface of the earth, would be a much more proper measure of the velocity of the wind.

In the end of March 1763, I had as favourable an opportunity of putting this method into practice, as I could have wished for; the storm was exceeding high, and moved with vast velocity; the sun was bright, the sky clear, except where it was spotted with light floating clouds; I took my station in the north window of my dining room, near the clock, from which I had a free prospect of the fields; the sun was in the meridian, the wind due west intersecting his rays at right angles; I waited until the fore-part of the shadow of a cloud, that was distinct, and well

defined, just touched a south and north line, which I had marked upon the ground; at that instant I began my reckoning, and followed the shadow with my eye in its progress, counting seconds all the while by the clock, until I had reckoned up 15 seconds; then I observed exactly where the foresaid edge of the shadow was. This experiment I repeated ten times in half an hour, and seldom found the difference of a second, in the time which different clouds took to move over the same space. On the 5th of May current, I repeated the trial four different times, the sun being also near the meridian, the wind in the west, with light clouds floating in a clear sky as formerly; and found that the shadows of different clouds took some of them 44, and others 45 seconds, to pass over the same space which they had moved over in 15 seconds, in the former trials.

	Feet.	
This space measures exactly	1384	= space passed over in 15 seconds,
which multiplied by	4	
gives	5536	= space passed over in one minute,
which multiplied by	60	
gives	332,160	= space passed over in one hour.

Which space is = 62.9 English miles per hour, the velocity of the wind in March 1763.

One third of this (or 21 miles nearly) shews the velocity of the wind on May the 6th, when it blew a fresh gale.

This day, May 12, there was a small westerly breeze, the velocity of which I measured upon the same line, the sun being 10 minutes past the meridian, and found that the shadow took 9.5 seconds to pass over the  
above

above space, which gives the velocity of the wind at the rate of 9.9 English miles per hour.

Thus, by having several lines in different directions of a known length marked upon the ground, one may easily (and with great accuracy, I imagine), measure the velocity of the wind. If a person was provided with an instrument for measuring the force of the wind, it would perhaps be worth while to observe, whether, when the velocities of different winds were the same, (or nearly so) the forces of these winds did not vary with the seasons of the year, the points of the compass from which the wind blows, and also with the different state of the barometer and thermometer, since the momentum of the wind depends not only upon its velocity, but also upon its density.

From the end of March 1765, to the end of March last, we, in this part of Scotland, had very little rain, and less snow in proportion; our rivers were as low, through the winter, as they use to be in the middle of summer; springs failed in most places, and brewers and maltsters were obliged, even in winter, to carry their water at a considerable distance; I was much afraid there would not be moisture enough in the earth for the purposes of vegetation, if this season should set in as dry as the former, before we got a new supply of rain. In the end of March last, we had a fall of snow; and, as I did not remember to have ever read an account of such an experiment, I wished to be able to determine, to what quantity of rain this fall of snow was equal.

The snow had been falling from five o'clock the former evening, till ten o'clock next day; about eleven o'clock I measured the depth of the snow, and found

it to be 6.2 inches ; then I took a stone jug, holding about three English pints, and turned the mouth of it downwards upon the snow measured, and where the ground below was smooth, and hard ; and by this means I took up all the snow from top to bottom in the jug ; this snow I melted by the side of a fire, and the 6.2 inches of snow yielded six tenths of an inch deep of water in the same jug. After emptying the jug, I dried, and weighed it in a balance, and took up the same quantity of snow in it as before, weighed it again, and found the weight of the snow taken up, and from this weight computed what quantity of water it should have produced, and found that it ought to have produced six tenths of an inch and  $\frac{1}{20}$  of an inch more : then I dissolved the snow, and found that it yielded a quantity of water in the bottom of the jug, six tenths of an inch deep as in the former experiment. The difference of  $\frac{1}{20}$  of an inch in the depth of the water, betwixt the weight and the melting of the snow, was probably owing to an exhalation from the jug, while the snow was melting by the fire, for I observed a steam sometimes rising from it. A greater or lesser degree of cold, or of wind, while the snow falls, and its lying a longer or shorter while upon the ground, will occasion a difference in the weight and in the quantity of water produced from a certain number of cubic feet, or inches, of snow ; but, if I may trust to the above trials, (which I endeavoured to perform with care) snow, newly fallen, with a moderate gale of wind, freezing cold, which was the case of the snow I made the trials upon, the 27th of March last, will produce a quantity of water equal to  $\frac{1}{10}$  part of its bulk ; or the earth, when covered with snow,

ten inches deep, will be moistned by it when melted; rivers, and springs recruited, as much as if a quantity of rain had fallen that covered the surface of the earth to the depth of one inch.

I am, my Lord,

with the greatest respect,

your Lordship's most obedient

and most devoted humble servant,

Alex. Brice.

Received June 5, 1766.

XXVII. *Some Observations on the Country and Mines of Spain and Germany, with an Account of the Formation of the Emery Stone; from William Bowles, Esq; Director General of the Mines of Spain; communicated by P. Collinson, F. R. S.*

Read June 19,  
1766.

AT the extremity of Old Castile, in Spain, is situated a territory called Montana, which is divided into two parts; the Low Montana is that chain of mountains, which bounds  
the

the Cantabrian Sea. The city Santander is its chief port, from whence you ascend southerly, twelve long leagues, a succession of high craggy mountains, to the town of Reynosa in the upper Montana, which extent stretches three leagues more, and then you continually descend about fourteen leagues to the city of Burgos, the capital of Old Castile.

Reynosa is in the center of an open plain, surrounded by a ridge of high mountains, at whose feet are low hills of pasture-land.

To the west of Reynosa, in an hour's walk, is the source of the great river Ebro, which receives all the waters on that side, and conveys them into the Mediterranean, seven leagues below the city Tortosa.

All the spring, rain and snow waters, of the mountains to the north of Reynosa, run into the Bay of Biscay.

The waters, from the south chain of the mountains, are collected in the river Pisuerga, which runs into the river Duero, and from thence are carried to the Atlantic ocean at Oporto.

Hence we see, that the adjacent parts of Reynosa divide the waters of the three seas, which lye north, east, and west.

Eight leagues square of this upper Montana is the highest land in Spain; the mountains rise to the atmosphere to the line of congelation; I see snow from my window this 4th of August, as writing this letter. Some years ago there used to fall so much snow, that the people were forced to dig lanes through it, to go to church, in the winter; but there has fallen little snow since the earthquake at Lisbon, and some years none at all. I am persuaded, it changed the climates

of many parts of Spain; for no man living saw, nor heard his father say he saw, snow fall in or about the city of Sevil, until the year 1756.

I found many plants in these mountains, which I remember to have seen in Switzerland; they abound with oak, beech, birch, holly, and hazel.

The hills and plains are fine pasture; I never saw a meadow in any other part of Spain, neither did I see horses and cows feed on hay any where else.

These mountains are formed of sand-stone, lime-stone, plaster-stone (or gypsum) and emery-stone.

The sand-stone, is at the summit of the mountains, and some hills, and the lime-stone forms the body; but the contrary is seen in others, the sand-stone abounds, and the plaster is always lowest.

As for example, the high mountain of Arandilla, which is a small league off the town, is all sand-stone at the summit; its body is a mass of ash-coloured lime-stone, in which is found imprisoned petrified cornu ammonis, and scollop shells; and there are beds of plaster-stone at its foot, towards the plain; these join to a stratum of black marble veined white and yellow, which is no more than a purer kind of lime-stone, as all other marbles are.

On the hill to the east of Reynosa, and in the plain, are found great blocks of emery-stone, of which I will say a word, because I think its nature is not truly known; at least that of Spanish emery, which the looking-glass grinders of the king's fabric at St. Ildefonso say is the most biting emery, they ever used; and I never saw any other in its native matrix.

That



That iron has been, and is now, in a fluid state, percolating through the earth, and that it subsides, chrysalises, or is precipitated, to form different bodies, is demonstrated by the black and red bloodstone, by some beautiful stalactites, which are almost pure iron, by the eagle-stone, by figured pyrites, by native vitriol, and by native crocus.

When this fluid iron penetrates a rock of sandstone, and only stains the surface of each grain, of a brownish, reddish, or yellow colour, it becomes only sand and crocus; but, when it is joined with the chrysaline matter in a fluid state, in the very act of chrysalisation of each grain of sand it incorporates with it, its weight and hardness is increased, and then it becomes emery.

The earth of the mountains and hills is of the same nature as that of the rock below. If it is limestone, the soil cast into any acid liquor will boil up with a violent effervescence, and the acid will dissolve it.

If the rock below be sandstone, or plasterstone, or emery, the earth of that hill or mountain will remain quiet in the acid, and there is no effervescence nor dissolution.

I have often observed, that, when the rocks below are mixed, calcary and noncalcary, the soil of the surface is also of a mixt nature; and I always found the action of the acid to be weak or strong upon these earths, in proportion to the sort of stone with which they abound.

Thirty one leagues south east of Madrid, and five leagues south of the source of the river Tagus, is situated the town of Molina Aragon, capital of a lordship

lordship of the crown, almost in the center of Spain; the high hills of this little territory are covered with pine trees; here I learned some truths, which prove, that the following opinions ought to be ranked amongst vulgar errors.

First, that salt-springs are not found in the high primitive mountains, but in the low hills and plains only.

The elevated town of Molina, and the rocky country about it, is formed of red and grey sand-stone, lime-stone, white and grey granite. These rocks contain either salt, or salt-petre; the houses built of this stone are covered with the saline efflorescences, which are drawn out by the sun after rain. The whole territory of Molina is full of salt springs; but there is a copious salt-spring, rising out of a land yet higher than the source of the Tagus, and not far from it, which is one of the highest territories in all the inland parts of Spain, for it divides the waters of the ocean and Mediterranean. This spring furnishes salt to the jurisdiction and bishoprick of Albarrazen. There is besides another salt spring, in another elevated ground, which supplies the eighty two towns and villages of Molina-Aragon with salt: besides which, there is a salt spring, issuing out of a spot in the Montana, which is higher than the fountain of the Ebro, and about a quarter of a mile distant from it.

Secondly, that metallic vapours destroy vegetation; the following instances evince the contrary.

There are many iron, copper, lead, and pure pyritous ores, in these mountains; and yet the same plants, and the same sweet grass grow there as in other parts, of which I will give a more particular account.

About two hours walk northwest of Molina, there is a little hill called Platilla; it extends about half a league over, from valley to valley; its body is a solid, rocky, white granite, through which run, in different directions, and without any order, an infinite number of blue, green, and yellow veins of rich copper ore, which holds a little silver, mineralized by a great quantity of arsenic and sulphur: the very surface of the rock is in many places stained blue, and green, and the veins of ore are not above a foot deep. In the fissures, and in the solid rock, is contained lead ore, which is sometimes found even on the surface; and yet the following plants grow out of the soil, which covers these arsenical sulphurous veins, and is not more than a foot deep; true oak, flax, white thorn, juniper, cytus, wild-rose, uva ursi, phlomis, verbascum, stœchas, sage, thyme, serpillum, rosemary, and many others, which it would take up too much time to mention. The earth of this same hill is covered with the same sweet small grass as the rest of the country.

I have also made the same observations, out of Spain, at the three greatest mines in Europe, viz. St. Mary of the mines in Alsatia; Clausthal, in the Hartz-mountains of Hanover; and Frayberg, in Saxony.

The mines of St. Mary are at the head of a valley. Its hills are some of them covered with oak, pines, and others with apple, pear, plum and cherry, and others, with fine grass downs. The tops of others are fields of wheat, which, in the year 1759, as I found by my notes, gave a produce of eight for one. All these vegetables grow in a soil, a foot or two deep,  
which

which covers a rock, full of the most arsenical, sulphureous, silver, copper, lead, and cobalt ores, in Europe, and most of the veins are near the surface.

The mines of Frayberg are in low hills near the city. I saw them all covered with barley in July. A stranger would not imagine that men were reaping corn over hundreds of miners heads, who were blowing up veins of ore, arsenick, and brimstone.

The mines of Clausthal are in a plain, which, in truth, is the summit of a mountain; the Dorothy and Caroline veins of silver, lead, and copper ore, stretch away eight miles to the Wild-Man mountain; the finest meadows and sweetest grass are upon these veins, and all their branches near the city: they feed nine hundred cows, and two hundred horses; they are mowed in June, and a second crop springs up, which is mowed in August: a multitude of plants grow in these high meadows, over the mines.

It is true, I saw mines in the barren naked mountains and hills: but it is certain that their barrenness is not the effect of mineral vapours; but the air, moisture, heat, and cold, have more power over the surfaces of some rocks, than of others, to moulder the stone into earth. Such is the high mountain of Ramelsberg, above Goslar, whose inhabitants have lived by the mines found therein. I crept up this steep rock to its summit; I found it split and cracked into millions of fissures, from one foot to an inch wide; in other places, it was shivered into small rotten stones, which became a receptacle for a few plants, grass, moss, &c. and, as this decayed stone moulders into earth, it will be more abundant in vegetable productions; this may, perhaps, have been the origi-

nal state of those mountains, which are now covered with verdure.

Communicated by

Madrid,  
Jan. 1765.

P. Collinson.

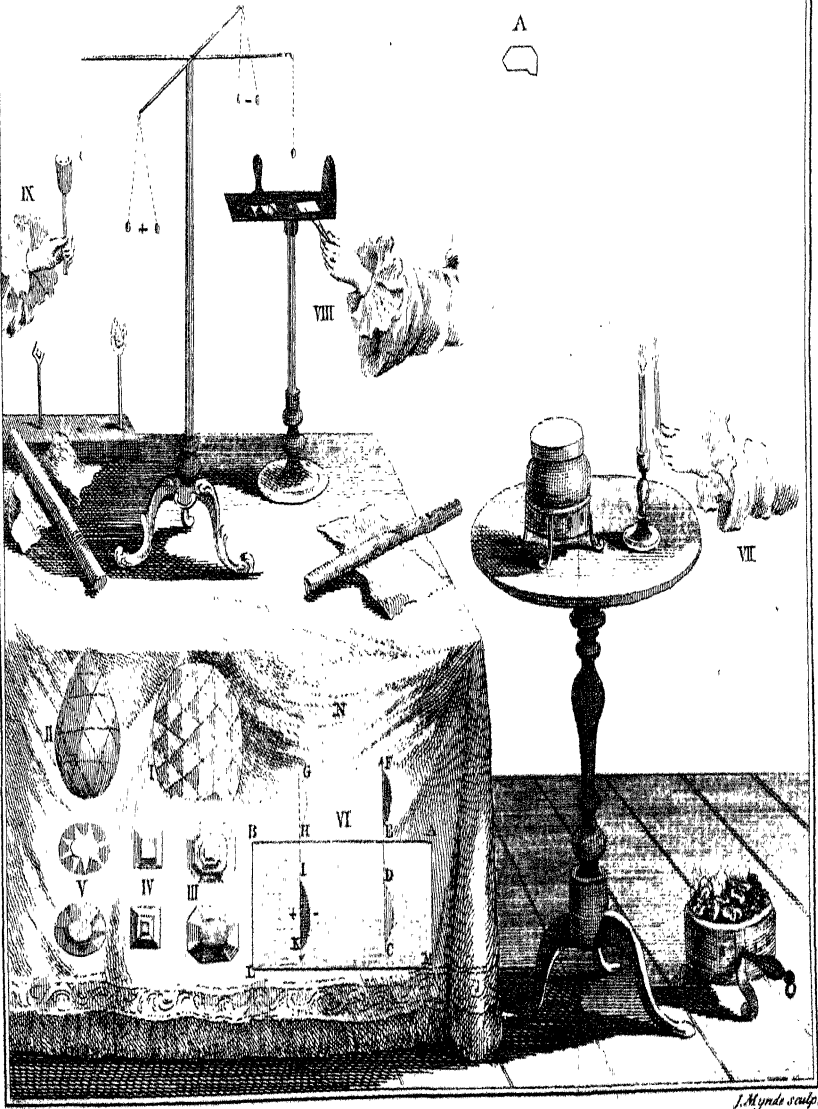
XXVIII. *Commentarius de indole Electrica Turmalini, auctore Torberno Bergman, Mathematicum & Philosophiæ Naturalis ad Reg. Academiam Upsl. adjuncto, Academiæ Imper. N.C. Academiæ Reg. Scientiarum Stockholmenfis & Societatis Regiæ Londinensis membro.*

Read Nov. 20, I.  
1766.

CUM circa finem anni præteriti quinque selecti turmalini, ab Academia Regia Scientiarum Stockholmenfi nuper acquisiti, in meas inciderent manus, venia illustris hujus Societatis varia hisce institui tentamina, ut singularium horum lapillorum naturam, si fieri posset, penitus expiscarer. Fateri vero convenit, me parum successus sperasse, postquam celeberrimi nostri ævi physici, Æpinum loquor & Wilsonum, turmalinum adtentione, viris hujus ordinis propria, scrutati fuerant. Et sane, si res esset dijudicanda ex numero scriptorum admodum parvo;











parvo, etsi indoles eorum electrica dudum ante novem innotuerit annos, in eam facile incidissem opinionem; tales lapides ex Indiis minori molestia comparari, quam novi quid iisdem inveniri. Ast dissensus memoratorum physicorum circa nonnulla experimenta fecit, ut non omnem abjicerem spem.

Omnia instituta tentamina enumerare tanto minus necessarium duco, cum non tantum hæc singula, sed etiam reliqua antea nota, ex unica simpliceque dependeant lege generali. Antequam vero hanc explico, ipsi turmalini breviter sunt describendi. Præterea, quidquid Domini Æpinus & Wilson de eorum proprietatibus scripserint, in sequentibus pro notis pono, ne longo anfractu circumscribatur disquisitio.

II. Primus & maximus coloris est fere nigri, tenues vero pellucidique margines rufescunt. Alterum latus est planum, alterum convexum arteque in triangula divisum, uti TAB. XII. Fig. 1. monstrat. In utroque ex modo descriptis lateribus unus est polus. Sub itinere margo paulum distractus fuit, & granum quartzi, adhuc in fractura remanens, fragilitatis causam indicat. Damnum hinc magnitudini illatum habitus solertissimi mineralogi, Domini Rinman, resarcivit, quippe qui particulis divulsis talia instituit experimenta, ut in regno minerali mirando huic lapidi conveniens adsignari queat locus, quod, quantum scio, nemo prius tentavit. De hoc vero plura adferre, a præsentis scopi foret alienum.

Secundus paullum minor, fusco flavescens, ab utroque latere æqualis & similis figuræ 2. Poli sunt oppositi in margine laterali, radiique lucis, ab altero in alterum, transire nequeunt lapillum in omni alia directione pellucidum.

Tertius.

Tertius parvus & tenuis adhuc dilutioris est coloris. Poli in lateribus oppositis sunt siti, atq; hic æque ac quintus in eo a reliquis differt, quod totus sit pellucidus Fig. 3.

Hi tres e Ceylona apportati, & quantum scio omnes ex hac insula in Europam translati, colore flavescente præditi fuerunt, licet magis minusve nigri fuerit admixtum. E contrario in Brasilia (ab æquatore austrum versus æque circiter distante, ac Ceylona in hemisphærio boreali) reperti turmalini, alios induunt colores. Rubros, virides & cœruleos vidi, forte etiam aliter tincti occurrunt.

Quartus viridis est, cujus latera Fig. 4. monstrat.

Quintus cyaneus, totus pellucidus, polos in lateribus oppositis possidet. Vide Figuram 5. Præterea rudes quoque habui, virides & cœruleos, nulla arte mutatos. Hi crystallos prismaticas multorum laterum referunt, & quod rarum est, non tantum angulos prominentes, sed etiam inflexos habent, uti ex sectione unius in Fig. A. adparet. Similem & regularem figuram non servant, sed aliis plura, aliis pauciora latera. Pyramides terminales si adfuerint nescio; omnes saltem erant abruptæ, cum in meas pervenirent manus. Omnes in eo luculenter conveniunt, quod poli extremitates columellarum occupent, quodque in hac directione, seu secundum longitudinem, perfecte sint opaci.

## LEX FUNDAMENTALIS.

III. *Cujusvis turmalini alter polus dilatatione electricitatem acquirit positivam, contractione negativam; alter vero contrariam habet indolem, adeo ut contractione fiat positivus & dilatatione negativus.*

Dum pororum capacitates minuuntur, turmalinum in statu contractionis esse dico. Status vero dilatationis adest, dum eorundem amplitudines augentur.

Sint ABN & ABLM (Fig. 6.) duo media, quorum alterutrum e. g. ABLM fit calidius. Turmalinus temperie medii AL præditus, in eo nulla electricitatis signa exferit, intret vero alterum frigidius, mox pori contrahuntur & lapillus fit electricus, idque hoc modo, ut alter polus fiat +, alter —. Postquam vero heic differentiam caloris plene amiserit, omnia cessant phænomena electrica. Si vero jam in calidius remigret, iterum fit electricus, at dilatatione, & quidem contrario modo, adeo ut polus in priori casu +, heic fiat —, — autem +. Hanc vim similiter retinet usque dum pororum capacitates calori medii fuerint adaptatæ.

Hæcce propositio explicata adeo est fœcunda, ut illa posita, omnes quotquot mihi notæ sunt proprietates electricæ, turmalino in specie competentes, ex hac fluant, corolariorum instar, ex theoremate fundamentalis. Phænomena frictionis huc non pertinent, quippe quæ turmalino cum multis aliis communia.

IV. Quæ ex lege fundamentalis pronò, ut aiunt, alveo deduci queant, jam examinanda. In genere patet, nonnisi quinque variationes esse possibiles; aut enim

alter porus est	(+)	& alter	(-)
aut - - -	(+)	- -	(+)
aut - - -	(-)	- -	(-)
aut - - -	(+)	- -	(+)*
aut - - -	(-)	- -	(-)

Hæ tamen omnes variationes tribus comprehendi possunt casibus.

(+) (-)

V. Casus primus duplici probari potest modo, & calore & frigore. Si turmalinus liquidis ebullientibus, carbonibus ardentibus, vel alio quocunque modo æqualiter calefactus, in aëre frigidiorè examini subjicitur, in statu contractionis invenietur. Heic adnotasse juvat electricitatem turmalini, aqua fervida extracti, calori non esse adscribendam, nam illa vis, quæ primis momentis intra aquam generatur, mox destruitur, partim medio ambiente, partim eam ob causam, quod cito fixum nanciscatur aquæ calorem. Extractus igitur lapillus initio nullam possidet electricitatem, sed sub voluminis contractione paucis minutis secundis comparat.

Metallis calidis impositus, e camera frigidiorè in calidiorem translatus, vel quocunque alio modo volumine auctus, reperitur in statu dilatationis.

Frigore artificiali rite & prudenter adhibito, statum vel contractionis, vel dilatationis prolubitu provocavi.

(+) (+)

(-) (-)

VI. Ut turmalinus eandem ubique nanciscatur electricitatem, alter polus contrahi debet, dum alter dilatatur. Experientiam consulamus.

\* Statum naturalem voco, dum nulla adfunt electricitatis signa, eumque designo ±.

Teneatur alter polus juxta candelæ flammam, dimidio circiter minuto, magis minusve pro crassitie lapilli (Fig. 7.). Si dein exploretur, uterque polus eam possidet electricitatem, quam polus calefactus in statu contractionis acquirit. Ratio hæc est. Quia alter polus magis calefit, necesse quoque est, ut e flamma remotus magis contrahatur, quam alter, qui parum vel nihil caloris obtinuit. Hinc calor, quem interiores particulæ nactæ sunt, alterum polum versus (seu non calefactum) cogitur, eodem fere modo ac faber calorem fundum versus aqua pellit, qua carbones adspersit. Polus igitur non calefactus parvo temporis spatio dilatatur, dum alter contrahitur, ideoque eandem uterque monstrat vim (§ 3.). Ast hæc poli non calefacti dilatatio non diu potest durare, ideoque cito contrahi incipit, & phænomena casus primi oriuntur.

VII. Ex hoc fundamento celebratissimorum physicorum, Wilsoni & Æpini litem componendam esse persuasus sum. Experimentum Wilsonianum, sectione præcedente explicatum, rite institutum semper succedit; si vero alter polus diutius vel fortiori, quam par est, exponatur igni, calor tanta copia penetrat, ut lapillo a flamma remoto, oppositus quoque contrahatur, non obstante particula caloris interni illum versus pulsi; & per se patet tunc prodire casum primum. Forte tali modo Domino Æpino phænomena casus secundi effugerunt, quam suspicionem magnitudines quoque turmalinorum, quos adhibet physicus Petropolitanus, confirmant.

Carbonibus ardentibus prudenter adhibitis phænomena Londinensia æque feliciter prodeunt.

Æpiniana tentamina fortioribus mutationibus producantur, & iisdem principiis luculenter congruunt.

Polus enim carbonem ardentem primo tangens, & mox vitro vel laminæ metallicæ impositus, ex novo contactu vehementius contrahitur, quam in aëre ejusdem temperiei, in ratione majoris soliditatis, adeoque oppositus non potest non calore, illum versus ruente, magis dilatari. Quomodo vero tandem statum contractionis induat, antea dictum est.

Nulla igitur est pugna inter experimenta Petripolitana & Londinensia. Solus in explicatione consistit dissensus. Ambo in caloris inæquali distributione causam quærunt. Hæc inæqualitas revera quoque adest, sed ex adductis in aprico est, illam pro circumstantiis protei instar varias induere species.

$$\begin{array}{cc} (+) & (+) \\ (-) & (+) \end{array}$$

VIII. Ut casus tertius prodeat, hoc est ut alter polus sit vel  $+$ , vel  $-$ , alter vero nullam prodat electricitatem, polus electricus juxta legem fundamentalem contrahi vel dilatari deberet, altero manente immutato. Hoc difficulter experimento monstratur, modo vero sequenti quodammodo mihi satisfeci. Alter polus laminæ metallicæ, pede vitreo instructæ, imponatur, & oppositus indusio tegatur metallico, ex quo columella extremo rotundato surgit, alii ex lamina, uti Fig. 8. indicat, respondens. Hisce ita præparatis, metallum candens laminæ imponatur, quo polus hanc tangens momento fere eundem caloris gradum accipit & per aliquot minuta conservat. Hinc lamina hoc tempore vix ullam prodit electricitatem, dum interea indusium vivacissima dilatationis edit signa. Turmalino quodam rudi hoc facilius præstavi, nam columnæ longitudo fecit; ut alterum extremum calefacere potuerim, alterius statu naturali manente immutato.

IX. Allata

IX. Allata sufficienter evincunt, phænomena electrica turmalini, quam satis, unicæ simplicique convenire legi, adeo ut proprietates, quas hucusque physicorum solertia detexit, & forte exhaurivit, paucis comprehendi possint lineis. Si ubique naturæ operationes æque feliciter ad causas proximas revocare liceret, proluxi phænomenorum catalogi in minimam cito redigerentur molem, effectus continentem principales. Ut huc perveniamus, hoc opus, hic labor.

X. Unicum addam experimentum, contrarias electricitates, me iudice, luculenter demonstrans. Scilicet capsulam metallicam, manubrio vitreo donatam (Fig. 9.) aqua fervida implevi, eique turmalinum injeci. Lapillus heic statim dilatationis acquirit, sed nihilo minus capsula ne minimum quidem electricitatis signum prodit. Si negativa, nonnisi debilior positivæ gradus esset, earum summa in præsentī casu sine dubio foret conspicua.



XXIX. *Theory of the Parallaxes of Altitude for the Sphere, by Mr. F. Mallet, Professor and Astronomer at Upsal; Translated from the French by M. Maty, M. D. R. S. Sec.*

Upsal, October 25, 1766.

Read Nov. 20, § 1. 1766. **L**ET  $P$  be = the moon's horizontal parallax, or 1 to  $\text{fin. } P$ , as the moon's distance to the radius of the terrestrial sphere, on which the spectator is supposed to be placed. Let  $A$  be the distance of the moon from the zenith, and  $p$  the parallax of altitude for the same distance. The astronomers usually compute the value of  $p$  in the following manner: let  $\text{fin. } p = \text{fin. } P \cdot \text{Sin. } A$ , and  $p'$  being found by the tables of logarithmic sines,  $\text{fin. } p' = \text{fin. } P \cdot \text{fin. } A + p'$  is found in like manner,  $p''$  being assumed for the true parallax, which is not accurate.

§ 2. In order to shew this, I have given another method of computing the parallax of altitude, as exactly as may be, by means of the common tables, in the following manner. Since  $\text{fin. } p = \text{fin. } P \cdot \text{fin. } A + p$ , we have  $\text{fin. } p = \text{fin. } P \cdot \text{fin. } A \cdot \text{cos. } p + \text{fin. } P \cdot \text{cos. } A \times \text{fin. } p$ , or  $\text{fin. } p (1 - \text{fin. } P \cdot \text{cos. } A) = \text{fin. } P \cdot \text{fin. } A \cdot \text{cos. } p$ ; hence  $\text{tang. } p = \frac{\text{fin. } P \cdot \text{fin. } A}{1 - \text{fin. } P \cdot \text{cos. } A}$ . This formula seems a little difficult to be wrought in numbers, but it is as easy as the above one; for, supposing  $\text{fin. } B^2 = \text{fin.}$

fin. P cof. A, the tables will give the angle B, and  
 $\text{tang. } p = \frac{\text{fin. P fin. A}}{\text{cof. B}^2}$ , the computation of which can  
 give no trouble. Hence it appears, that the calculus  
 for finding the true parallax is not more difficult than  
 that, which gives the said parallax with an error, the  
 value of which is unknown; for it is evident that the  
 above computation for finding  $p''$  is only an approxi-  
 mation, and that, to make it accurate, it would be ne-  
 cessary to carry it still on by finding  $\text{fin. } p''' = \text{fin. P}$   
 $\text{fin. } (A + p'')$ , and afterwards  $\text{fin. } p'''' = \text{P fin.}$   
 $(A + p'')$  &c.

§ 3. I therefore think myself in the right to prefer  
 my method to that hitherto used by astronomers. To  
 confirm my opinion, I made a trial, by putting  $P = 59'$   
 and  $A = 30^\circ$ , and found  $p - p'' = 0'',43$ , in which  
 the error of the usual computation amounts to near  
 half a second; I therefore give the preference to the  
 geometrical calculus.

§ 4. Before I quit the formula  $\text{tang. } p =$   
 $\frac{\text{fin. P fin. A}}{1 - \text{fin. P cof. A}}$ , I must observe, that the computation  
 of  $p$  may be executed by other methods to the same  
 exactness. If we take  $\text{cof. } 2 C = \text{fin. P cof. A}$ , we  
 shall have  $\text{tang. } p = \frac{\text{fin. P fin. A}}{2 (\text{fin. C})^2}$ , and the computation  
 of this new formula is extremely easy.

§ 5. The formula  $\text{tang. } p = \frac{\text{fin. P fin. A}}{1 - \text{fin. P cof. A}}$ , gives  
 besides,  $\text{fin. } p = \frac{\text{fin. P fin. A}}{\sqrt{1 + \text{fin. P}^2 - 2 \text{fin. P cof. A}}}$ ; make  $\text{fin.}$   
 $P = 2 \text{ cof. D}$ , D being a given angle, of which we  
 may have tables ready made, and we shall have  $\text{fin.}$

$p =$

$$p = \frac{\text{fin. } P \text{ fin. } A}{\sqrt{1 + 2 \text{ fin. } P (\text{cof. } D - \text{cof. } A)}} = \frac{\text{fin. } P \text{ fin. } A}{\sqrt{1 + 4 \text{ fin. } P \text{ fin. } \frac{A+D}{2}}}$$

$$\frac{\text{fin. } \frac{A-D}{2}}{\text{fin. } \frac{A+D}{2}}; \text{ since cof. } D - \text{cof. } A = 2 \text{ fin. } \frac{A+D}{2} \text{ fin. } \frac{A-D}{2}.$$

This being found without any logarithmic computation, we shall find  $\text{tang. } E^2 = 4 \text{ fin. } P \text{ fin. } \frac{A+D}{2} \text{ fin. } \frac{A-D}{2}$ , if  $A > D$ , and hence we may easily compute  $\text{fin. } p = \text{fin. } P \text{ fin. } A \text{ cof. } E$ ; but if  $A < D$  we shall find  $\text{cof. } F^2 = 4 \text{ fin. } P \text{ fin. } \frac{A+D}{2} \text{ fin. } \frac{A-D}{2}$  and hence  $\text{fin. } p = \frac{\text{fin. } P \text{ fin. } A}{\text{fin. } F}$ .

§ 6. Similar formulæ may be found for cof.  $p$ , but as the angle  $p$  is pretty small, one might easily fall into some error by the usual tables of logarithms. I shall not say what would be the amount of this error of  $p$ , having furnished the manner of avoiding it; but this remark has not, I think, as yet been made in astronomical calculations; and I have found it of great consequence in computing eclipses, where the distances to be found are very small arches.

§ 7. It may moreover be observed, that if  $A = D$ ,  $\text{fin. } p = \text{fin. } P \text{ fin. } A$ ; hence  $p' = p$  in the same case, and  $p'' > p$ , which seems very odd; but the moon then is below the sensible horizon.

*Theory of the apparent Diameters of the Moon.*

§ 1. First the expression of horizontal diameter of the moon, or of the diameter seen at the horizon, seems to me too vague; for one ought to understand by it the diameter seen at the center of the terrestrial

terrestrial sphere, rather than the apparent diameter at the horizon, which is not affected by refraction. Without this, if the one was confounded with the other, an error would arise for the latitude of Paris from  $0'',25$  to  $0'',32$ .

§ 2. Let us keep the same denominations of  $P$ ,  $p$ , and  $A$ , and call  $D$  the apparent semi-diameter of the moon at the centre of the sphere, and  $d$  the apparent semi-diameter of the moon at the zenith distance  $=A$ . We shall have  $\sin. A : \sin. \overline{A+p} :: \text{tang. } D : \text{tang. } d$ , or if one will,  $\sin. A : \sin. \overline{A+p} :: D : d$ : the error not exceeding an 100th part of a second.

§ 3. We had above  $\sin. p = \sin. P \sin. \overline{A+p}$ . Hence  $\sin. P \sin. A : \sin. p :: (\text{tang. } D : \text{tang. } d) :: D : d$ , or because  $\sin. p = \frac{\text{cof. } p \sin. P \sin. A}{1 - \sin. P \text{ cof. } A}$ ,  $1 - \sin. P \text{ cof. } A : \text{cof. } p :: D, d$ , and  $d = \frac{D \text{ cof. } p}{1 - \sin. P \text{ cof. } A}$ .

§ 4. Mr. Euler, in the Memoirs of the Academy of Berlin, 1747, pag. 175, makes this same value  $= \frac{V}{1 - p^2 \sin. b}$ , and according to him,  $V = D.M = \sin. P \sin. b = \text{cof. } \overline{A+p}$ ; from whence it appears, that the true value of the apparent diameter of the moon, is not more difficult to be computed than the approximated one of Mr. Euler, the exact and geometrical formula being  $\text{tang. } d = \frac{\text{tang. } D \text{ cof. } p}{1 - \sin. P \text{ cof. } A}$  and that of Mr. Euler  $d = \frac{D}{1 - \sin. P \text{ cof. } \overline{A+p}}$ ; for in both, the values of  $D$ ,  $A$  and  $p$  must be employed.

§ 5. It likewise appears to me, that since  $\frac{\text{cof. } p}{1 - \text{fin. } P \text{ cof. } A}$   
 $= \frac{\text{fin. } p}{\text{fin. } P \text{ fin. } A}$  and therefore  $\text{tang. } d = \frac{\text{tang. } D \text{ fin. } p}{\text{fin. } P \text{ fin. } A}$ , astro-  
 nomers ought no less to employ this last formula, than  
 any other more troublesome, in practical computation.

The simplest is  $\text{tang. } d = \frac{\text{tang. } D \text{ fin. } A + p}{\text{fin. } A}$ , upon the  
 supposition of an exact table of the parallaxes of  
 altitudes ready made; and I believe it will be  
 as easy to compute with tangents as with arches,  
 by means of logarithms; and therefore this simpli-  
 fication in putting arches instead of tangents is  
 unnecessary.

§ 6. To try the consequences of this theory, I made  
 $A = 30^\circ$ ,  $D = 15'$ , and taking the vertical of Upsal to  
 the terrestrial axis for the radius of the sphere, I found  
 $P = 55', 10'', 3$ , supposing that the axis of the earth,  
 is to the diameter of the equator as 199 to 200, and by  
 the formulæ  $\text{tang. } d = \frac{\text{tang. } D \text{ fin. } A + p}{\text{fin. } A}$   $\frac{\text{tang. } D \text{ cof. } p}{1 - \text{fin. } P \text{ cof. } A}$   
 $\frac{\text{tang. } D \text{ fin. } p}{\text{fin. } P \text{ fin. } A}$ , I found  $d = 15', 12'', 664$ , but by  
 the formula  $d = \frac{D \text{ cof. } p}{1 - \text{fin. } P \text{ cof. } A}$ , I had  $d = 15', 12'',$   
 $675$ . and lastly by that of Euler  $d = \frac{D}{1 - \text{fin. } P \text{ cof. } A + p}$   
 we have  $d = 15', 12'', 635$ ; from whence it appears  
 that the error is very small, but that with the same  
 trouble one may avoid any error whatsoever.

§ 7. The present case did not give an error of  
 $0'', 001$  in substituting 1 or the radius instead of  $\text{cof.}$   
 $p$ . Hence I conclude that  $d = \frac{D}{1 - \text{fin. } P \text{ cof. } A}$  will be  
 a more

a more exact formula than that of Euler  $d = \frac{D}{1 - \sin. P \cos. A + p}$ .

§ 8. By taking  $d = \frac{D}{1 - \sin. P \cos. A}$ , we have  $d - D = \frac{D \sin. P \cos. A}{1 - \sin. P \cos. A} = \frac{D \sin. p \cos. A}{\sin. A \cos. p} = \frac{D \tan. p}{\tan. A}$ , which affords an elegant theorem, to find the increase of the apparent diameter of the moon.

§ 9. I have found others by the following methods. Since  $\sin. A : \sin. A + p :: \tan. D : \tan. d$ , and  $\sin. A : \sin. A + p - \sin. A :: \tan. D : \tan. d - \tan. D :: \sin. D \cos. d : \sin. d - D$ ; but  $\cos. D = \cos. d$  without any sensible error, and  $\sin. D \cos. D = \frac{1}{2} \sin. 2 D$ , and  $\sin. A + p - \sin. A = 2 \sin. \frac{1}{2} p \cos. A + \frac{1}{2} p$ , we shall have  $\sin. d - D = \frac{\sin. 2 D \sin. \frac{1}{2} p \cos. A + \frac{1}{2} p}{\sin. A}$ . In the

same manner, as I before found  $\sin. p' = \sin. P \sin. A$  and  $\sin. P \sin. A : \sin. p :: \tan. D : \tan. d$ , hence  $\sin. p' : \sin. p - \sin. p' :: \sin. p' : 2 \sin. \frac{p - p'}{2} \cos. \frac{p + p'}{2} \dots$   
 $\sin. 2 D : \sin. d - D = \frac{\sin. 2 D \sin. \frac{p - p'}{2} \cos. \frac{p + p'}{2}}{2 \sin. P \sin. A}$

§ 10. Lastly let  $L =$  the distance of the moon from the center of the sphere,  $l$  its radius, that of the sphere being  $= 1$ , we have  $1 : L :: \sin. P : 1$  and  $L : l :: 1 : \tan. D$  or  $1 : l :: \sin. P : \tan. D = l \sin. P$ ; hence  $l = \frac{\tan. D}{\sin. P}$  being once found, since  $\sin. A : \sin. A + p :: \tan. D : \tan. d$ , and  $\sin. A + p : \sin. p :: 1 : \sin. P$ , we shall have  $\sin. A : \sin. p :: \tan. D : \sin. P \tan. d :: l : \tan. d = \frac{l \sin. p}{\sin. A}$ . I found the logarithm of  $l =$

9.4343965 at Upsal, by putting 10 for that of the radius of the sphere determined as before.

F. Mallet.

XXX. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1765, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. nuper Præses: By William Hudson, Societatis Regiæ & clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelseæ. Præfectus et Prælector Botanic.*

Read Nov. 20, 2151.  
1766.

- A**CHYRANTHES lanata, caule prostrato, spicis ovatis lateralibus, calycibus tomentosis. Lin. Sp. pl. 296. Mill. Dict. tab. 11. fig. 1.  
Amaranthus Indicus verticillatus albus, foliis lanugine incanis. Pluk. alm. 27. tab. 79. f. 8.  
2152 Andrachne procumbens herbacea. Lin. Sp. pl. 1439.  
Telephoides Græcum humifusum flore albo. Tourn. cor. 50. Dill. Hort. Elth. 377. tab. 282 f. 364.  
2153 Bryonia Africana, foliis palmatis quinquepartitis utrinque lævibus: laciniis pinnatifidis. Lin. Sp. pl. 1438.

Bryonia

- Bryonia Africana laciniata, tuberosa radice,  
 floribus herbaceis. Herm. parad. 107. tab. 108.
- 2154 *Carthamus corymbosus*, floribus corymbosis  
 numerosis. Lin. Sp. pl. 1164.  
*Chamæleon niger*. Dalech. hist. 1454.  
*Chamæleon niger umbellatus*, flore cœruleo  
 hyacinthino. Bauh. pin. 38.
- 2155 *Ceanothus Americanus*, foliis trinerviis. Lin.  
 Sp. pl. 284. Mill. Dict. tab. 86.  
*Euonymus Novi Belgii*, corni feminæ foliis. Comm.  
 hort. Amst. 1. p. 167. tab. 86. Raj. Dendr. 69.  
*Euonymus*, jujubinis foliis, Caroliniensis, fructu  
 parvo, fere umbellato. Pluk. alm. 139.  
 tab. 28. f. 6.
- 2156 *Chryfocoma*, *Coma aurea*, fruticosa, foliis lin-  
 earibus, rectis glabris. Lin. Sp. pl. 1177.  
*Conyza Æthiopica*, flore bullato aureo, pinastri  
 brevioribus foliis læte viridibus. Pluk. alm.  
 400. tab. 327. f. 2.
- 2157 *Coronilla juncea*, fruticosa, foliis quinatis ter-  
 natifque lineari-lanceolatis subcarnosis obtusis.  
 Lin. Sp. pl. 1047.  
*Colutea caule genistæ fungoso*. Bauh. hist. 1.  
 p. 383.  
*Polygala major massiliotica*. Bauh. pin. 246.
- 2158 *Draba muralis*, caule ramoso, foliis cordatis  
 dentatis amplexicaulibus. Lin. Sp. pl. 897.  
 Hudf. Fl. angl. 243.  
*Bursa pastoris major* loculo oblongo. Bauh. pin.  
 108. pr. 50. tab. 50.
- 2159 *Galium verrucosum*, foliis tenuis lanceolatis  
 serrato-aculeatis, pedunculis trifloris, fructi-  
 bus verrucosis.



- Aparine; femine coriandri saccharati. Park.  
 theatr. 567. Rand. Ind. Hort. Chelf. 19.
- 2160 *Gypsophylla pilosa*, foliis lanceolatis trinerviis  
 amplexicaulibus, caule piloso, floribus solitariis,  
 pedunculis filiformibus longissimis.
- 2161 *Gypsophylla prostrata*, foliis lanceolatis lævibus,  
 caulibus diffusis, pistillis corolla campanulata  
 longioribus. Lin. Sp. pl. 581.
- 2162 *Hedera quinquefolia*, foliis quinatis ovatis  
 ferratis. Lin. Sp. pl. 292.  
*Edera quinquefolia Canadensis*, Corn. Canad.  
 99. tab. 100.
- 2163 *Hypericum*, *Androsæmum*, floribus trigynis,  
 pericarpis baccatis, caule fruticoso ancipiti.  
 Lin. Sp. pl. 1102. Hudf. Fl. Angl. 291.  
*Hypericum maximum*, *Androsæmum vulgare*  
 dictum Raj. Syn. 343.  
*Androsæmum maximum frutescens*. Bauh.  
 pin. 208.
- 2164 *Hypericum hirsutum*, floribus trigynis, calycibus  
 ferrato-glandulosis, caule tereti erecto, foliis  
 ovatis subpubescentibus. Lin. Sp. pl. 1105.  
 Hudf. Fl. Angl. 291.  
*Androsæmum hirsutum*. Bauh. pin. 280.
- 2165 *Hypericum montanum*, floribus trigynis, caly-  
 cibus ferraturis glandulosis, caule tereti erecto  
 glabro, foliis ovatis. Lin. Sp. pl. 1105.  
 Hudf. Fl. Angl. 291.  
*Hypericum elegantissimum non ramosum*, folio  
 lato Bauh. hist. 3. p. 383.
- 2166 *Hypericum supinum*, floribus trigynis axillaribus  
 solitariis, caulibus ancipitibus prostratis fili-  
 formibus,

- formibus, foliis glabris. Lin. Sp. pl. 1105.  
Hudf. Fl. Angl. 290.
- Hypericum minus supinum glabrum. Bauh.  
pin. 279.
- 2167 Hypericum pulchrum, floribus trigynis, caly-  
cibus ferratoglandulosis, foliis cordatis glabris,  
caule tereti. Lin. Sp. pl. 1106. Hudf. Fl.  
Angl. 290.
- Hypericum minus erectum. Bauh. pin. 279.
- Hypericum pulchrum Tragi. Bauh. hist. 3. p.  
383. Raj. Syn. 343.
- 2168 Hypericum elodes, floribus trigynis, foliis  
rotundis tomentosis, caule repente. Hudf.  
Fl. Angl. 292.
- Hypericum floribus trigynis, caule tereti foliis-  
que villosis subrotundis. Lin. Sp. pl. 1106.
- Ascyrum supinum villosum palustre. Bauh.  
pin. 280.
- 2169 Hypericum Balearicum, floribus pentagynis,  
caule fruticoso, foliis ramisque cicatrisatis.  
Lin. Sp. pl. 1101. Mill. Ic. tab. 54.
- Myrto-Cistus Pennæi. Clus. hist. 1. p. 68.
- 2170 Lepidium Bonariense, floribus diandris tetrape-  
talis, foliis omnibus multifidis. Lin. Sp. pl.  
901.
- Thlaspi Bonariense multiscissum, flore invisibili.  
Dill. Elth. 281. tab. 286. f. 370.
- 2171 Lotus, edulis, leguminibus subliolaribus gibbis  
incurvis. Lin. sp. pl. 1090.
- Lotus pentaphyllos, filiqua cornuta. Bauh.  
pin. 332.
- Lotus edulis Cretica. Raj. hist. 967.
- 2172 Lotus hirsutus, capitulis hirsutis, caule erecto  
hinto

- hirto, leguminibus ovatis. Lin. Sp. pl. 1091.  
 Lotus pentaphyllos filiquosus villosus. Bauh.  
 pin. 372.
- 2173 Oldenlandia biflora, pedunculis bifloris petiolo  
 longioribus, foliis lanceolatis. Lin. Sp. pl.  
 174. Fl. Zeyl. 63.
- 2174 Oxalis stricta, caule ramoso erecto, pedunculis  
 umbelliferis. Gron. virg. 161. Lin. Sp.  
 pl. 624.  
 Trifolium acetosum corniculatum luteum  
 majus rectum. Indicum. Hist. Ox. 2. p.  
 184. f. 2. tab. 17. f. 3.
- 2175 Physalis, angulata, ramosissima, ramis angulatis  
 glabris, foliis dentatis ovatis. Lin. Sp. pl.  
 262.  
 Solanum veficarium Indicum. Bauh. pin. 166.  
 Alkekengi Indicum glabrum, chenopodii folio.  
 Dill. Elth. 15. tab. 12. f. 12.
- 2176 Physalis, pubescens, ramosissima, foliis villosis-  
 viscosis, floribus pendulis. Lin. Sp. pl. 262.  
 Solanum veficarium Virginianum procumbens  
 annuum, folio lanuginoso. Hist. Ox. 3. p.  
 527. f. 13. tab. 3. f. 24.
- 2177 Physalis curassavica, caule fruticoso, foliis ovatis  
 tomentosis. Lin. Sp. pl. 261.  
 Solanum veficarium curassavicum, solano anti-  
 quorum simile, foliis origani subincanis. Hist.  
 Ox. 3. p. 527. Pluk. Alm. 352. tab. 111.  
 f. 5.
- 2178 Passiflora laurifolia, foliis indivisis ovatis integer-  
 rimis, petiolis biglandulosis, involucri dentatis.  
 Lin. Sp. pl. 1356.

Passiflora

- Passiflora arborea*, laurinis foliis, Americana.  
Pluk. alm. 282. tab. 211. f. 3.
- 2179 *Passiflora vespertilio*, foliis bilobis basi rotundatis glandulosisque: lobis acutis divaricatis, subtus punctatis. Lin. Sp. pl. 1357.  
*Granadilla bicornis*, flore candido, filamentis intortis. Dill. Elth. 164. tab. 137. f. 164.
- 2180 *Refeda alba*, foliis pinnatis, floribus tetragynis, calycibus sexpartitis. Hort. Ups. 149. Lin. Sp. 645.  
*Refeda maxima*. Bauh. pin. 100.  
*Refeda foliis calcitrapæ*; flore albo. Moris. Hort. Bleff.
- 2181 *Refeda undata*, floribus trigynis tetragynisque, calycibus quinque partitis, foliis pinnatis undatis. Lin. Sp. pl. 644.  
*Refeda minor*, foliis incis. Barr. rar. 78. tab. 588.
- 2182 *Serratula scariosa*, foliis lanceolatis integerrimis, calycibus squarrosis pedunculatis obtusis laterationibus. Lin. Sp. pl. 1147.  
*Eupatorio affinis Americana bulbosa*, floribus scariosis, capitulis contextis. Pluk. alm. 142 tab. 177. f. 4.
- 2183 *Sibbaldia procumbens* foliolis tridentatis. Lin. Sp. pl. 406. Hudf. Fl. Ang. 118.  
*Pentaphylloides pumilla* foliis ternis ad extremitates trifidis. Raj. Syn. 256.  
*Fragariæ sylvestri affinis planta*, flore luteo. Sibb. pr. hist. natur. Scot. p. 11. pag. 25. tab. 6. f. 1.
- 2184 *Silene quinquevulnera*, petalis integerrimis subrotundis,

- subrotundis, fructibus erectis alternis. Lin.  
Sp. pl. 595.
- Lychnis sylvestris lanuginosa minor. Bauh.  
pin. 206.
- Lychnis hirsuta, flore eleganter variegato.  
Raj. hist. 997.
- 2185 Silene Vallesia, caulibus subunifloris decumben-  
tibus, foliis lanceolatis tomentosis longitudine  
calycis. Lin Sp. pl. 603.
- Lychnis maritima pinguis e Corsica. Bocc.  
mus. tab. 34.
- 2186 Solanum Bonariense, caule subfrutescente subin-  
ermi, foliis cuneiformibus sinuato-repandis.  
Lin. Sp. pl. 264.
- Solanum Bonariense arboreescens, papas floribus.  
Dill. Elth. 304. tab. 272. f. 351.
- 2187 Solanum Indicum, caule aculeato fruticoso,  
foliis cuneiformibus angulatis subvillosis in-  
tegrerrimis, aculeis utrinque rectis. Lin. Sp.  
pl. 268. Fl. Zeyl. 94.
- Solanum Indicum spinosum, flore boraginis.  
Robert. Ic. 28. Dill. Elth. 362. tab. 270.  
f. 349.
- 2188 Sophora alopecuriodes, foliis pinnatis : foliolis  
numerosis oblongis villosis caule herbaceo.  
Lin. Sp. pl. 533.
- Eryum Orientale alopecuriodes perenne, fructu  
longissimo. Tourn. cor. 27. Dill. Elth. 136.
- 2189 Theligonum. Lin. Sp. pl. 1411.
- Cynocrambe Dioscoridis. Bauh. pin. 122. pr.  
59. tab. 59.
- 2190 Trigonella polycerata, leguminibus subsessilibus  
congestis erectis subrectis longis linearibus,  
pedunculis

- pedunculis communibus muticis. Lin. Sp.  
pl. 1093.
- Fœnum Græcum sylvestre alterum polyceration.  
Bauh. pin. 348.
- 2191 *Trygonella corniculata*, leguminibus pedun-  
culatis congestis declinatis subfalcatis, pedun-  
culis communibus longis subpinosis, caule  
erecto. Lin. Sp. pl. 1094.
- Melilotus, corniculis reflexis, major. Bauh.  
pin. 331.
- 2192 *Trigonella spinosa*, leguminibus subpedunculatis  
congestis declinatis subfalcatis, pedunculis  
communibus spinosis brevissimis. Lin. Sp.  
pl. 1094.
- Fœnum græcum sylvestre polyceration Creticum  
majus. Breyn. Cent. 79. tab. 33. f. 1.
- 2193 *Trygonella Monspeliaca*, leguminibus sessilibus  
congestis arcuatis divaricatis inclinatis brevi-  
bus, pedunculis molliter mucronatis. Lin.  
Sp. pl. 1095.
- Fœnum græcum sylvestre polyceration minus  
Monspeliacum. Breyn. Cent. 80. tab. 33.  
f. 2.
- 2194 *Valantia hispida*, floribus masculis trifidis her-  
maphroditici germi hispido infidentibus.  
Lin. Sp. pl. 1490.
- Galium floribus masculis trifidis omnibus plantæ  
partibus hispidis.* Zinn. goet. 233.
- 2195 *Valantia muralis*, floribus masculis trifidis her-  
maphroditico germi glabro infidentibus.  
Lin. Sp. pl. 1490.
- Rubeola echinata saxatilis.* Bauh. pin. 334.
- Cruciata muralis minima Romana.* Col. ecphr. 1.

- p. 298. tab. 297. hist. Ox. 3. p. 328. f. 9.  
tab. 21. f. 2.
- 2196 *Valantia articulata*, floribus masculis quadrifidis,  
pedunculis dichotomis nudis, foliis cordatis.  
Lin. Sp. pl. 1491.  
*Cruciata orientalis latifolia erecta glabra*. Tourn.  
Cor. 4.
- 2197 *Valantia cruciata*, floribus masculis quadrifidis,  
pedunculis diphyllis. Lin. Sp. pl. 1491.  
Hudf. Fl. Angl. 375.  
*Cruciata hirsuta*. Bauh. pin. 335.
- 2198 *Veronica*, *Chamædryis*, racemis lateralibus, foliis  
ovatis sessilibus rugosis dentatis, caule debili.  
Lin. Sp. pl. 17. Hudf. Fl. Angl. 5.  
*Chamædryis spuria minor rotundifolia*. Bauh.  
pin. 249.
- 2199 *Veronica montana*, racemis lateralibus pau-  
cifloris, calycibus hirsutis, foliis ovatis rugosis  
crenatis petiolatis, caule debili. Lin. Sp. pl.  
17. Hudf. Fl. Angl. 5.  
*Veronica chamædryoides*, foliis pediculis oblongis  
insidentibus. Raj. Syn. 281.  
*Chamædryis spuria affinis rotundifolia scutellata*.  
Bauh. pin. 249.
- 2200 *Vitis arborea*, foliis supra decompositis: foliolis  
lateralibus pinnatis. Lin. Sp. pl. 294.  
*Frutex scandens, petroselinii foliis, Virginianus*  
*claviculis donatis*. Pluk. Mant. 85. tab.  
412. f. 2.

XXX. *Observations on the Eclipse of the Sun of August 5, 1766, made at Colombes, the Observatory of the Marquis of Courtenvaux, 20° West of the Royal Observatory at Paris, and in Lat. 48° 55' 28". By M. Messier, Astronomer to the Marine of France, of the Royal Academy of Sciences at Paris, and F. R. S. Translated by M. Maty, M. D. Sec. R. S.*

Read Nov. 26, 1766. **T**HE Marquis of Courtenvaux having desired me to observe the eclipse of the 5th of August 1766, in his Observatory, I got thither some days before the observation, in order to verify the clocks, by corresponding altitudes of the sun, and by its transits, with an instrument placed in the plane of the meridian. It is a common refractor of 5 feet focus, which does not vary a second from the plane of the meridian. The day of the eclipse, and the next day, I took a great many corresponding altitudes, and likewise observed the sun with the transit instrument. The sky was perfectly clear at the time of these observations, as well as during the eclipse. The clock which I made use of was adjusted to the mean time; it goes very regularly. For the observation of the eclipse, I employed an excellent Gregorian telescope of two feet focus, constructed in England by the celebrated Mr. Short. The tube was



mounted on a brass parallactic machine exactly divided. There likewise was a micrometer with silk threads adapted to this instrument, which was moveable every way, in so much that it was easy to place it according to the sun's parallel, and to measure with great facility the solar diameter, as well as the distances of the cusps, and the parts of the sun which remained uncovered. To make the observation of the beginning, I had determined, by means of the micrometer, the point of the solar limb, where the contact was to happen. This was a little lower than the sun's diameter parallel to the equator; the point of contact was not distant from it above  $2' 30''$ , and the time was exactly  $5^h 43' 50''$  true time. Mr. Jeurat, who observed in the same observatory with myself, judged the beginning  $3\frac{1}{2}''$  later by a refractor of 5 feet focus.

Here follows the result of my observations :

Time by the Clock.			Tr. Tim.	Parts of the Microm.	Dist. of the Cusps.	Light Parts of the Sun.	Diameter of the Sun.
H. M. S.	H. M. S.				M. S. T.	M. S. T.	M. S. T.
0 3 29	0 0 0			1082			
5 47 14 $\frac{1}{2}$	5 43 50			Beginning of the Eclipse to a Second			
5 53 0	5 49 35			713	11 22 56		
5 58 0	5 54 35			938	14 58 27		
6 1 51	5 58 26			1660		26 30 0	
6 5 4	6 1 39			1151	18 22 28		
6 8 0	6 4 35			1986			31 42 16
6 11 0	6 7 35			1288	20 33 42		
6 14 0	6 10 35			1423 $\frac{1}{2}$		22 43 27	
6 18 13	6 14 48			1413	22 23 25		
6 20 54	6 17 29			1230 $\frac{1}{2}$		19 38 36	
6 27 7	6 23 43			1248		19 55 23	
6 30 1	6 26 37			1513	24 9 12		
6 36 0	6 32 36			1534	24 29 19		
6 38 23	6 34 59			1192		19 1 44	
6 42 9	6 38 45			1988			31 44 11
6 45 34	6 42 10			1514 $\frac{1}{2}$	24 10 38		
6 57 25	6 54 1			1399	22 20 0		
7 4 26	7 1 2			1289	20 34 39		
7 8 36	7 5 12			1183	18 53 7		
7 11 54	7 8 30			1053	16 48 35		
7 16 43	7 13 19			905 $\frac{1}{2}$	14 27 18		
7 22 22	7 18 58			643	10 15 53		
	7 22 36						

The sun disappears behind the trees of the park, but very near the horizon, and the eclipse is almost

XXXI. *A Letter from the Prince de Croy to the Earl of Morton, President of the R. S. containing the Observations of the Eclipses of the Sun of the 16th of August 1765, and of the 5th of August 1766, made at Calais, together with some Remarks on the first of these Eclipses: Translated from the French, by Mathew Maty, M. D. Sec. R. S.*

Calais, August 9, 1766.

My Lord,

Read Nov. 27, 1766. **I** Have the honour to send your lordship, according to your desire, the curious observation, which I made of the eclipse of last year; and likewise inclose with it the observation we just now have been making, and which, though less curious, (because this part of the moon has neither pits nor mountains), confirms the former with regard to the height of the cusps, or that small elevation which is seen against the sun's cusps. I do not send the figure of the latter, because it is nearly the same thing, excepting that it was, as you know, towards the lower part of the sun. I think I have seen enough to conclude, that the atmosphere of the moon extends to at least double the height of the highest mountains

in it, and that it has some dry cavities, which sink below the disk. These two remarks, having been thought curious by Mr. Le Monier, and Mr. Caffiny, I have taken the liberty to communicate them to you; and have the honour to be,

My Lord,

Your Lordship's

most obedient humble Servant,

Le Prince de Croy.

*Observation of the Eclipse of the Sun, of the 16th of August 1765, at Calais, near the Steeple, in the same Place, where the central and annular Eclipse was observed.*

Beginning	-	-	-	3 <sup>h</sup>	50'	46''*
End	-	-	-	5	8	17 $\frac{1}{2}$
The duration, of which I am very certain	1	17	31 $\frac{1}{2}$			
The greatest magnitude was of 2 <sup>dig.</sup> 50 <sup>min.</sup>						

The points of ingress and egress are marked as they appeared to us in the three refracting telescopes, two of which had very nice micrometers, and in the large

\* This might have happened 1'' sooner, viz. at 45'', on account of the uncertainty of the ingress. I, however, think the former determination just, because I, at that instant, was pointing with the great refractor towards the spot, where the moon entered, and I was struck with the suddenness of the ingress of the penumbra.

refractor.

refractor. These, as well as the size of the cusps and the number of digits, were verified by Mr. de Fourcroy, chief engineer, who was so good as to assist us, and by Mr. Mouron, who assisted Mr. Blondeau in the observation of the annular eclipse.

There appeared hardly any sign of an atmosphere, except against the cusps of the sun, which seemed something bigger at the two sides, which touched the moon. There is no indication of one any where else, unless that the disk seems not so well defined at the ingress and egress. These two remarks would induce me to believe there is a small atmosphere; and this is the subject of a separate memoir.

I saw very distinctly with the reflector of 4 feet 3 inches (which answers a common telescope of 70 or 80 feet) and with the acromatic telescope, several mountains, and particularly five, which I have delineated (see TAB. XII), besides some inequalities. I always saw them in their fixed places, whatever motion was given to the telescope.

N<sup>o</sup> 1. Emerged from the sun's left limb or

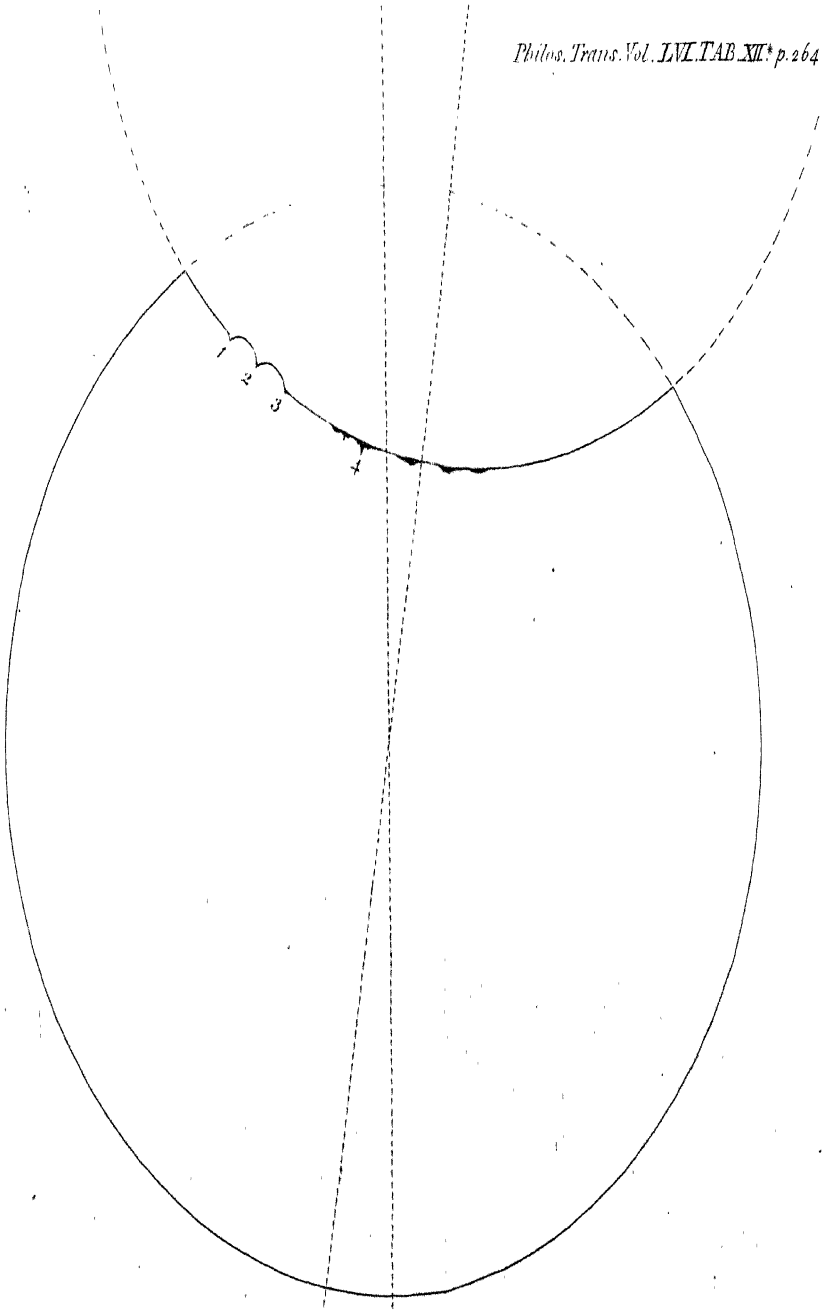
cusps at	-	-	-	-	4 <sup>h</sup>	32'	6''
2.	-	-	-	-	4	35	18
3.	-	-	-	-	4	38	6

These seemed to have their summits at about an equal distance from one another.

The mountain N<sup>o</sup>. 4. which was the highest and well defined, made its exit at 4<sup>h</sup> 45' 0''; it was accompanied with a smaller but well defined one a little on the left. On the right hand several inequalities remained; but the right limb of the moon had none, and was smooth and perfectly well defined.











We all four very well saw these mountains, and we certify their existence. The three first seemed to have hollows between them, which penetrate a little into the moon; all the rest projected out of the disc.

The clock was verified by the meridian the six following days. The whole is referred to the meridian and point of Calais; and it appears that the time marked for the new moon of that day requires a rectification. Signed,

Prince de Croy, De Fourcroy, chief Engineer,  
Garnier, Engineer: Mouron.

*Remarks on the Eclipse of the 16th of August 1765 at Calais, with regard to the Atmosphere, and the Mountains seen in profile.*

*The Atmosphere.*

Upon mature consideration, I think the moon's atmosphere perceptible, see Fig. 1. 1. Because the two small elevations, which I constantly saw of the sun's limb, towards the cusps, is easily distinguished in the large telescope, through which the other parts appeared very exactly defined. It is true that the sun appears equally bright at the place of this small elevation, but the atmosphere cannot affect its brightness, and can only somewhat raise the rays, which pervade or slide over a denser medium.

2. Because this elevation is gradual, rising higher towards the moon, according to the degree of the density of its atmosphere.

3. Because this elevation of the sun against the cusps seemed to me a little higher than the mountain N<sup>o</sup>. 4. which jetted out the most on a side view.

4. Because the penumbra, which I observed at the ingress and egress, and which certainly is much less defined than towards the middle, is very probably occasioned by the mixture of the rays, which are refracted in passing through the lunar atmosphere. This appears to me more likely than that it should be caused by the inflexion of the light, from the globe of the moon; because such inflected light sliding upon the globe ought rather to lessen its dark limb; whereas, on the contrary, the penumbra, which was sufficiently thick, served, as we all observed, to enlarge that limb.

That the telescope was clear, appeared from the spots of the sun appearing exactly defined. A parcel of these was seen somewhat below the middle on the right, and above it on the left.

#### *The Mountains.*

The most surprising circumstance to me, as I observed to the gentlemen present, and made a sketch of, after half an hour's continual attention, was that of the mountains seen in profile, and perfectly well defined; N<sup>o</sup>. 4. in particular with the small elevation on the left, had several inequalities and protuberances, which I have drawn exactly enough, though with a pencil not sufficiently sharp; I could have been more correct as to particulars, had I had a drawing pen\*.

\* Mr. Short told me in the month of July 1766, in London, that he had had a side view of these mountains with his telescopes of 8 and 12 feet.

The three summits of N°. 1, 2, and 3, were very distinct, and especially the hollows between them, surprizing on account of their depth, for they made cavities within the circle of the disc. I am no longer astonished at the luminous points, which are seen starting from the crescent; but I wonder that no notice should hitherto have been taken of these mountains seen sideways in the eclipses of the sun: a four feet telescope well fixed is necessary to observe them distinctly. Large refracting telescopes cannot serve for that purpose, as it is difficult to prevent intirely their shaking. Happily we had none of them; the air was clear, the wind to the north, and I often made the eclipse move from the top to the bottom of the field of the telescope, in order to see whether any spots in the glass did occasion this phænomenon. But the indentings were still the same, and equally well defined; and when their place was known, they were likewise seen through the other telescopes. It is only by sight that I have marked the place and distances of these mountains, as the great telescope had no wires; but, to remedy this inconveniency, I exactly observed the instant of the egress of the four chief eminences, whence their position might be ascertained: it may be, this side of the moon is one of the least mountainous parts.

In delineating the mountain, N°. 4, whose figure was so exactly defined, I thought of the atmosphere; but no trace of that is seen in that case. This, however, in my opinion, does not afford a sufficient reason for den, ing it, because the atmosphere, in that position,

must be, on all sides, pervaded by the solar rays, which must render it invisible to our sight. It is therefore only against the cusps, and that at the ingresses and egresses, that this atmosphere is to be searched for, with very large instruments, and sufficiently well fixed.

Prince de Croy.

N. B. I am inclined to believe, that there is no water in the moon, for the two cavities, which penetrated within the disk continued concave to the bottom, whereas if there had been there any water, the bottom ought to have been convex. The mountains in the moon must be very high and hollow; and that is the reason of their white circle. If ever Tycho could be seen edge ways, it would make a beautiful appearance.

XXXII. *Observation of the Eclipse of the Sun the 5th of August 1766, near the Courgain at Calais.*

Read Nov. 27,	<b>B</b> EGINNING	-	5 <sup>h</sup> 39 <sup>m</sup> 9 <sup>f</sup>
1766.	End	- - -	7 19 13

The sun did not set in the sea till about 14 minutes after the end of the eclipse; that is, at least 10 minutes later than the almanac makes it to do at Paris. His lower limb touched the sea at 7<sup>h</sup> 23<sup>m</sup> in thick vapours, which made me prefer the setting of this lower limb rather than the centre. It is easy to conclude the difference, which should result from the different situations of Paris and Calais.

No

No inequalities nor mountains were discernable, nor could any be seen on that part of the moon.

The elevation of the light of the sun's cusps was but small at the middle of this eclipse (about as much as in the last eclipse) enough, however, to indicate an atmosphere, as in my last observation \*; but the air was charged with strong vapours, which hindered me from seeing it so distinctly as I then did.

As for the rest, the weather was fine, calm and hot.

\* Mess. Mouron and Rigaut saw it also, as well as I, in my great telescope.

Le Prince de Croy.

XXXIII. *An Account of the Extrac-tion of three Inches and ten Lines of the Bone of the upper Arm, which was followed by a Regeneration of the bony Matter; with a Description of a Machine made use of to keep the upper and lower Pieces of the Bone at their proper Distances, during the Time that the Regeneration was taking Place; and which may also be of Service in Fractures happening near the Head of that Bone. By Mr. Le Cat Professor of Anatomy and Surgery at Rouen, Member of several Academies, and F. R. S. Translated from the French by J. O. Justamond, Surgeon to the First Troop of Horse Grenadier Guards.*

Read Nov. 27, 1766. **I**N the year 1751, I communicated to the academy at Rouen the case of Charles Lchee, a child of three years old, of the village of Pitre, &c. from whom I had extracted an entire tibia, exostosed and carious in its whole extent, between the two articulations, which had remained sound: this great deficiency of bony substance was entirely supplied again by nature, and the patient acquired a new tibia, much firmer than that which he had lost. The advantages which must necessarily accrue to mankind, and the credit which surgery must acquire from observations of this sort, would induce us to hope that instances of them might become more frequent;

quent; and that the professors of the art might be thereby encouraged to attempt the preservation of limbs in all cases; where there should appear the least probability of bringing about this kind of regeneration. The observation, which I now offer to the public, is a farther proof of the powers, which nature is capable of exerting in these cases, when assisted by art. The quantity of bone regenerated is not indeed so considerable as in the preceding case; but the different age of the patient, and many other circumstances, render it equally curious and useful, and have induced me to think this account worthy to be presented to the Royal Society.

Francis Romain, called La Joye, of the village of Routot, &c. an invalid, and formerly a foot-soldier in the regiment of Languedoc, aged 41 years, received, at the battle of Rocou, a gun-shot wound in the left arm, about two fingers breadth below the head of the bone of the upper arm, which had been considerably shattered in this place by the ball.

Mr. Bouffelard, his surgeon, says, he found the situation of the wound too high to risk amputation. After seven months attendance however, the patient appeared to be cured; he was put upon the list of invalids, and stationed with one of those companies at Dieppe.

Encouraged by good health, he ventured to undertake the laborious business of a ship-wright; but the great fatigue, which this employment was attended with, produced abscesses in the arm which had been lately healed; and he was admitted into our hospital in the year 1755. I immediately made free incisions and counter-openings in the parts which con-

tained



tained the matter, and extracted some splinters of bone; I then applied a proper bandage, and after the separation of several bony fragments, the cure was completed, at least a good cicatrice was formed.

He returned to his work, which was then going forward at Rouën, and was employed in carrying wood for the construction of flat-bottomed boats. His limbs in general were strong enough to support these loads; but his left arm, in which he had received the wound, was of little use to him, being shorter and weaker than the other.

On the 15th of March, 1760, being seized with a pleurisy and peripneumony, he was again brought into our hospital. After his recovery from this disease, an abscess was formed in the injured arm, which made an opening for itself in the fore-part, larger than a bullet. The arm was deprived of all motion, strength, and connection; and the callus of the former fracture appeared to be entirely destroyed by this fresh accident. In this state of the case, the patient being brought from the infirmary into the ward designed for wounded persons, I passed my probe into the wound, and found the bone of the arm bare, and carious to a very great extent: the middle of this carious part was rotten and totally destroyed throughout its whole substance. Anodine cataplasms were applied, to abate the inflammation and swelling, which attended the ulcer.

On the 15th of April I began to put in execution the plan which I had fixed upon for his cure; the first intention of which was, to lay bare the carious part of the bone in its whole length, which was rather

more

more than three inches. The wound was then filled with pads of lint, and the second operation deferred to the next day.

It was the opinion of the by-standers, that the arm should be taken off at the shoulder-joint; but the great danger attending this kind of amputation deterred me from performing it, and induced me more particularly to consider, whether it might not be possible to save the limb.

The instance of Charles Lehee had sufficiently convinced, me that bones have the power to regenerate: it must indeed be allowed as a favourable circumstance to the vegetation of the bone, that Lehee was a child; but, although this patient was an adult, I considered that we knew not at what age nature had put a stop to this regenerative faculty, and that therefore no argument could be deduced from experience to prevent the expectation of the like success in the present case.

These considerations determined me, and on the 16th of April I performed the operation, by separating the upper and lower parts of this carious bone from their connections with the sound parts, by methods, which every operating surgeon will readily conceive. I measured the distance between the end of the bone left at the upper part, and the blade of the saw at the lower, and found it to be just three inches and ten lines.

The cavity was then filled with proper dressings; and the form of the arm, as well as its natural length, preserved by an instrument calculated to answer these intentions; the description of which I thought more

particularly deserving the attention of the Society, as it may be of service in many cases different from this.

TAB. XIII. A B C is a double screw, the turns of which are in contrary directions; these screws are moved by the handle A, in such a manner that, when each of them, B C, is screwed into its correspondent worm D E, one motion of the handle A brings the two worms D E nearer together; and a contrary motion sets them at a greater distance.

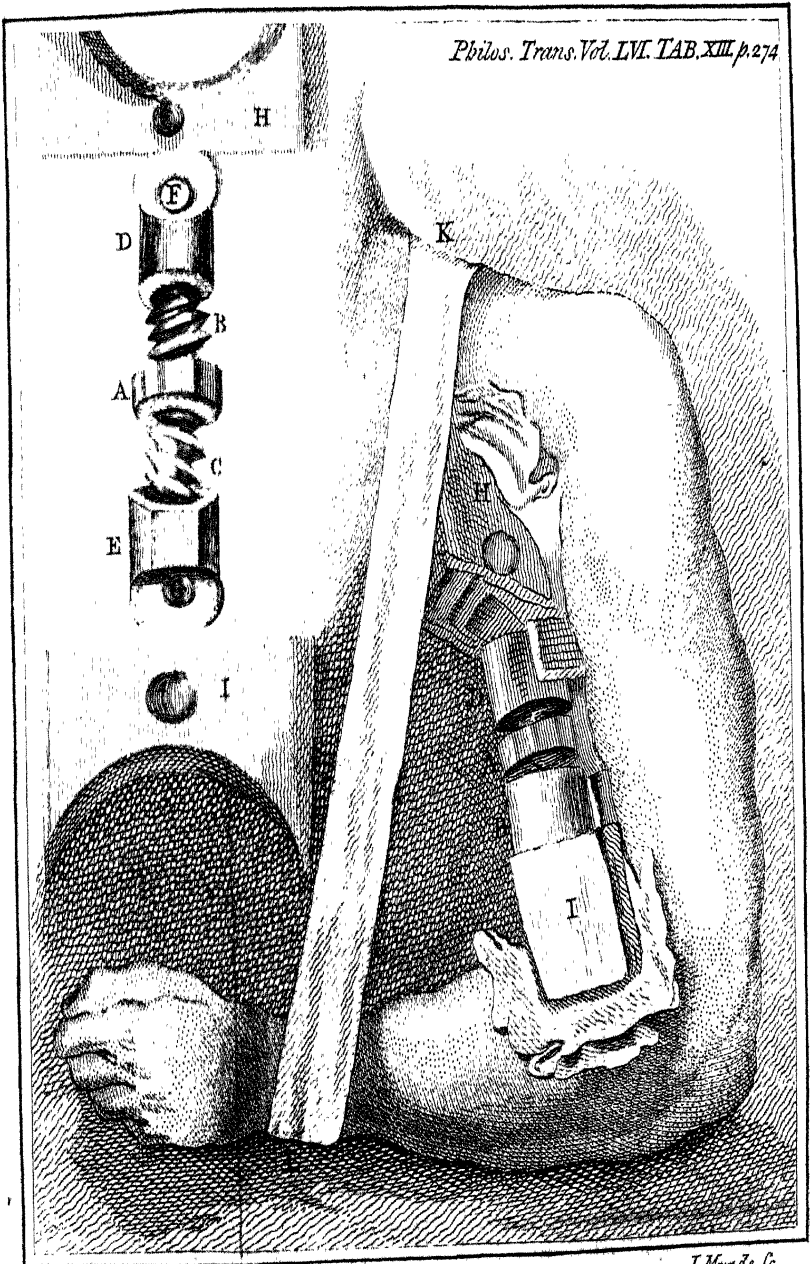
I invented this instrument about fifteen years ago, to compress the wounds of those, who had been cut for the stone, so as to prevent the passage of the urine, and thereby hasten the closing of the incision. To apply it properly in these cases, I passed a collar fastened to the neck of the patient, through the ring F, in the upper screw worm, and the bandage which supported the dressings, through the ring G, in the lower screw worm; and I have frequently experienced the success of this manœuvre.

In the year 1757, I made use of it to preserve an arm, fractured near the shoulder-joint, in its natural state of extension, by fastening the two flat rings F G, in the two pieces of wood H I; placing H under the arm-pit, and I upon the fore-arm against the bend of the elbow; and keeping the fore-arm bent by the sling K L.

I also applied this instrument in the present case; and in order to assist in giving the proper direction, and necessary solidity to the part, I supported the arm with a vambrace, or half-canal, made of one very thin piece of wood, which surrounded two thirds of the circumference of the limb. The whole  
was







*Drawn by M<sup>rs</sup> David Daughter of the Author*

*J. Mynde sc*



was fastened by the bandages commonly used in fractures.

On the 15th of May, the twenty-ninth day after the operation, the wound having filled up very fast, the arm appeared to have a sort of firmness, that the muscles alone were incapable of giving it; and on the 30th of the same month it had acquired a degree of solidity, which was nearly equal to the hardness of bone; for it was strong enough to support itself, and yielded very little to the pressure that was made upon it; but the patient was still unable to make any use of it, till the fifty-fifth day after the operation, at which time he began to move it a little.

It was scarce possible that so remarkable a case should not be attended with some accidents, in the course of the cure; so that, on the 16th of June, I was not at all surprized at the appearance of a pustule on the upper part of the scar, from which there was an oozing of matter. On the next day I passed a probe into this opening, which entered with some difficulty the length of an inch; but I did not find any splinters of bone, which I expected to meet with.

On the 18th of June, I made a large opening in this part, and extracted a point of bone, which seemed to have shot out in a very particular manner towards this pustule, and might probably have contributed, by its irritation, to have produced this fistula.

On the 19th of June, I took away the remains of this bony substance, situated underneath the above fistula, and which was but slightly attached to the neighbouring soft parts; after which the cure of this wound was completed in fifteen days.



On the 25th of July, the hundredth day after the operation, I perceived another pustule breaking out near the lower part of the scar. I then began to suspect, as there had been a flow of matter from the arm for several years, that the constitution had acquired a habit of discharging itself at this part; and attributed to that cause these trifling relapses, as well as the former more considerable one, which the patient had suffered.

From whatever spring these habitual discharges may be derived, and whatsoever may be the reasons assigned for the relapses they produce, it is well known that they are sometimes diverted or cured by issues; I therefore ordered a large caustic to be applied to the patient's left leg. The arm was at this time very solid to the touch, but still yielded a little to its own weight, as the branch of a tree, or a piece of green wood; I therefore had a bracelet of whalebone made to support and keep it steady.

On the 1st of August, the pustule, which I had perceived on the 25th of July, appeared to be much increased in size, and was spread out into fungous flesh: I passed a probe into it, and found some pieces of bone bare towards its upper part.

On the 10th of August, a small scale was exfoliated, and from that time the fungous flesh began to disappear.

In September, five months after the operation, the issue afforded a very plentiful discharge, and had attracted the humour so powerfully towards this part, that the leg on which the caustic had been applied, appeared swelled and grew painful; but, at the same time,  
the

the arm was entirely cured ; having recovered all its actions and uses, together with its proper form and length. And on the 12th of October, Romain was discharged from the hospital, very thankful for the recovery of his health, and the perfect cure he had received. He then went to rejoin his company at Dieppe, and resume his former employments.

This observation, at the same time that it furnishes a remarkable instance of animal vegetation, strongly encourages surgeons to attempt the preservation of limbs, in all cases, where there is a possibility of bringing about this sort of regeneration, so useful to mankind, and so honourable to the art.

Received August 1, 1766.

XXXIV. *A Letter from Mr. Wargentin, F. R. S. and Secretary of the Royal Academy of Sciences at Stockholm, to the Rev. Mr. Maskelyne, M. A. F. R. S. and Astronomer Royal at Greenwich, containing an Essay of a new Method of determining the Longitude of Places, from Observations of the Eclipses of Jupiter's Satellites.*

Vir reverende atque celeberrime,

Read June 19,  
1766.

ANTE triennium fere, dominus Ellicott tuo nomine ad me misit exemplar libri a te editi, *The British Mariner's Guide*, pro quo gratissimo munusculo maximas tibi ago gratias, debitasque statim persolvere voluissim, nisi eodem tempore indicasset Ellicottus noster, te tunc esse absentem, excepto videlicet longo in insulam Barbadoes itinere. Benigne excuses precor, quod jam demum officii memor, tibi gratuler felicem reditum, & munus astronomi regii nuper in te collatum, quo ut diu, in astronomiæ incrementum, fungaris, opto, & ut illustrissimorum prædecessorum tuorum æmulus, parem acquiras gloriam.

Dictus liber fuit mihi duplici nomine acceptissimus, & argumenti pertractati elegantia, & ut amicitia  
sponte

sponte mihi a te oblatæ tessera. Non honori tantum singulari, sed et certissimo usui mihi erit commercium litterarium cum primarii observatorii astronomo. Dabo operam, ut non prorsus indignus tuo favore videar.

Ne vacuæ ad te veniant primæ litteræ meæ, dissertationiunculam astronomici argumenti inferam, cum observationes alicujus momenti, quas tibi communicem, jam non habeam.

Quamvis non dubitem, quin jam diu determinaveris longitudinem geographicam insulæ Barbatorum, ope observationum ibi a te habitarum, quæ extant in novissimo Transactionum Philosophicarum tomo; non tamen ingratum tibi futurum speraverim meum videre tentamen in ea re, quod simul est specimen methodi, quâ, nullibi quidem antea distincte explicatâ, per plures annos usus sum, quando ex observationibus satellitum Jovis, in aliquo diffito loco institutis, ejus loci longitudinem elicere conatus sum.

Consuetæ & diu usitata methodus est, ut conferantur observationes habitæ in loco, cujus longitudo quæritur, cum observationibus institutis eodem tempore in observatorio quodam vel alio loco, quo longitudo determinatur. Si quæ inter illas occurrunt immediate correspondentes, ita ut eadem immersiones vel emersiones Satellitis cujusdam, præcipue primi utrinque, notatæ sint, quodvis par correspondentium suppeditat differentiam quandam meridianorum; & si hoc modo inventæ plures differentiæ aliquantulum inter se discrepant, media inter omnes sumta pro verâ vel veræ proximâ haberi solet. Satis bene succedit hæc methodus, quando plurimæ suppetunt observationes correspondentes tam immersionum quam emersionum  
primi,

primi, habitæ tubis vel telescopiis fere æqualis utrinque potentia. Et si sequamur regulas, quas huic negotio præscripsit celeberrimus astronomus Cæsareus Vindebonensis, pater S. J. Maximilianus Hell, in ephemeridibus suis astronomicis ad annum 1765, quæsitam meridianorum differentiam acû fere, quod aiunt, tangere licet, ne obstante quidem instrumentorum discrepantiâ & observatorum inæquali perspicaciâ. Laborat tamen illa methodus imperfectione quadam. Plerumque multæ in loco determinando factæ observationes nullam nactæ sunt in alio loco correspondentem; tum vero omnes illæ nullius fere in geographiâ sunt usus & quasi perditæ, quamvis fortassis per se sint præstantissimæ. Quid? si una tantum alterave, vel nulla prorsus reperiatur correspondens: contingit autem hoc sæpissime, imprimis si locus, cujus longitudo quæritur, longe remotus est versus orientem vel occidentem ab Europâ, astronomiæ sede. Sic parti longe maximæ observationum Pekinensium deficiunt correspondentes Europææ. Hujus quoque rei exemplo esse possunt tuæ, in insula Barbatorum versus finem anni 1763, & initio 1764 habitæ: nam inter 17 a te observatas eclipses primi satellitis, non nisi duæ, quantum quidem mihi constat, correspondentes habuerunt.

Ad supplendum correspondentium defectum, astronomi duplici incedere consueverunt viâ. Alii quærunnt, ane aliqua eclipsium satellitis proximæ præcedentium, vel sequentium in observatorio quodam notata sit, quâ inventâ, & addito vel subtracto revolutionum intercedentium noto numero, invenire satagunt verum tempus, quo contingere debuisset in eodem observatorio eclipsis desiderata correspondens.

Sed

Sed alexæ plena est hæc methodus, revolutiones enim satellitis inæquales sunt, ut vix duæ vicinæ perfecte conveniant. Inæqualitates plerasque eruere docet quidem theoria, sed operosus labor est & lapsui obnoxius. Nonnunquam deficiunt observationes etiam hoc modo, mediate scilicet, correspondentes, vel nimis longe sunt remotæ a tempore desiderato, vel si quæ occurrit propinquior, una pluribus non sufficit. Sic toto mense Decembri, quo tu in dictâ insulâ septem observasti emerfiones primi satellitis, unica tantum, quod sciam, in Europâ videri contigit. Si unica ista minus bona fuisset, & tamen omnes tuas ad illam exigere oporteret, ane in erroneam induceret meridianorum differentiam, tuis quantumvis per se optimis?

Melior sane est altera ratio supplendi defectum correspondentium, ope calculi ad datum meridianum facti, & addito vel subtracto tabularum errore, correcti, quem errorem aliæ observationes, eodem mense vel proximis habitæ, prodiderunt: nam hoc modo omnes observationes, in loco determinando institutæ, pro ratâ parte contribuunt ad indagandam longitudinem. Certitudo hujus methodi, quam ego potissimum expoliendam reor, ab eo pendet, ut pro quovis momento rite corrigatur error, cui tabulæ, etiam optimæ, sunt obnoxix. Facile hoc fieret, si error iisdem annis vel saltem mensibus constans esset. Sed experientia docuit, observationes, circa idem tempus factas, quamvis ipsis observatoribus fere æqualiter bonæ sint visæ, inæqualiter tamen a calculo recedere; immo eandem immerfionem, vel emerfionem, a diversis astronomis notatam, sæpe multis secundis quin & integro minuto differentem dare calculi errorem, non totum tabulis vitio vertendum; sed quâ partem

ipsis observationibus. In hoc casu, valde frequenti, arduum est invenire verum tabularum errorem, nam, insciis observatoribus etiam exercitatissimis, sæpissime irrepunt defectus, ut pro certis venditent observationes vitii non expertes. Si itaque, ut plerumque fit, una tantum alterave seligatur observatio pro inveniendò tabularum errore, facile in devia itur.

Hiscè rationibus persuasus, necessarium omnino duco examinare ipsas observationes, antequam adhibeantur determinandæ per quamcunque methodum longitudini. Fieri id potest modo sequenti.

Omnes, quæ suppetunt, observationes ejusdem satellitis, præcipue primi, habitas circa idem tempus, tam in loco determinando quam in observatoriis satis antea determinatis, dispono, secundum temporis seriem, in tabulam. Differentiam meridianorum quæsitam tantisper assumo, quantam vel observationes, si quæ adsunt, immediatè correspondentes offerunt, vel calculus non correctus unius observationis requirere videtur. Deinde ad tabularum amussim exigo omnes observationes, & inventum calculi errorem juxta quamvis annoto. Quo factò, & attentè examinatâ errorum serie, satis patet, 1º, quænam inter observationes in locis determinatis habitas reliquis sint meliores & fide dignissimæ: 2º, quis eo tempore medius sit tabularum error; & denique, 3º, an quæsitâ meridianorum differentia major vel minor sit, quam quæ in calculo assumpta erat, quantumque vel augenda vel minuenda erit, ut quam proxime pares prodeant calculi errores.

Quod ad primum attinet, verisimillimum utique puto, immersiones, quæ plerisque aliis multo citius, & emersiones quæ serius visæ sunt, inter minus certas esse

esse referendas, etiam si bonas crediderint ipsi observatores: nam theoria satellitum Jovis, imprimis duorum inferiorum, hoc tempore adeo exulta est, ut vix dubitare liceat, quin omnes ejusdem anni vel saltem mensis immersiones fere æqualiter a calculo recederent, si observationes æquali gauderent certitudine. Idem de emersionibus seorsim dicendum. Ideoque variationes &, ut ita dicam, saltus errorum, inæquali observationum bonitati videntur tribuendæ. His aliisque circumstantiis, præsertim observatorum de quavis observatione annotationibus bene pensatis, sat in propatulo erit verus vel medius tabularum eo tempore error. Si observationes in loco determinando habitæ pariter fere a tabulis discrepant, indicio est, assumptam tantisper meridianorum differentiam esse veram: sin minus, quantum augenda, vel minuenda sit. Si immersiones majorem minoremve potere videntur meridianorum differentiam, quam emersiones, id inæquali tuborum potentiæ adscribendum, & media inter utraq; meridianorum differentia tanto certius pro verâ haberi potest, quanto plures utrinque observationes eam confirmant.

Sit exempli loco determinanda longitudo insulæ Barbatorum, ope 17 observationum primi satellitis ibi a te habitaram, mensibus Novembri et Decembri 1763, Januario et Febuario anni 1764. Reperio alias 21 observationes iisdem mensibus factas in Europâ, in locis probe determinatis, quarum duæ tantum tuis sunt correspondentes, quæ per medium dant differentiam 5 horarum, 10 minutorum, & 14 secundorum, inter meridianum observatorii Stockholmensis tuique loci. Scire lubet, an illa reliquis quoque observationibus congruat. Omnes itaque horum mensium observationes, additis ultimis in Octobri 1763, & primis in Martio 1764, in justum ordinem redigo, &



verum tempus secundum tabulas computo, quo quis in suo meridiano contingere debuisset.

Peracto calculo, apparet, immerfiones Europæas alias paucis fecundis ferius, alias citius observatas fuiffe, quam per tabulas oportuerat. Illas meliores jure puto, has, præcipuè quæ maxime difcedunt, inter dubias refero. Per medium, immerfiones omnes calculo quam proxime convenire debuiffe videntur. At inter 15 in Europâ observatas emerfiones, pleræque calculum aliquot, ubi maxime, 31 fecundis præverterunt, exceptis tribus Tyrnavenfibus, quæ computatis momentis ferius viſæ, ideoque vitio non carere cenſendæ ſunt. Media bonarum emerfionum præceſſio fuit 20 fecundorum circiter.

Conſiderato ulterius tuarum obſervationum calculo, vidi eas fere omnes, poſitâ differentiâ meridianorum  $5^h 10' 14''$ , multo magis prævertere computum, quam Europæ, ſed fere æqualiter a tabulis diſceſſuras, ſi differentiam meridianorum 22 fecundis auxerim. Eam itaque  $5^h 10' 36''$ , quam proxime æqualem autumo; cujus rei ut fidem tibi faciam, heic ſubjuncto omnes obſervationes, cum correcto tuarum calculo.

Parifienſes habitæ ſunt teleſcopio Gregoriano, diametros objectorum  $104^a$  augente, à Domino Meſſier, in Obſervatorio Societatis Maritimæ, quod duobus temporis fecundis orientalius eſt obſervatorio regio.

Viennenfes, à Rev. Patre Hell, S. J. teleſcopio  $4 \frac{1}{2}$  pedum, in obſervatorio Cæſareo, cujus à Parifienſi longitudo præciſe eſt ſtabilita  $56' 11''$ .

Tyrnavenfes a R. P. Weiſs, S. J. teleſcopio 4 pedum Newtoniano. Differentia meridiani Tyrnaviæ à Parifienſi obſervatorio eſt  $1^h 1' 56''$  ſatis certa.

Meæ habitæ ſunt tubo Dollondiano 10 pedum egregio. Diſtat autem obſervatorium Stockholmenſe à Parifienſi  $1^h 2' 51''$ , & à Grenovicenſi  $1^h 12' 7''$ .



Jam ipsi tibi, vir celeberrime, relinquo dijudicandum, anne sensibilibiter majorem vel minorem admittant hæ observationes meridianorum differentiam? Tuarumne idem fere inter se & cum tabulis consensus, atque est Europæarum? Videsne, quasi ictu oculi, quæ observationes erroris sunt suspectæ, quæ reliquis præferendæ? Quas ipse dubias notasti, calculus quoque arguit. Emerfio die 6 Dec. omnium maxime, sine dubio, est vitiosa. Inæqualem tuborum potestatem etiam indicat errorum series: nam Weiffius suo fere semper observavit immerfiones citius, & emerfiones tardius, quam reliqui. Tuum telescopium meo Dollondiano fere palmam præripere videtur. Si differentia meridianorum inventa non est exacte vera, eam potius aliquot secundis augendam quam minuendam puto, sed paucis.

Pari ratione differentiam meridianorum observatorii Grenovicensis & insulæ S. Helenæ inveni 23 minutis primis vix majorem, sed potius aliquot secundis minorem, nisi duæ ex emerfionibus 15 ibi a te observatis sint prorsus rejiciendæ: immerfiones enim solæ paulo majorem admitterent.

Persuafus sum, hanc methodum determinandi longitudes per observationes satellitum Jovis esse reliquis præferendam, quod evidentior & generalior sit, & præcipue quoniam simul indicat observationum majorem minoremve præstantiam. Sed satis de his.

Vale, vir æstumatissime, & fave,

Reverendi celeberrimique nominis tui,

Cultori sincero,

Dab. Stockholmiæ,  
die Julii, 1766.

Petro Wargentia.

XXXV. *A Letter from John Ellis, Esq;  
F. R. S. to the President, on the Coluber  
Ceraftes, or Horned Viper of Egypt.*

My Lord,

Read Dec. 11. 1766. **T**HE Coluber ceraftes, or horned viper of Egypt, which I have the honour to present a specimen of to this illustrious Society, I am informæd, is very rare, and scarce to be found in any of the cabinets of natural curiosities in Europe. Besides, the authors who have treated on the Ceraftes, as Alpinus and Bellonius, have given such unsatisfactory descriptions of it, and inaccurate figures, that I thought an exact drawing from nature, together with the best and latest systematical account of it, would be agreeable, as well to the lovers of antiquity as natural history.

The ancient Egyptians most certainly esteemèd it a hieroglyphic of some importance; for when we examine their monuments of the greatest antiquity, such as their obelisks, temples, statues, palaces, and even their mummies, we are almost sùre to find many representations of it on them. Those two immensely large stones, lately brought from Alexandria, in Egypt, now in the court-yard of the British Museum, which appear to be part of the grand cornice of some magnificent palace, have many figures of the Ceraftes curiously engraved upon them.

Dr.

Dr. Haffelquist, a pupil of the celebrated Linnæus, who was in Egypt in 1750, has given us a particular description of this curious animal; but neither he nor the former writers on Egypt, that mention the *Cerastes*, say any thing about the venom of its bite. This we are informed of only by Dr. Turnbull, who lived many years in Egypt, both at Alexandria and Cairo, and who was so kind to present me with two specimens of it.

Dr. Linnæus, in his *System of Nature*, p. 217, calls it *Coluber cerastes*.

Dr. Haffelquist, in his *Iter*. p. 315, *Coluber cornutus*; the following is an extract from his description.

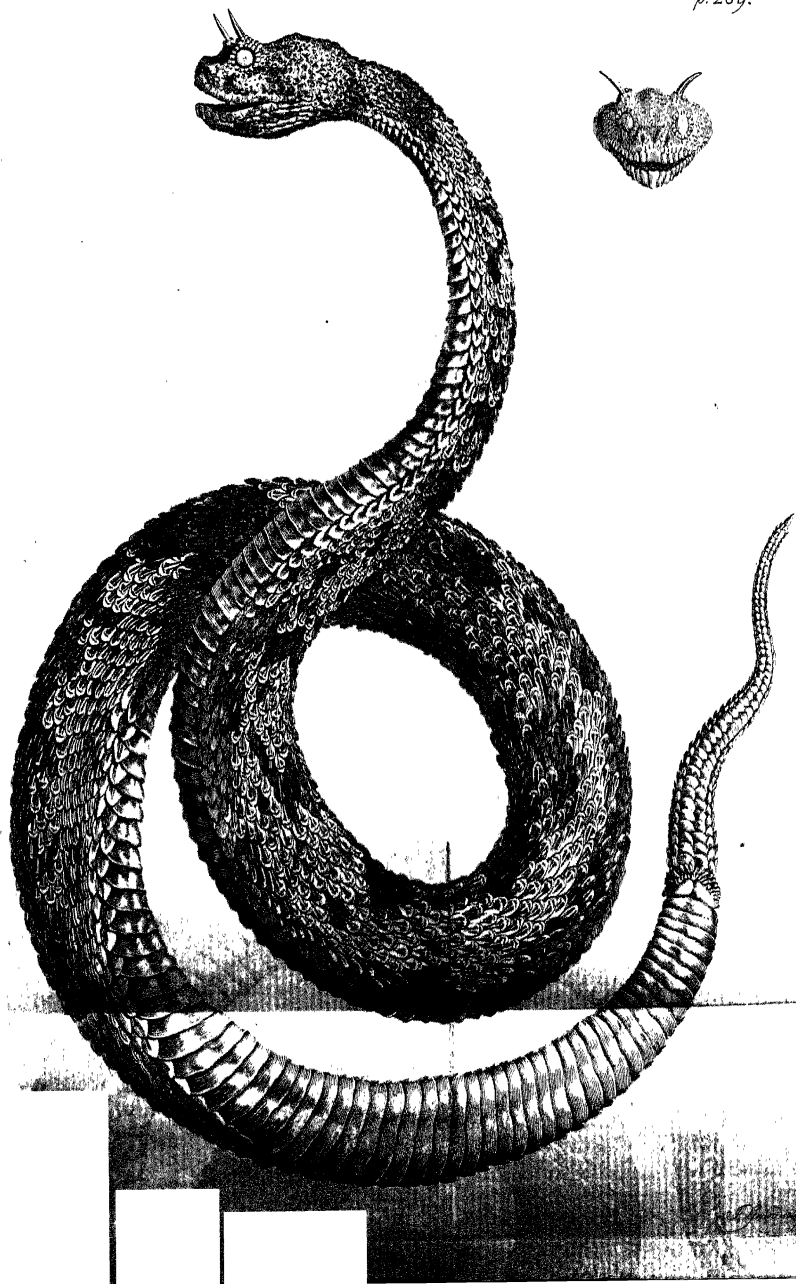
The head, between the horns, is much depressed; the cheeks are swelled out, so that the hinder part of the head is considerably thicker than the neck; the snout is short and blunt; the outward front of the upper and under jaws have a small cavity, or depression, in both; the nostrils project like those of a pug dog.

The eyes have a perpendicular narrow and black pupil; the iris is of a yellowish grey colour; the orbits of the eyes are neatly set round with small hemispherical scales.

The tongue is divided at the extremity into two parts.

The











**The teeth.** In the upper jaw there are no teeth, but two bones placed lengthways in the palate; in them are fixed several small teeth, generally about ten: they sharp, of an equal length, and bend a little towards the throat. On the sides of the under jaw, near the snout, are placed three or four teeth; but none quite in the fore part or hinder part.

**The horns.** Just above the eyes, near the upper part of their orbit, are two tentacula, which we call horns, about a quarter of an inch long; they are not straight, but bend a little outwards; they are channelled lengthways, sharp-pointed, but not very hard; their basis is surrounded with a circle of small erect scales.

**The body** is narrow towards the neck; the diameter of the thickest part of the middle about one inch; the tail grows suddenly taper, and ends in a sharp point.

**The colour.** The top of the head, the back, and upper part of the tail, are variegated with large irregular spots, of a bright ochry colour, or reddish brown; the throat, belly, and under part of the tail, are whitish.

**The length** of this specimen (see TAB. XIV.) is as follows; from the nose to the anus  $22 \frac{1}{2}$

inches, the tail  $3\frac{1}{2}$  inches; so that the whole serpent is 26 inches long.  
 The Belly is covered with 145 broad scales, or scuta; the tail with 43 pair of small scales, or squamæ.

The number of squamæ and scuta have been thought by late authors to be the best method of determining the species of serpents; but they are not ignorant that they differ a few now and then: Hasselquist reckoning 150 scuta, and 50 pair of squamæ, to his *Coluber cornutus*.

I am, my Lord,

your Lordship's

most obedient humble servant,

To the Right Hon. the Earl of Morton,  
 President of the Royal Society.

John Ellis.

XXXVI. *Abstract of a Journal of the Weather in Quebec, between the 1st of April 1765, and 30th of April 1766. By Cap. Alex. Rose, of the 52d Regiment; communicated by the Rev. P. Murdoch, D. D. F. R. S.*

Read December 18, 1766.

Days	Height on Fahrenheit's Scale.		inds	Weather
	Greatest	Least		
1765				
Apr. 1	34½	30	E.	{ Sunshiny morning; afternoon overcast; night snow
	53	31	N.W.	{ calm sunshiny day; at night frost
10	39	30	E.	{ Fine sunshiny day, brisk wind
15	60	31	N.W.	{ Sunshiny day; evening rain
20	41	38	N.W.	{ High wind the whole day; night hard frost
25	47	39	E.	{ Ditto
27	44	38	E.	{ Ditto
30	60	49	E.	{ Ditto
May 1	56	45	S. E.	{ Overcast morning; afternoon heavy rain
	59	47	E.	{ Sunshiny day, brisk wind
10	54	37	N.	{ Ditto ditto
12	73	43	W.	{ Ditto ditto
15	44½	39½	E.	{ Overcast the whole day, brisk wind
20	56	41	S. E.	{ High wind with rain
27	85	58	W.	{ Sunshiny day, morning calm, afternoon high wind
31	67	46	W.	{ Fine sunshiny day, little wind.

Days	Height on Fahrenheit's Scale.		Winds	Weather
	Greatest	Least		
1765				
June 1	67	53 $\frac{1}{2}$	W.	Fine sunshiny day; afternoon E.
	380	63	W	{ Sunshiny morning; afternoon cloudy with rain
	569	51	E.	Sunshiny day, little wind
	987	64	S.	Sunshiny day; afternoon W.
	1552	45	N.W.	Cloudy day, brisk wind
	1969	54	E.	Sunshiny day; night rain
	2562	47	N.W.	Cloudy day, brisk wind
	2768	50	N.W.	Fine calm sunshiny day; night rain
	2974	61	W.	Ditto ditto
	3062	60	E.	Sunshiny day, brisk wind ditto
July 1	60	55	E.	Rain the whole day
	572	58	W.	{ Morning sunshiny; afternoon thunder with rain
	1080	62	S. W.	Sunshiny day
	1573	58	E.	Ditto
	1984	65	W.	Ditto
	2377	60	E.	Ditto
	2884	76	N.	{ Morning sunshiny; afternoon high wind with thunder
	3071	56	W.	Brisk wind with flying clouds
Aug. 1	69	53	N. E.	Ditto ditto; aftern. S. W.
	576	57	W.	Sunshiny morning; afternoon showery
	979	64	S. W.	Ditto
	1462	50	N.W.	Sunshiny day, brisk wind
	1766	47	W.	Overcast the whole day; night much rain
	2167	51	W.	Fine sunshiny day
	2874	68	E.	Ditto
	3169	56	W.	Ditto

Days	Height on Fahrenheit's Scale.		Winds	Weather
	Greatest	Least		
1765				
Sept.	1	69	52 W.	Ditto, brisk wind
	3	70	55 E.	Ditto, calm
	5	68	59 E.	Rain the whole day
	12	50	40 N. W.	Sunshiny day, brisk wind
	17	64	47 W.	Ditto, calm
O&E.	26	40	33 N. W.	Overcast the whole day; afternoon E.
	29	52	38 S. E.	Sunshiny day; afternoon S. W.
	1	56	47 S. W.	Sunshiny day
	6	56	39 W.	Ditto
	9	60	53 S. E.	Fog with rain the whole day
	11	38	34 W.	{ Overcast, with high wind; some snow for the first time
	12	41	28 W.	Sunshiny calm day
	20	55	54 E.	Rain with high wind the whole day
	23	32	19 W.	Sunshiny calm day
	29	40	36 W.	Overcast the whole day
Nov.	31	35	34 E.	Heavy rain the whole day
	1	40	30 S. W.	Fine calm sunshiny day
	5	31	26 W.	Cloudy with snow
	11	40	37 W.	Foggy morning; afternoon sunshiny
	14	31	30 E.	{ Heavy storm of snow, from this day the ground remained covered
	18	36	33 W.	Overcast the whole day
	23	20½	12 W.	Sunshiny day, little wind
Dec.	26	22	7 W.	Ditto; afternoon E.
	30	31	30 W.	Overcast, some snow
	1	21	18 W.	Sunshiny day
	6	13	2 S. W.	Ditto
	9	29	29 E.	Overcast, little snow
	14	25	4 W.	Sunshiny day
	18	31	25 E.	Overcast, little snow
	23	0	12 W.	Sunshiny day
24	7	11 W.	{ Ditto morning, snow afternoon	



Days	Height on Fahrenheit's Scale.		Winds	Weather
	Greatest	Least		
1765				
Dec. 25	29	0	W.	Overcast the whole day
30	<sup>b</sup> 17	<sup>b</sup> 24	W.	Sunshiny day, brisk wind
31	<sup>b</sup> 15	<sup>b</sup> 29	W.	Ditto
1766				
Jan. 1	<sup>b</sup> 3	<sup>b</sup> 28	W.	{ Morning overcast, little wind; after- noon brisk wind E.
2	12	<sup>b</sup> 5	E.	High wind, little snow
3	10	<sup>b</sup> 14	W.	Brisk wind and cloudy
4	<sup>b</sup> 6	<sup>b</sup> 19	W.	Brisk wind, sunshiny day
5	25	<sup>b</sup> 25	W.	Overcast the whole day
10	25	19	W.	Ditto
12	0	<sup>b</sup> 5	W.	High wind, clear weather
14	29	9	E.	High wind, overcast
16	10	<sup>b</sup> 10	W.	Ditto ditto
17	<sup>b</sup> 2	<sup>b</sup> 17	W.	Ditto, clear weather
21	37	33 <sup>1</sup> / <sub>2</sub>	S. E.	{ Foggy with rain the whole day (earth- quake at 5 o'clock next morning)
23	<sup>b</sup> 10	<sup>b</sup> 23	N. W.	Brisk wind
24	2	<sup>b</sup> 25	W.	Sunshiny day
25	22	10	E.	Morning snow; afternoon clear
31	27	4	S. W.	Sunshiny day
Feb. 1	6	<sup>b</sup> 5	W.	Sunshiny day, high wind
2	6	<sup>b</sup> 10	W.	Ditto, little wind
7	28	25	E.	Overcast, high wind
14	39	18	W.	Calm sunshiny day
20	6	<sup>b</sup> 6	W.	Sunshiny day, brisk wind
23	26	16	E.	Heavy fall of snow, brisk wind
25	5	<sup>b</sup> 10	W.	Calm sunshiny day
28	35 <sup>1</sup> / <sub>2</sub>	28	S. W.	Overcast the whole day, brisk wind
Mar. 4	19	3	S. W.	Sunshiny day, high wind
7	36	26	S. E.	Brisk wind, with flying clouds

Days	Greatest Heat on Fahrenheit's Scale.		Winds	Weather
	Greatest	Least		
Mar. 12	40	27	E.	Flying clouds, with snow
14	29	<sup>b</sup> 2	N.W.	High wind, with snow
15	2	<sup>b</sup> 10	N.W.	High wind the whole day
16	23 <sup>1</sup> / <sub>2</sub>	<sup>b</sup> 5	S. E.	Fine calm sunshiny day
17	30	31	S. E.	Overcast and showery the whole day
23	27	22	E.	Snow the whole day, brisk wind
27	32	9	W.	Sunshiny day
31	46	29	N.	Ditto
Apr. 1	42	25	W.	Ditto, afternoon E.
3	46	19	W.	Ditto
10	42	36	E.	Ditto, little wind
13	48	35	W.	Ditto
19	42	19	W.	At night snow; the least snow this season
22	27	26	W.	Sunshiny day
29	51	31	W.	Ditto
30	42	30	E.	Rain and overcast the whole day

N. B. The letter <sup>b</sup> placed over any figure, signifies that the Mercury was so many degrees below 0. Thus on the 31st of December 1765, you find <sup>b</sup>15, <sup>b</sup>29, that is, 15 and 29 below zero; or 47 and 61 below the freezing point. Again, on the 5th of January 1766, the numbers are 25 and <sup>b</sup>25, which is 25 above, and 25 below 0. So that the difference between the highest and lowest degrees of heat on that day, is 50 degrees.

On the 27th of May (1765), when the Mercury rose to 85, and fell no lower than 58 (the mean of which is 71<sup>1</sup>/<sub>2</sub>) at Quebec, at a place down the river, called Mont Louis, was a heavy storm of snow, which continued the whole day: the distance of these places is about 260 miles.

Alexander Rose,

Lieutenant in the 51st Regiment.

XXXVII. Dec-



Received July 4, 1766.

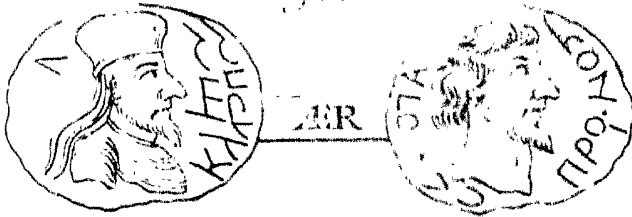
XXXVII. *Description of two Parthian Coins, never hitherto published. By the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany, in a Letter to Charles Morton, M. D. Sec. R. S.*

Dear Sir,

Read Dec. 18, 1766. **T**OWARDS the close of last month, a few days after my visit to you at Montague-House, I met with two antient brass coins, pretty well preserved, that had formerly had a place assigned them in the valuable collection brought by the Right Reverend Dr. Pococke, Lord Bishop of Meath, out of the East. The workmanship of these pieces seems considerably different from that both of the Parthian coins, hitherto published, and those struck by the Persian princes of the house of Saffan. It somewhat, however, resembles that of the brass medals of one or two of the later Parthian kings. As I take them never to have appeared before in this part of the world, and highly to merit the attention of the curious; I shall beg leave, without any farther apology, to transmit accurate draughts of them to the Royal Society, together with a few cursory observations upon them, which may, perhaps, prove not altogether unacceptable

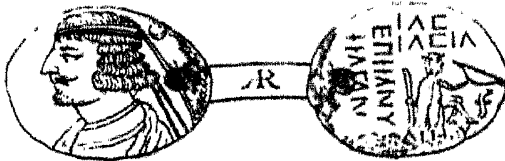


Fig. 1.



*Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.*

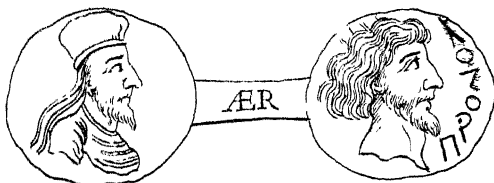
3.



*Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.*

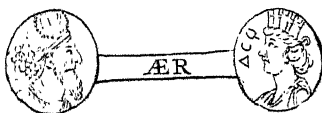
*Philos. Transf. Vol. LVI. TAB. XV. p. 296.*

2.



*Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.*

4.



*Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.*



unacceptable to that most illustrious and very learned body.

At first, from several slight circumstances, I attributed these coins to one of the Persian monarchs of the house of Sassan; but examining them afterwards, with greater attention, I was rather inclined to believe, that they appertained to one of the latter Parthian kings. This, upon a farther inquiry, may perhaps appear probable, from the following considerations.

1. The workmanship of these pieces, however rude and inelegant, seems tolerably well to agree with that of several brass medals of the later Parthian kings; whereas it almost totally differs from that of the coins generally assigned the Persian monarchs, posterior to those kings.

2. The tiara of the prince exhibited by the medals before me is somewhat similar to those handed down to us by two Parthian coins, attributed, by Dr. Vaillant, to Vologeses II. and Artabanus IV.

3. These medals may naturally enough be supposed to have first appeared in some neighbouring province, reduced by the Parthian arms, rather than in the interior part of Parthia itself; which may possibly, in some measure at least, account for the rudeness and inelegance of taste they apparently present to our view.

4. The coins in question seem to have had some Greek characters originally impressed upon them. If this be admitted by the learned, those letters may possibly be thought to point at a Parthian mint. For we have scarce ever hitherto met with any Greek

characters on the medals (1) generally ascribed to the Sassanian kings.

5. The pieces under consideration here have likewise several unknown characters upon them, which can by no means be deemed the same with those preserved on the Persian coins struck by the princes of the house of Sassan.

These two medals are of the size of the smaller middle Roman brass, or nearly so. Their workmanship, as has been just remarked, is inelegant, or rather somewhat rude. They are so similar to each other, that they may be considered, without any great impropriety, as duplicates of the same medal. Both of them, on the anterior part, seem to have retained the effigies of the same Parthian king; and, on the reverse, they both exhibit a human head, with the hair formed into curls, on which is just visible a rude sort of crown. Before the face of the latter, the Greek elements Π, Ρ, Ο, Ζ, Ο, Υ, or ΠΡΟΖΟΥ, on both plainly enough appear; though one of them only presents to our view, before the face of the Parthian king, a complex

(1) We meet, however, with a Greek date on a little brass coin, very ill preserved, in my small collection, which seems to indicate the piece to have been struck by one of the princes of the house of Sassan. That date was undoubtedly formed of the letters ΔΟΦ, so that the first appearance of the medal must be allowed posterior to the dissolution of the Parthian empire, whether we adopt F. Corfini's Parthian Æra, or that followed by Dr. Vaillant. The coiffure of the prince's head is exactly the same with that peculiar to the Sassanian kings. The globe also, above the diadem, and the large tuft of hair here, seemingly adorned with pearls or other precious stones, and forming another kind of globe, are frequently visible on the medals appertaining to those kings. The turrited head, on the reverse, represents the genius of the city where the coin was struck. *Philos. Transact.* Vol. LVI. TAB. XV. N. 4.

character, or monogram, seemingly composed of the Greek letters E, A, and three or four unknown characters, that have suffered a little from the injuries of time. Such are the coins now described, and the proper objects of my attention here.

With regard to the word, or rather name, ΠΡΟΖΟΥ, I shall not hesitate a moment to read and pronounce it ΠΕΡΟΖΟΥ; instances of the omission of a Greek letter, according to the (2) writers here referred to, having been met with on the Parthian coins. Nay, I find in my small cabinet an inedited Parthian coin, with the following imperfect inscription upon it, ΒΑΣ :: :: :: ΒΑΣΙΑ :: :: :: ΔΙ :: :: :: ΕΠΙΦΑΝΤ :: :: ΦΙΛΕΑΝ :: :: :: for ΦΙΛΕΛΛΗΝ :: :: to omit others that might, with equal facility, be produced. Nor can this be matter of surprize to any one who considers, that Greek words are sometimes very inaccurately expressed on those coins. The unusual curls, on the reverse, may possibly be thought to point at Armenia (3), as the country where these pieces were struck; especially, as the complex character, if it is a monogram formed of the Greek elements E, A, or EA, seems to direct us to the city of Elegia in Armenia, where a whole Roman army (4) was cut off by Vologeses II. And this will appear

(2) Edvard. Corfin. *Ad Clariss. Vir. Paul. M. Paciaud. Epist. Romæ*, 1757. *Erasmi. Frcel. Reg. Vet. Numism. Sc. Tab. I. N. X. Viennæ Austriæ*, 1753. Vid. etiam *Saggi di Dissertazion. Accademich. Sc. di Corton. Tom. VII. p. 203. In Roma*, 1758. *Nov. Act. Eruditor. Lips. Mens. August. 1758. p. 436. Lipsiæ*, 1758. & *Philos. Transf. Vol. LVI. TAB. XV. N. 3.*

(3) *Philosoph. Transact. Vol. L. Par. I. p. 186.*

(4) Jul. Capitolin. in *M. Antonin. Philosoph. Lucian. in Vit. Alexand. Pseudomant. p. 486, 487.*



still more probable, after we have discovered the monarch denominated Perozes, or Peroz, and the reason of that name. To what prince therefore, and on what occasion, the name, or rather surname, Perozes was applied, I am next to inquire.

Vologeses II. having finished his preparations for a war with the Romans, in the reign of Antoninus Pius, soon after that prince's death, made an irruption (5) into the Greater Armenia. This happened, according to Dio, in the year of Rome 915, or of CHRIST 161. Meeting with little or no opposition, he advanced to Elegia, a city of that kingdom, where a Roman army, under the command of Severianus, the prefect of Cappadocia, was at that time posted. This formidable body he immediately attacked, (6) and so intirely defeated it, that scarce a single Roman found means to escape. So complete a victory as this must of course have put Vologeses in possession of the greatest part of Armenia, and particularly of the city of Elegia. After so important a conquest, the Parthian monarch may naturally enough be supposed to have caused the medals I am considering to have been struck, and that in the town of Elegia. And that this was really the case, some will perhaps allow deducible from the monogram presented to our view on one of these coins. Nay, that he derived the name, or surname, Peroz, or Perozes, itself from a successful expedition he undertook against the Romans, we learn from Moses Chorenensis, the Armenian historian. " At which time, says this " writer (7), after the death of Titus the Second,

(5) Jul. Capitolin. & Lucian, ubi sup.

(6) Dio, lib. LXXI. p. 802. B.

(7) Mos. Chorenens. *Histor. Armeniac.* lib. II. cap. LXI. p. 181. Lond. 1736.

“ king of the Romans, called Antoninus Augustus,  
 “ Peroz, or Perozes, king of the Persians, (Parthians)  
 “ made an irruption into the Roman empire; from  
 “ whence he deduced the name Perozes, which  
 “ signifies *The Conqueror*, or *The Victor*, having  
 “ before been denominated Vologesus, according to  
 “ the Greeks, but by what name he went amongst  
 “ the Persians I have not yet been able to learn.”  
 Which passage seems not only to point at the defeat of  
 Lucius Attidius in (8) Syria, but likewise at the terrible  
 overthrow given the Romans in Armenia, soon after  
 Marcus Aurelius and Lucius Verus ascended the  
 imperial throne.

I shall only beg leave at present to add, that the  
 (9) Arabic Firûz and the Persian or Armenian Peroz  
 agree intirely in signification, (10) if they may not be  
 considered as absolutely the same word; that a Persian  
 king, named Firûz by the Arabs, (11) is called Perozes  
 (Περόσης) by Agathias; that (12) Moses Chorenensis  
 and one, at least, of the medals here described mu-  
 tually strengthen and support each other; and that  
 I am, with all possible consideration and esteem,

S I R,

Your most obedient humble Servant,

Christ-Church, Oxon.

John Swinton.

June 30, 1766.

(8) Jul. Capitolin. ubi sup.

(9) Al Makîn, Greg. Abu'l Faraj, Iâm. Abulfed, alique  
 scriptor. Arab. Firûz, (فروز) is rendered *Felicitas*, *Victoria*,  
*&c.* by Golius; and by Meninski *Victoriosus*, *Felix*, *Prosper*. It  
 is a Persian word.

(10) Golius, Meninski, alique lexicograph. Arab. & Persic.

(11) Agath. Scholast. *De Imper. & Reb. Gest. Imperat. Justi-*  
*nian.* Lib. IV. p. 137. Parisiis, 1660.

(12) Moses Chorenens. ubi sup.

ERRATUM, in *Philos. Transact.* Vol. LIV.

Page \*139. note, l. 2. for פש read שת.

XXXVIII. An

Received October 24, 1766.

XXXVIII. *An Account of a successful Operation for the Hydrops Pectoris, by William Moreland, Surgeon at Greenwich; communicated by W. Watson, M. D. F. R. S.*

Read Dec. 18, 1766. **A**S very few instances are to be met with in medical or chirurgical writers, of the successful opening of the thorax in the dropfy of the breast; the following person's case, who was preserved by it in the most imminent danger of death, may encourage others under similar circumstances to perform the operation, which has hitherto been very rarely attempted.

Anne Harmsworth of Crooms Hill, Greenwich, of a thin, hectic habit of body, and subject to defluxions on the breast, about the latter end of the year 1760, complained of a smart, shooting pain in her right side, which somewhat affected her breast. Her evacuations by stool and urine were by no means deficient, nor was there any remarkable appearance on the part affected. A blister was applied, and oily medicines given, which relieved her in a few days, yet not so intirely but that she had returns of the pain at different times, though not sufficient to make her apply for advice, till November 1762, when she was seized with a much greater degree of the same kind of pain, attended with difficulty of respiration, a sense of weight  
on

on the diaphragm, and a quick pulse, with a little more heat than usual.

On the 18th of December, I saw her, for the first time, with Mr. Mills, a surgeon at Greenwich, when she related to me the above complaints, now much augmented, having a sense of fulness in that side (which was ready to burst, as she termed it) and an evident fluctuation in the right cavity of the thorax. But her left side was free from complaint. She made very little urine, and that limpid. The expectorant medicines (blister and cathartic) were administered without the least relief; her symptoms gradually increasing.

On the 1st of January 1763, she could breathe in no other situation than that of the thorax brought forward to the knees, in which posture she continued till the 30th of January, when finding the ribs elevated exceedingly, and the right side of the thorax uniformly distended, with every other reason tending to confirm the notion of a fluid's being lodged there: we, in company with Mr. William Sharp (whose opinion we had, this day, requested) proposed the operation to her, which the present pressure of her disease, and the little probability of her living long in that state, determined her to consent to.

I, then, in presence of Mr. William Sharp, surgeon to St. Bartholemew's, and Mr. Mills, made an incision, about four inches long, between the sixth and seventh ribs, (reckoning upwards) and about half way between the spine and sternum into the cavity of the thorax, and discharged from thence seven pints of limpid serum. Immediately the difficulty of breathing was removed, but a faintness succeeding seemed to endanger her

her for a short time, occasioned more by the sudden removal of the pressure from the lungs, than any other inconvenience from the operation, the loss of blood being very inconsiderable.

From this time to the next morning, the urine was secreted and discharged to the quantity of three pints more than she had drank. On the first dressing, the next day, there issued about a spoonfull of serum, but none afterwards: and though she remained weak and faint for several days, yet she had no other inconvenience, from the time of the operation to that of the cicatrization of the wound, which was completed in less than a month; the wound having been dressed superficially the whole time.

It may be remarked, that, though, at the time of the operation, she was two months gone with child, she nevertheless completed her pregnancy, and is now in as good a state as she had enjoyed for many years before.

XXXIX. *A Letter from Mr. Emanuel Mendes da Costa, Librarian, &c. to the Royal Society, to M. Maty, M. D. Sec. R. S. containing a Supplement to the Account of the Discovery of Native Tin, Art. VII.*

Dear Sir,

Read Dec. 18, 1766. **I** Communicate to you the following paragraph from a letter written to me by the Rev. William Borlase, LL. D. and F. R. S. of Ludgvan, near Penfance, in Cornwall, and bearing date November 23 last past. As it is a paragraph relative to the native tin found in Cornwall, which I had the honour to communicate to the Royal Society on the 6th of March last, and is ordered to be printed; permit me to beg of you to read it at the meeting to-morrow, that, if judged worthy by the Committee of papers, it may be printed with the former paper as a part of it.

I am, with great respect,

SIR,

your very obliged

humble servant,

Royal Society house,  
December 17, 1766.

Emanuel Mendes da Costa,

VOL. LVI.

R r

EXTRACT.

## EXTRACT.

“ Mr. Henry Rosewarne, of Truro, says, that  
 “ when he sent the first specimen (presented to the  
 “ Royal Society by me William Borlase, and now  
 “ lodged in their Museum) he mentioned as a proof  
 “ of its being native tin, that between the ore and the  
 “ tin there was a mixture of quartz: but, upon a  
 “ nearer examination and some trials with aqua fortis,  
 “ he and another person found it was not quartz. At  
 “ last, on melting a piece, he perceived no small  
 “ quantity of arsenic to be mixed with it, and there-  
 “ fore suspected that the white parts which had  
 “ passed for quartz were nothing but arsenic. Accord-  
 “ ingly he scraped off a little of it and put it on a red  
 “ hot iron, where it immediately caught fire, and  
 “ evaporated into smoke, leaving behind it the most  
 “ poisonous stench they ever smelt. This confirmed  
 “ some, who had hitherto doubted, in the most  
 “ firm belief that it really was native tin and genuine,  
 “ it being impossible for tin to be melted and the  
 “ arsenic left untouched.”

XL. *A Supplement to the Account of an Amphibious Bipes*; by John Ellis, Esq; (Art. XXII.) being the *Anatomical Description of the said Animal*, by Mr. John Hunter, F. R. S.

Read June 5, 1766. **T**HE tongue is broad and has very little motion. It has a bone similar to that in birds, turtles, &c. On the posterior and lateral parts of the mouth, are three openings on each side; these are similar to the slits of the gills in fish, but the partitions do not resemble gills on their outer edges, for they have not the comb-like structure. Above\* and close to the extremity of each of these openings externally, so many processes arise, the anterior the smallest, the posterior the largest; their anterior and inferior edges, and extremity are serrated, or formed into fimbriæ: these processes fold down and cover the slits externally, and would seem to answer the purposes of the comb-like part of the gill in fish.

At the root of the tongue, nearly as far back as these openings reach, the trachea begins much in the same manner as in birds. It passes backwards above the heart, and there divides into two branches, one going

\* To avoid the confusion in our ideas, which might arise from the use of the words anterior, posterior, upper, lower, &c. in the whole of this description, the animal is considered in its natural horizontal position, so that the head is forwards, the back upwards, &c.



to each lobe of the lungs. The lungs are two long bags, one on each side, which begin just behind the heart, and pass back through the whole length of the abdomen, nearly as far as the *anus*. They are largest in the middle, and honey-combed on the internal surface through their whole length. The heart consists of one auricle and one ventricle. What answers to the inferior vena cava, passes forwards above, but in a sulcus of the liver, and opens into a bag similar to the pericardium; this bag surrounds the heart and aorta, as the pericardium does in other animals; from this there is an opening into a vein which lies above, and upon the left of the auricle, which vein seems to receive the blood from the lungs, gills, and head, is analogous to the superior vena cava, and opens into the auricle which is upon the left of the ventricle. The aorta goes out, passing for a little way in a loose spiral turn, then becomes straight, where it seems to be muscular; at this part the branches go off, between which there is a rising within the area of the aorta like a bird's tongue, with its tip turned towards the heart\*.

\* This account of the *venæ cavæ* opening into the cavity of the pericardium may appear incredible; and it might be supposed, that, in the natural state of the parts, there is a canal of communication going from one cava to the other, which being broken or nipt through in the act of catching or killing the animal, would give the appearance above described. I can only say, that the appearances were what have been described, in three different subjects which I have dissected; and in all of them the pericardium was full of coagulated blood. But, besides the smallness of the subjects, it may be observed that they had been long preserved in spirits, which made them more unfit for anatomical enquiries. They had been in my possession above seven years.

The

The liver is principally one lobe, pretty close to the heart at the fore part, and passes back on the right of the stomach and intestines; at its anterior extremity on the left side, there is a very short lobe, ending abruptly. The gall-bladder lies in a fissure on the left side of the liver near its middle; there is no hepatic duct; the hepato-cystic ducts, which seem to be three in number, enter the gall-bladder at its anterior end or fundus, and the cystic duct passes out from the posterior end of the gall-bladder, and terminates in the gut, about half an inch from the pylorus. The œsophagus, which is pretty large, passes back, and is continued into the stomach in the same line. The stomach, at the posterior end, bends a little to the right, where it terminates in the pylorus. The intestines pass back making many turns; at the posterior end they become pretty straight, forming what may be called the colon, or rectum, where they are a little larger and run to the anus in a straight direction. At the beginning of this larger part of the intestinal tube, there is no valvular structure. The spleen is a very small but long body; its anterior end is attached to the upper surface of the stomach, and it is continued back along the left side of the mesentery, to which it adheres. The pancreas is a small body lying above the duodenum, and is attached also to the left side of the mesentery. The kidneys are situated in the upper and posterior part of the abdomen, having the rectum passing below and between them as in the snake, &c. Below the rectum lies a long bag, like a bladder; it adheres all along to the inside of the abdominal muscles, and its mouth opens into the rectum; but whether

ther it is the bladder of urine, or not, I cannot tell. On each side of the rectum, close to the lungs, there is a body, the posterior end of which rests upon the anterior end of the kidney: whether they are testicles or ovaria, I cannot pretend to determine; but should imagine that they are either the one or the other.

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A N  
I N D E X

T O T H E  
Fifty-Sixth V O L U M E

O F T H E  
*Philosophical Transactions.*

For the YEAR 1766.

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