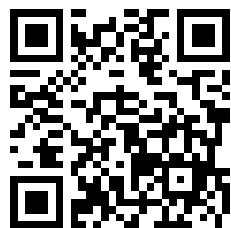

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PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT
OF THE
Present Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Considerable Parts of the WORLD.

VOL. LIII. For the Year 1763.

LONDON:

Printed for L. DAVIS and C. REYMERS,
Printers to the ROYAL SOCIETY,
against Gray's-Inn Gate, in Holbourn.

M.DCC.LXIV.

1763. 26

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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 adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they
a 2 should

A D V E R T I S E M E N T.

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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- PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Sun's Distance from the Earth, deduced from Mr. Short's Observations relating to the horizontal Parallax of the Sun: In a Letter from Peter Daval, Esq; V.P. of R.S. to James Barrow, Esq; V.P. of R.S.*

Read Jan. 13,
1763.

ACCORDING to Mr. Short,
the mean horizontal parallax of
the Sun is $8''$, 65 .

Now this parallax is the angle, which the semidiameter of the earth subtends, being seen from the Sun.

Therefore as $8''$, 65 , is to 360° (the whole periphery of a circle) so is the semidiameter of the earth to the periphery of the orbit of the earth round the

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B

Sun.

Sun. But $8'', 65$, is very nearly $\frac{1}{360}$ th part of 360° , as may be easily proved by division.

According to the latest observations, the mean semidiameter of the earth is 3958 English miles, which being multiplied by 149,826 produces 593,011,308 miles for the circumference of the orbit of the earth.

The distance of the earth from the Sun is the semidiameter of this orbit: and the periphery of the circle is to it's semidiameter very nearly as $6,283,185$ to one.

Therefore if we divide 593,011,308 by $6,283,185$: the quotient, which is very nearly $94,380,685$, will give the mean distance of the earth from the Sun in English miles.

N. B. As the orbit of the earth is an ellipsis, not a circle, the distance of the earth from the Sun will be greater in it's aphelion, and less in it's perihelion, than here assigned.

Dear Sir,

I have from Mr. Short's observations deduced, as above, the mean distance of the Sun from the earth, and am pretty sure I have made no material mistake.

I am

Your's entirely,

Dec. 18, 1762.

Peter Dayal.

II. Ob-

II. *Observatio cometæ, qui mense Maio A. 1759 apparuit, facta Hagæ-Comit. à Petro Gabry, I. V. D. Societatis Reg. Scientiar. Socio, et Commercio Literar. cum Academ. Scientiar. Parisiensi et Reg. Societ. Gotting. juncto: Communicated by Mr. Emanuel Mendez da Costa, Librarian of the Royal Society.*

Read Jan. 13, 1763. **D**IE 2 Maii, vesperi hora 9, cœlo sereno, prima vice cometam vidi exorientem, et in gradu $19^{\circ} 12' 24''$, Virginis, cum $28^{\circ} 40' 5''$ latitud. austral. existere, observavi: cauda existente admodum exigua, et vergente versus stellam W *Doppelmajeri* Atl. cœl. in constellatione Hydræ, vel β *Bayeri* Uranometr. Tab. 42. et *Flamsteedii* Atl. cœl. in constellationibus Hydræ, Corvi, & Crateris, in basi Crateris australis. Ipse vero cometes ei stellæ, quæ est tertia et proxima ante Craterem V *Doppelmajeri*, vel γ *Bayeri* Tab. 44. in constellatione Hydræ, quasi subsidebat, paulo tamen illa australior.

Secunda vice observavi eundem cometam ascendentem supra horizontem proximè sequenti die 3 Maii, post nonam et dimidiam vesp. horizonte nostro meridiem versus exhalationibus infra cometam obiecto. Inveni autem ejus longitudinem $17^{\circ} 11' 40''$ cum latitud. $27^{\circ} 20' 20''$ austral. Idem tum australior factus longius à Crateris basi recesserat, ita ut ferè in recta cum Ω ad alam dexteram Corvi *Bayeri* Tab. 43, et spica ω linea existere videretur.

B 2

Tertia

Tertia vice cometa mihi fuit conspectus vesperi die 6 ejusdem mensis post horam nonam cum dimidia. Vidi tunc cometam multò occidentaliorem quam , et supra ϕ Bayeri Tab. 44, factum esse, et cum corde Hydræ et ala sinistra Corvi γ Bayeri Tab. 43, efficere triangulum obtusangulum, cuius angulus erat in cometa obtusus et horizonti vicinior; basis sive hypotenusa erat recta jungens dictas stellas, et versus zenith vergebatur. Cometa longitudinem habebat $12^{\circ} 51' 7''$, et latitud. austral. $22^{\circ} 37' 24''$.

Proximè sequenti die 7, post nonam et dimidiā (paullò ante 10) vespertinam, eum iterum contemplatus sum, et cum prima trium ante craterem S Doppelmajeri, vel μ Bayeri Tab. 44, et proxima ante craterem V Doppelmaj. vel ν Bayeri, iterum ferè confidare triangulum obtusangulum; non ut hesterno die, sed in statu inverso, et non tam obtuso: cuius angulus ad cometam obtusus erat, et zenith versus vergebatur, hypotenusa seu basis jungens dictas stellas, recta, et horizonti vicinior, et cometa sane sine cauda. Longitudo ejus $11^{\circ} 59' 14''$, cum latitud. $21^{\circ} 1' 44''$ meridional,

Cæterum, diebus 15 et 16 ejusdem mensis cometam admòdum debilem vidi, et sine magno labore tubo contemplatus sum, ita ut nulla amplius observatio à me fieri potuerit.

*III.. Observatio cuiusdam Meteori igniti instar
Chasmatis, facta Hagae-Comit. d. 21 De-
cembr. 1758. Nov. St. à Petro Gabry, I.
V. D. Socio Reg. Societatis Scientiar. Lon-
din. et Commercio Literarum cum Acad.
Reg. Scientiar. Parisiens. et Societ. Reg.
Scient. Gottingens. juncto. Communicated
by Mr. Emanuel Mendez da Costa, Li-
brarian of the R. S.*

Read Jan. 13, 1763. **H**ORA octava vespertina, lumen in-
signe, sive meteoron ignitum instar
chaismatici, à me conspiciebatur in cœli plaga ferè oc-
cidentali, quum cœlum eo tempore esset nebulosum,
perobscurum, et tranquillum, zephyro leniter spirante.
Aér hoc tempore erat caloris ferè temperati, nam
thermomotrum mercuriale sub diō denotabat grad. 47.
Interdum plaga cœli occidua ardore videbatur, et aér
una cum inferioribus nebulis in flamas fumosque
mutatus. Ipso momento contigit etiam, ut ex ipsa
plaga effulgentes flamas aliquando ad zenith ferè
usque ascenderent, et speciem radiorum ejaculantium
efformarent usque circa cœli verticem.

- Paulò post fulgor apparebat candens et satis nota-
bilis, repræsentans ignem quasi continuum, instar
massæ ignæ: sub initium fulgor tenuis debilisque erat,
mox vero auctus, à plaga aquilonari ad occidentalem
ultra extendebatur, haud absimilis lumini in horizonte
comparanti proximè ante solis ortum. Paulò post

fulgor rutilum ducebat colorem, ita ut ego, et alii mecum, incendium in vicinia extra urbem coortum arbitraremur; sed aliter edocebamur, cum fulgor sensim altius supra horizontem evehetur, tantum emittens lumen, ut circumiectas ædes collustraret.

Ex ipsa massa ignea flammæ tam rapido emittebantur motu, ut duorum triumve secundorum spatio ab horizonte sensibili ad zenith ferè eveharentur, paululum autem ad septentrionem vergentes. Dictam massam evanescensem excipiebat altera, flamas itidem ejaculans. Quædam flammæ ascendere videbantur, altius tamen quam in vulgaribus incendiis evolabant; quippe quæ ferè ad zenith usque tendebant, aliæ ab horizonte sensibili ortæ rutili erant coloris, ferri instar igniti.

Hujus meteori figura valde mutabilis erat, ita ut nec certam descriptionem neque minus ejus delineationem dare possim; licet hoc spectaculum pariter jucundum et quodam modo visu horrendum esset.

Hic notatu dignum mihi esse videbatur, quod ejusmodi meteoron, tempestate regelata, nebulosa, et, quod magis est, pluviosa, tanta nitebat luce, ut non lunæ, sed illam potius imitaretur lucem, quæ solem tempore æstivo orientem præcedere solet. Fluctuum quoque instar marinorum aut segetis spicatae in agro undulantis, vento acrius spirante, agitabatur. Micatio præterea continua cernebatur, qualis interdum tempore æstivo vesperi vel noctu è nubibus promicare cernitur, et apud nos vulgo *Zee-vlammen* appellantur.

Hora autem octava cum dimidia meteoron hoc evanuerat, ita ut ne minimum ejus vestigium amplius à me conspiceretur.

**IV. An Account of a remarkable Decrease of
the River Eden, in Cumberland: In a
Letter to Charles Lord Bishop of Carlisle,
F. R. S. from William Milbourne, Esq;**

My Lord,

Read Jan. 13, 1763.

AS I know your Lordship sometimes condescends to amuse yourself with natural curiosities, I have taken the liberty to send you an account of a very sudden decrease of the river Eden at this place, attended with some particular circumstances, of the exact truth of which I can venture to assure your Lordship.

In the night between the twenty-eighth and twenty-ninth of December last, the river Eden, at Armathwaite, fell at least two feet perpendicular. The decrease of the water was so sudden, that several trouts and young lampreys had not time to save themselves, but were found the next morning frozen to death. Of the former, eye-witnesses can speak to fifteen, of the latter, two hundred, all which were found in the extent of no more than forty yards. And several dozens of young lampreys were easily taken up alive, by the hand, in the shallows. The suddenness of the water's decrease, may be so far ascertained, as follows. The miller of Armathwaite-mill left off grinding at twelve o'clock that night, there being then sufficient water to work the mill. He went to the mill the next morning at six, and there was not then water enough to turn the wheel round. It hath not

not been known, that the river Eden was ever so low at this place, by a foot, in the dryest summer. The water continued in this state, till about eleven o'clock of the morning of the 29th, and then gradually increased (no rain or snow falling) till about one in the afternoon, by which time it had risen about a foot perpendicular.

N. B. The trouts in general were small, the lampreys about ten inches or a foot long.

I shall only observe to your Lordship, that there was a most intense frost that night, and a strong wind varying from the North-east to the South-east; and that the river runs here from South-west to North-east.

I am,

my Lord,

your Lordship's

most obedient humble servant,

Armathwaite Castle,
Jan. 4, 1763.

William Milbourne.

V. A.

[9]

V. An Account of the Rain fallen in a Foot-square at
 Norwich, by Mr. William Arderon, F. R. S. Com-
 municated by H. Baker, F. R. S.

Read the 12th of January, 1761.

| | 1749 | 1750 | 1751 | 1752 | 1753 | 1754 | 1755 | 1756 | 1757 | 1758 | 1759 | 1760 | 1761 | 1762 |
|-------------------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|--------|
| | wine |
| | pints. |
| Jan. | 5 | 2 | 10 | 4 | 8 | 6 | 8 | 5 | 5 | 9 | 9 | 7 | 4 | 5 |
| Feb. | 8 | 5 | 4 | 6 | 9 | 8 | 14 | 6 | 4 | 14 | 4 | 6 | 9 | 4 |
| Mar. | 6 | 2 | 14 | 6 | 8 | 7 | 3 | 5 | 10 | 9 | 12 | 4 | 13 | 6 |
| April | 8 | 9 | 11 | 7 | 8 | 4 | 8 | 7 | 6 | 13 | 9 | 7 | 11 | 1 |
| May | 4 | 3 | 10 | 3 | 11 | 9 | 5 | 3 | 8 | 7 | 3 | 4 | 7 | 7 |
| June | 8 | 4 | 6 | 2 | 12 | 2 | 4 | 7 | 9 | 6 | 15 | 7 | 1 | 5 |
| July | 9 | 7 | 25 | 8 | 19 | 5 | 11 | 6 | 11 | 3 | 18 | 7 | 9 | 3 |
| Aug. | 4 | 7 | 9 | 4 | 12 | 2 | 16 | 8 | 17 | 4 | 17 | 22 | 4 | 17 |
| Sept. | 4 | 2 | 15 | 2 | 4 | 3 | 3 | 2 | 0 | 7 | 12 | 9 | 3 | 7 |
| Oct. | 16 | 2 | 9 | 3 | 2 | 4 | 21 | 2 | 6 | 7 | 6 | 5 | 12 | 2 |
| Nov. | 4 | 2 | 13 | 8 | 16 | 6 | 9 | 5 | 18 | 7 | 7 | 8 | 2 | 5 |
| Dec. | 9 | 10 | 6 | 12 | 5 | 17 | 1 | 18 | 5 | 12 | 9 | 5 | 4 | 12 |
| | | | | | | | | | | | | | | |
| | 100 | 7146 | 6124 | 6134 | 1105 | 6128 | 132 | 142 | 127 | 9113 | 1133 | 9116 | 1143 | 7 |
| Total inches deep | 20 | 1829 | 3824 | 926 | 821 | 125 | 626 | 428 | 525 | 522 | 726 | 823 | 328 | 8 |
| VI. OZ. | | | | | | | | | | | | | | |

VOL. LIII.

VI. OZ.

VI. Observations upon the Effects of Electricity applied to a Tetanus, or Muscular Rigidity, of four Months Continuance. In a Letter to the Royal Society. By William Watson, M. D. F. R. S. Member of the Royal Colleges of Physicians of London and Madrid, and Physician to the Foundling Hospital.

To the Royal Society.

Gentlemen,

Read Feb. 10, 1763. EVER since your establishment, the communicating the history of uncommon diseases has seldom failed of a favourable reception by you, and has been frequently thought to merit a place in your journals and register-books. This has emboldened me to lay before you the following history.

CATHERINE FIELD, a girl in the Foundling Hospital, aged about seven Years, and otherwise a healthy child, having been disordered a few days with what were considered as complaints arising from worms, was observed, on Thursday, July 8, 1762, to open her mouth with great difficulty. This particular circumstance increased so much, that by the Sunday following, when I first saw her, her teeth were so much confined, it was with difficulty that even liquids could be admitted into her mouth. She had two days before parted with two worms, and had several

several very offensive stools. Her breath was now, and had been for some days, very fetid.

Though her jaw was locked very close, she was without pain; even in the Temporal and Masseter muscles, whose office is to bring the under-jaw to the upper; and which, in this instance, were tense, hard, and spasmodically affected. She was feverish, her pulse was quick, and her flesh hot; and she had had but very little sleep.

On Monday, July 12, I visited this poor girl in consultation with my learned and ingenious friend and colleague Dr. Morton. We found she had had a restless night; her fever was high, and it was infinitely difficult to introduce any thing between her teeth. As there had been no wound, no eruption repelled, we were of opinion, from her offensive breath and other indications, that the spasm of her jaw was symptomatic, either of worms or foul bowels.

Whatever was admitted into her mouth was swallowed without difficulty; neither in this state of the disease was her breathing at all affected. The regimen we put this patient under, for this formidable complaint, will be mentioned hereafter.

For near three weeks the disorder confined itself to the jaw, during which time she was constantly feverish. At times indeed her fever ran very high, and her pulse beat 130 strokes in a minute. At other times it beat only about 100; but never for these three weeks was it slower than that number.

Notwithstanding our best endeavours, the disease not only continued, but the rigidity communicated itself to the muscles of her neck, so that she could not

move her head in the least: And from pains shooting down her back, we had reason to apprehend, and which indeed did soon after happen, that the muscles of her back would soon likewise be rigid.

After the back was affected, the disease extended itself very fast; so that by the end of September, almost all the muscles of her body were rigid and motionless. To be somewhat more particular; the rigidity from the Temporal and Masseter muscles had extended itself to the cheeks, to the neck, breast, abdominal muscles, all those of the back, the right arm, the hips, thighs, legs, and feet. Nor were they by any force, that could be exerted with safety, to be extended. By the rigidity and contraction of the large and long muscles of the back, the Os Sacrum and hips were pulled towards the shoulders; so that the spine formed a very considerable arch. By the superior strength of the Flexor muscles of the thighs to that of the Extensors, the legs were pulled up almost to the thighs.

Of all her limbs, the left arm only preserved any motion. Of this the joint of the shoulder was rigid, that of the elbow extremely impaired; but the wrist, hand, and fingers, were reasonably pliant. The various muscles subservient to the motions of the eyes, eyelids, lips, and tongue; as well as those, internal ones at least, which assist in performing the offices of respiration and deglutition, did not seem in the least to partake of the rigidity.

From the end of September to the middle of November, the disease, as though it had exerted all its power, was at a stand. The feverish heat had left her, and her pulse beat generally between eighty and

and ninety strokes in a minute. But during this interval the poor patient was seized many times, both in the night and in the day, with violent convulsions in those muscles of the eyes, face, and right arm, which had any mobility left. These were so severe, that, in her weak and wretched state, her attendants imagined every attack would put an end to her distresses.

In this state, partly from the severity of the disease and partly from the very small quantity of food which could be given to her, and which was only through a small opening made by extracting two of her teeth and without which she must inevitably have been starved, she was emaciated in a most extraordinary manner. Her belly was contracted, and pulled inwards towards the spine. Her whole body, to the touch, felt hard and dry, and much more like that of a dead animal than a living one. This, added to the very great distortion of her back and lower limbs, heightened the disagreeable spectacle, and called to my mind that admirable passage of * Aretaeus, who, when treating of and contemplating this disease, calls it "inhumana calamitas, injucundus aspectus, triste intuenti spectaculum, et malum insanabile." And he subjoins, that "their distortions are such, that they cannot be known by their most intimate friends;" which in the case before us was most strictly true.

During the continuance of this disorder, which had lasted now more than four months, nothing had been omitted that either Dr. Morton or myself

* Cap. vi. Ἐξάθρωπος οὐ ευμφορή, καὶ αὔρετης μὲν οὐδείς, οἰνογένη δὲ καὶ τῷ οἴνῳ. θέν, αἵμασιν δὲ τὸ δινόν.

could

could suggest for her relief. While worms or foul bowels could be suspected to have occasioned this illness, as her stools were at first very offensive, and she had voided two worms, vermifuges of the most celebrated kind, linseed oil both by the mouth, and by clysters, and such other medicines as tend both to carry off or destroy the worms, and cleanse the bowels, were assiduously administered. But no relief arising from these, bleeding with leaches at the temples, when her fever ran high, blisters behind the ears, round the neck, upon the head, and in various parts of her body, were from time to time applied, as the disorder seemed to indicate. Nor during this time were antispasmodic remedies of various kinds omitted, and that in very liberal doses. Among these, as in several cases of locked jaws, related by authors of undoubted credit, opiates had been found to have been attended with great success. Tinctura Thebaica was copiously given. So that, between the 12th of July and the end of the month, more than nine hundred drops of that tincture were taken: A large quantity for so young a person! This we sometimes thought had a good effect, as the jaw was at times somewhat loosened; but this advantage was temporary, and the stricture soon returned as severe as before.

Though this medicine, given in large doses, did not affect her head, but only gave her quiet nights, yet it was occasionally obliged to be suspended; as her pulse was at times much sunk, and her sweats cold, and clammy. Volatile liniments were liberally used to the rigid parts, and warm bathing was continued for many weeks, with much friction, while in the warm water.

After warm bathing had been so long tried without sensibly good effect, cold bathing, recommended by Hippocrates * for the cure of this disease, was directed; and she was dipped several times, without being apparently the better or worse for it.

From the end of September, as what had been done hitherto had not been able to prevent the rigidity extending itself, we desisted from attempting to relieve her by medicine, and determined to nourish and support her; and wait to observe, though it was scarce to be expected, whether nature unassisted would point out any crisis for her relief. This attention was continued to the middle of November, without any other alteration than that her convulsions increased in their force; and every day, by those who were about her, was expected to be the last; and which was an event, as the prospect was so unpromising, much to be wished for. Dreadful however as her situation was, she was still alive: we were desirous therefore of omitting nothing, that in the least might be expected to relieve her.

I HAD heretofore many times observed, that in paralytic limbs, the muscles of which had for a considerable time ceased to be subservient to the will of the patient, I had been able, by the means of electricity, to make any muscle I thought proper contract itself, and act as a muscle, without the patient's being able to controul it. I had seen in one instance the good effects of electricity, in restoring to the hands and arms of a paralytic almost their accustomed strength, and voluntary motion; but these good effects, the

* Περὶ νούσων, Lib. III.

greatest

greatest part of them at least, were only temporary, and the patient relapsed. But I had never seen or known the effects of electricity in the contrary affection, viz. rigidity of muscles. I was very desirous therefore of trying its effects in this instance, and of shaking the rigid muscles by electricity; especially as I could have it done with very little pain, and no danger to the patient.

I just now mentioned, that I was able in paralytic persons to make any particular muscle at my will exert its action. This was to be effected by *simple electrifying* only; but by modifying and altering the apparatus of the *charged vial*, I was able to do much more. It is now seventeen years since, that I discovered, and communicated it to you at that time, that by means of the *electric circuit* I could cause the electricity to pervade any muscle, any number of muscles, or whatever part of the body I pleased, without affecting the rest with that unpleasing sensation. Many experiments, relating to this matter, and which I laid before you, were printed in the forty-fourth volume of the Philosophical Transactions.*

But to return to our patient: We ordered her to be electrified about the middle of November. This was done every day, or every other day for about twenty minutes, by *simply electrifying* the muscles subservient to the motion of the lower jaw, her neck, and her arms. This at first was very difficult to be achieved; as she was not capable of being placed in a chair to be electrified by herself, and as an assistant could scarce hold her on account of her being greatly distorted. It with difficulty, however, was done.

* Pag. 718, & seq.

After

After about a fortnight, the convulsions left her, and her sleeps were longer and more quiet; but the rigidity continued the same. After this, such parts of her body, as were thought expedient, were made part of the electric circuit, and were shook by the explosion of the charged vial. These applications were at first more particularly made to the Temporal and Masseter muscles (the parts first affected) and to the muscles of the neck and arms; afterwards to those of her back, hips, thighs, and legs. Care was taken to moderate the shocks in a manner, not to be too severe; and she was electrified every second, and sometimes every third day.

The fits, as I just now mentioned, and which were of the epileptic kind, left her in about a fortnight from her being electrified, and have never since returned, even in the slightest degree. In about a fortnight more her jaw was looser, and the muscles of her neck and armshad a large share of motion: and it was very observable, that as her muscles increased in their power of motion, they increased in their size, and the patient in her strength. By the end of January, not to be too tedious in my narration, by continuing the electricity, every muscle in her body was loose, and subservient to her will; and she could not only stand upright, but walk, and can even run like other children of her age. With her strength, she has so far recovered her flesh and colour, that her present appearance is that of a reasonably healthy child; and her breath has quite lost its late offensive smell. The only parts of her body not quite so loose as the rest, are the Temporal and Masseter muscles, which were the parts first affected by the disease.

This prevents her opening her mouth quite so wide as she formerly could ; but this hindrance is so little as not at present to be taken notice of, unless hinted at beforehand. She now goes to school, lives at large, and goes out every day when the weather is seasonable; but the electrising is still continued, tho' not so constantly and regularly as before. This I propose should be continued, until the return of warm weather. In the last week this child was presented to the committee of the Foundling Hospital, where several of the governors, who were apprised of her case, expressed their amazement at her, so unexpected, recovery.

It is here to be observed, that, except the muscles subservient to the motion of her jaw, none so long continued their rigidity as those of the back, denominated “*longissimi dorsi*” by anatomists. These, when almost all the other muscles of the body were loose, remaining tense and hard; and, by pulling the loins up towards the shoulders, continued the arch of the spine before mentioned. As the patient was so much emaciated, these muscles might be traced, on each side of the spine, from their origin to their insertion; and for a considerable time after she was in other respects recovering, these felt hard like twisted cords. At length, however, by directing the electricity through them, and the parts near them, in a very liberal quantity, these likewise gave way, and are now as loose as any other muscles of her body.

In proportion as a matter is extraordinary, the proofs to support its reality should be extraordinary. That excellent maxim, “*Nil temere credere,*” should never

never be lost sight of in our inquiries; otherwise novelty and the love of the marvellous will be apt to mislead us. On the other hand, the indulgence of an extravagant Pyrrhonism may prove equally detrimental in every endeavour to extend the bounds of science. It may prevent the giving due weight to matters of real information, and hinder their being made useful. For my own part, I should think it an indignity offered to the Royal Society, to lay before you any extraordinary phænomenon, which is supported only by a slight degree of evidence. On the contrary, when a number of concurrent circumstances tend to establish a fact, we ought not in a certain degree to refuse our assent to it, though somewhat out of the common course. Thus in the case before us; when an unusual disease of several months continuance, and when the patient was supposed to be reduced to the last extremity; when medicines and applications of every kind, celebrated by the ablest writers and practitioners both antient and modern, had been tried with little or no effect, at least with regard to the rigidity; when during a course of electrising no medicines or applications of any kind were made use of; when likewise, during this course, the patient voided no worms, had no purgings, eruptions on the skin, or kindly impostumations, which might have been considered as critical discharges, and to have brought about the cure; when, I say, none of these things happened, and the patient under electrising only, and that at a very severe season of the year, has been restored to perfect health, I cannot refuse my assent in believing it effected by the power of electricity. That so active

a principle, when properly directed to the diseased parts, should have important effects, no one can doubt who has been in the least conversant with it. Though at the same time I confess, well apprised of the salutary effects of warm weather in restoring a more perfect motion to torpid limbs, that had the electrising been begun in March, and continued to the end of May, though attended with the same success as in the present instance, I could not have suppressed my doubts of the warm weather greatly contributing thereto. But as this was done during the depth of winter, and that a severely cold one, no scruples, in my mind at least, can arise upon this head. I take the liberty however to lay the whole evidence before you, that every one may make from it such deductions as he thinks proper.

Perhaps indeed some may be of opinion, that even the cold weather contributed to cure this disorder. But it is well known, that warmth relaxes the animal fibres, and that cold constipates and braces them. In the case before us, the muscles, composed of minute fibres, were as rigid and tense as they well could be, even in a diseased and obstructed state. If cold therefore contributed any thing, it was to make this case worse. And this is conformable to the opinion of Aretæus*, who, among the causes of the disease, reckons intense cold; and says, "that for " this reason the winter of all the seasons is most pro- " ductive of this disease." He subjoins, "that women " are more subject thereto than men, on account of " the coldness of their constitution." Celsus † like-

* Aretæus, Lib. I. Cap. vi.

† Celsus, Lib. II. Cap. i. Frigus modo nervorum distentionem, modo rigorem infert: illud Σπασμός, hoc Τέρατος Græcè nominatur. wife

wise expressly asserts, that cold sometimes is the cause of it, and in another part of his excellent work says, “ that the greatest caution should be used to defend the patient from cold ; and that therefore the fire in his room should be constant.” He moreover recommends warm bathing both in water and oil, as conducive to the cure of the disease. To these may be added the sentiments of Cælius † Aurelianus, who considers that cold is frequently the cause of this disease. He recommends various kinds of warm external applications ; such as warm bathing, rubbing the affected parts with warm oil, the application of warm cataplasms, bags of heated bran, or linseed. With Celsus, this author recommends, that attention be given to the warmth of the patient’s chamber. How far therefore, for the reasons and authorities before-mentioned, cold weather could probably assist in the cure of the case before us, need not in my opinion be insisted upon.

AND now, Gentlemen, permit me to make a few observations upon the disease itself, which, at least in the degree of the case before you, is a very rare one in temperate climates. In warmer countries, and especially between the tropics, it is too often seen. It was well known to the ancients. Hippocrates § calls ‘it Τίτανος, and says, that those who have it severely, die on the fourth day ; if they survive that day, they recover. He makes farther mention of it in other parts of his works* ; more particularly in his book Περὶ Νοσῶν, where he describes both the

† De Morbis Acutis, Lib. III. Cap. vi. viii.

§ Aphorism, Lib. V. Sect. vi.

* Vide Lib. Περὶ χρισμῶν—Περὶ τῶν ἐπὸς παθῶν, & alibi.

Tetanos

Tetanos and *Opisthotonos*. In this part of his work, instead of the fourth, he mentions once, and repeats it, that if they live beyond the fourteenth day, they recover. Let it should appear, that the father of the medical art seems to contradict himself, it may not be improper to remark, that when he says, that the *Tetanos* is mortal in a very few days, he most generally means those which are symptomatic, and are attendant upon wounds, luxations, and bruises; such as the three instances mentioned in his *Epidemics*. Those affected with this disease, mentioned by Hippocrates in his book Περὶ Κεραύνων, are expressly said to arise from wounds. These were soon mortal. But where these diseases took their rise from other causes; they were less violent, continued longer, and the expectation of recovery was greater. In his book therefore, Περὶ Φύτων, when treating of the *Opisthotonos*, attendant upon a fever, inflammation of the throat, or other internal disorders, he says, that if they live beyond the fortieth day, they recover.

Aretæus*, under the same appellation with Hippocrates, has given us an excellent history and remarks upon this disease, as well as upon the *Opisthotonos*, and *Emprosthotonos*, which are nearly related to it; or, to speak more properly, the same disease affecting different muscles, and throwing the body into different kinds of distortion. Celsus † has mentioned and described this disease, to which no name was assigned by his countrymen, and has called it “Quidam nervorum rigor.” Tho’ this excellent author reckons it among the diseases of the neck, the parts

* Morb. Acut. Lib. I. Cap. vi.

† Lib. IV. Cap. iii.

first

first affected by it are the muscles subservient to the motions of the lower jaw, from which it is usually, if the disease continues, propagated to those of the neck. Cælius * Aurelianus has, as it is supposed from Soranus, described it, and handed down to us such methods of cure, as had been found in his time most successful.

Pliny § mentions the Tetanus in many parts of his Natural History. He forbids the use of wine to those who labour either under this disease, or the Opisthotonus. He recommends in different parts of his work, as internal remedies, castor, hellebore ||, the ashes of the fig-tree, pediculi marini, and pepper. He advises warm baths, with the nitre of *the ancients* dissolved in them; and directs the patient at other times to be rubbed with the coagulum found in the stomach of a calf, or with the juice of Peucedanum, or hogs-fennel. This, it is to be presumed, was the most general method of treating these diseases, in the age wherein this author wrote.

This disease is frequent in Greece, Italy, and in the warmer parts of Europe, where its effects are severely felt. † Bontius, who resided long in the East Indies, has briefly described it; which, though he says it is

* Morb. Acut. Lib. III. Cap. vi.

§ Plinii Hist. Nat. Lib. XXVI. XXXI. XXXII.

¶ Ibid. Lib. XXV. The hellebore made use of, was to be prepared in (at that time) a newly discovered manner, which was to prevent the effects of its acrimony. This was, by putting the hellebore between radishes split, and then tied together, including the hellebore; which, by being macerated in this manner for about seven hours, was supposed to become more mild in its operation.

† Bontii Meth. Medendi, Cap. ii. De Spasmo.

rare

rare in Holland, may be reckoned endemic in India. He seems not to have known what had been written by his predecessors upon this subject. He takes notice, that sometimes men seized with it became *dicto citius* rigid as statues.

An admirable account of this disease was a few years since communicated to the public by Dr. Lionel Chalmers † of South Carolina, where it is very frequent, especially among the negroes. And I am informed by a learned gentleman of undoubted credit, that in our military operations between the tropics in America, great numbers of our people, particularly of those who were wounded, died with locked jaws.

In England we generally give the name of the *locked jaw*; but that, let it arise from what cause it may, is only one symptom of it. If it continues, as in the case before you, the occasion of this paper, it propagates its rigidity to the neck, breast, and then to the other parts of the body.

It is seldom seen here that the Tetanus is an original disease. It is generally symptomatic, and the consequence of some other disorder. It frequently is subsequent to wounds and bruises of the nerves and tendons. I have known it arise to a certain degree from the sudden checking of an eruption upon the skin. I knew a temporary Opisthotonus occasioned by the too sudden loss of a large quantity of blood. To these permit me to add, that the Tetanus of the Temporal and Masseter muscles constantly attended those whom I have known to have been accidentally

* Medical Observations, Vol. I. pag. 87.

poisoned

poisoned by taking the *Oenanthe aquatica fucco virgo trocante* of Lobel; and of which, two communications of mine occur in the Philosophical Transactions.

I must here remark, that in the true Tetanus, the arms, when rigid, are straight, and extended along the trunk; the legs and thighs are likewise straight; but the case before you, in some degree, partook of the Opisthotonus, especially in the lower parts; as the spine was remarkably curved, and as the legs were pulled up towards the thighs.

The Tetanus I now lay before you, was an original disease; as there had been no wound, no eruption suppressed, nor other cause, which, we imagined, could occasion it. A case of a similar kind, as an original disease, occurs in Dr. * Storck's *Biennium Medicum*. And the Emprosthotonus, mentioned by the ingenious Dr. Macaulay, in the second volume of the Medical Observations, lately published, seems to have been likewise an original disease, and not a symptom of any other. As the case I now communicate is a very singular one, at least in Great Britain, and the treatment of it not less singular, though attended with all possible success, I had reason to hope that you would not be displeased to have it laid before you, in a manner somewhat circumstantial. I am firmly of opinion, if the epilepsy had left this patient, and life had continued, that she would have remained a most miserably helpless object, and as confirmed a cripple as can be imagined.

At present the patient is well; but if, contrary to

* Part I. Pag. 6.

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expectation, she should relapse, or any thing should occur in her case worthy your notice, I shall not fail to acquaint you with it; and am, with the utmost regard,

Gentlemen,

Your most obedient humble Servant,

William Watson.

Lincoln's Inn Fields, 9 Feb. 1763.

P. S. The patient continues well, her jaw is as loose as ever. The electrising has been discontinued above a month; and she is in every respect perfectly recovered.

27 March 1763.

July 8, 1763.

The patient is perfectly well, and there remain not the least indications of her having been diseased.

W. W.

VII. *An*

VII. *An Account of the late mild Weather
in Cornwall, of the Quantity of Rain fallen
there in the Year 1762 : In a Letter from
the Rev. William Borlase, M. A. F. R. S.
to Mr. Henry Baker, F. R. S.*

Dear Sir,

Ludguan, Jan. 22, 1763.

Read Feb. 10, 1763. I AM very sorry to hear of your distresses at London, by the rigour of the season.—Our winters in Cornwall are indeed generally more mild than any where in this island, but I do not remember so wide a difference as that of the present season with you and us.—In November, on the 12. 13. 14. our frost began, mostly attended with hoar frosty mornings: here and there a pool of still water had a film over it, scarce strong enough to bear an egg, not a large pebble: and the frost was always over before noon.—Frost of the same degree on the 18th, and 20th,—hoar frost only the 26th.—Frost, but of no greater degree, Dec. 5. 6. and 7th.—Hoar only on the 11th.—On the 14th and 15th, frost, but of the above degree only: a little sleet on the 31st post merid.—To this day no frost or snow. On these coldest days the Thermometer was never so low as 38° but on three days only, viz. Dec. 14 and 15th, and Jan. 9th.—I must not conceal from you, however, that some allowance must be made for the height of the Quicksilver, because my Thermometer is not with doors; but yet it stands in a little stair-case far from any fire, where the Sun in

the midst of summer never reaches till 6 o'Clock P. M.. and in winter never: and the case in which the tube of Quicksilver is fixed communicates with the open air, by three holes lined with tin, pierced through the munition of the window to which it is fixed; so that tho' it is not in the open air, yet must the Quicksilver be exposed to every extremity of the Atmosphere by constant intercourse.

You will judge that our cold was no ways excessive, when I add, that the balm of Gilead, in the natural open ground, has not suffered: the myrtles are in perfect health: the mignonettes in flower: the cluster rose and white Violet in bloom at Christmas; and at the same time I had the scarlet double ranunculus full blown given me by a neighbour. The double hyacinths have formed their bells, and some are now ready to unfold.

It has not (I believe) been remembered in the age of man, that in the west of Cornwall we have ever had such a long continuance of easterly winds.

About the middle of Nov. for 14 days the wind had its prevailing turn from the east. — It was easterly, with a variation now and then (a point or two) to the north or south, every day of December, excepting the 21st, when it blew W. S. W. and S. S. W. — and to this 22d day of January it has blown every day from the east, varying half a point or so to the S. or N.

Since I have entered into these latter disquisitions on the season, give me leave to add the quantity of water fallen here in the year 1762.

January

[29]

Inches. Tenth. Parts of a Tenth.

| | | | | | | | |
|--------------|----|---|---|---|---|---|---|
| January | — | — | 4 | — | 3 | — | 0 |
| February | — | — | 2 | — | 1 | — | 0 |
| March | — | — | 2 | — | 8 | — | 0 |
| April | — | — | 1 | — | 0 | — | 0 |
| May | — | — | 1 | — | 0 | — | 0 |
| June | — | — | 0 | — | 2 | — | 0 |
| July | — | — | 0 | — | 5 | — | 0 |
| August | — | — | 3 | — | 5 | — | 0 |
| September | — | — | 4 | — | 3 | — | 0 |
| October | — | — | 5 | — | 6 | — | 0 |
| November | — | — | 3 | — | 2 | — | 0 |
| December | — | — | 1 | — | 4 | — | 0 |
| <hr/> | | | | | | | |
| In the whole | 29 | — | 9 | — | 4 | — | 0 |
| <hr/> | | | | | | | |

If you, Sir, or any of your acquaintance keep an ombrometer, and register of the rain at London, I should be glad to know how much fell there, for by such observations it might in time be known where the quantity exceeds. I think round Paris they reckon but at 19 inches, but in islands, and near the Sea coast it must be more.

I remain, Sir,

your most obedient servant

William Borlase.

IX. An

[80]

VIII. *A Delineation of the Transit of Venus expected in the Year 1769, by Mr. James Ferguson.*

*To the Right Honourable the Earl of Macclesfield,
President of the Royal Socie'y.*

My Lord,

Read Feb. 10, 1763. I Beg leave to present to the Royal Society a delineation of the transit of Venus in the year 1769 [TAB. I.] which will be a much better transit for discovering the Sun's parallax than that in 1761 was.

Although I have only mentioned Wardhuys in Norwegian Lapland, and the Solomon isles in the great South Sea, as proper places for observing that transit; yet I am sensible, that any other place near the north cape will be just as well for the northern observers; and Tuberon's Isle, or St. Bernard's, or the Fly Islands, in the great South Sea, will answer as well for the Southern.

Although it cannot be expected, that any delineation can be so exact as calculation, yet I hope this projection will be found to come very near the truth; and am, with the highest respect,

My Lord

your Lordship's
and the Royal Society's
most obliged humble servant

James Ferguson.

Mortimer-Street,
Feb. 10, 1763.

IX. An

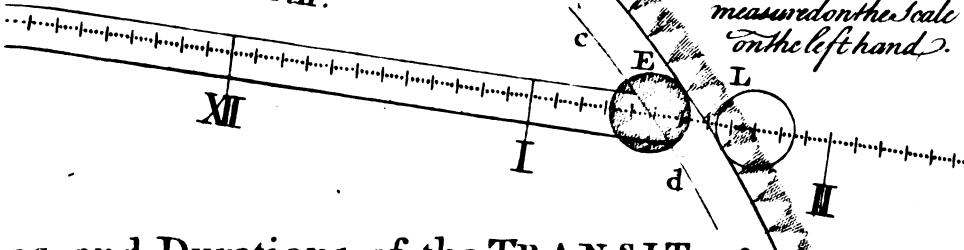
Scale of Minutes and Seconds of a Degree.



EXPLANATION.

The larger divisions are minutes of a Degree, and the smaller ones are Seconds. The Dials of the Sun & Venus & the geocentric latitude of Venus, are laid down from this Scale according to the tabular Elements. The Parallaxes of Venus's Latitude as seen from Wardhuys and from the Solomon Isles both at the times of total Egresses and beginning of Egresses are taken from this Scale, and set off from the geocentric Transit line by marks, and through these marks the lines of the visible Transits are drawn. All the parallaxes are delineated on the Earth's Disc, and measured on the Scale on the left hand.

Parts of the Earth.



Places and Durations of the TRANSIT.

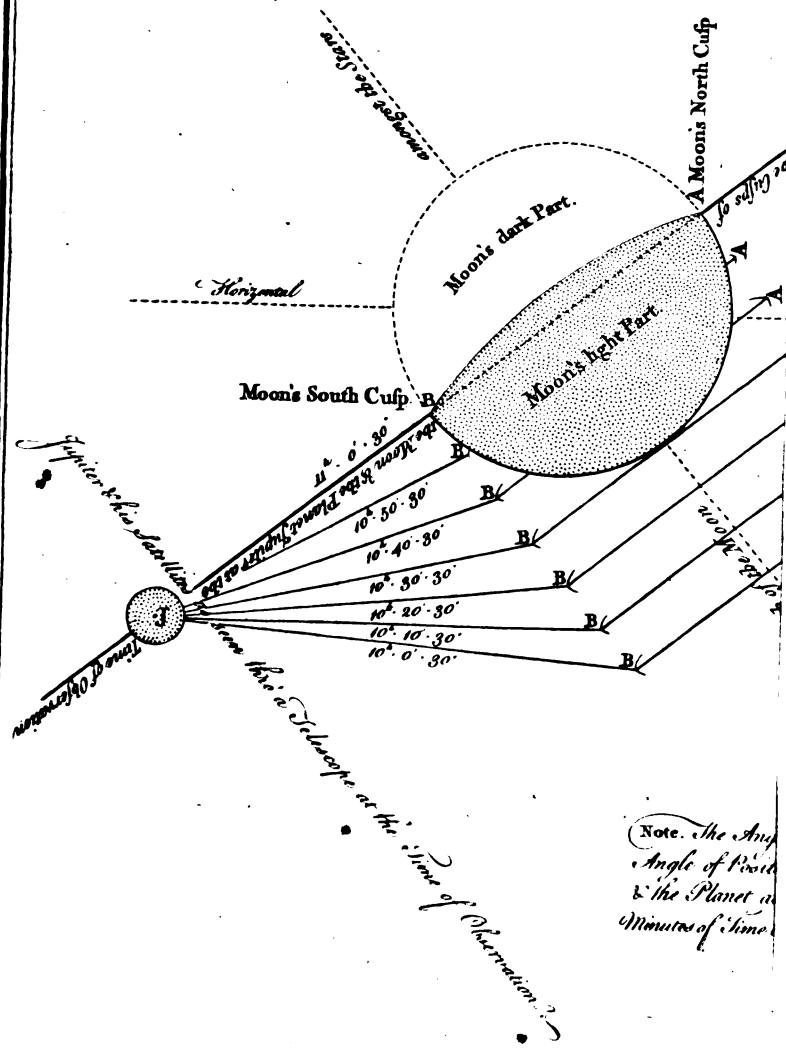
1. As they will be if neither the sun
nor any Parallax.

2. As they will be on Account of the
Parallaxes of the Sun and Venus.

In Wardhuys, when the Sun is at S, and Venus at O, she will be in the same way, from S to O, and Wardhuys the same way, from W to O. When Venus is at O in her Orbit, she will appear on the Sun at E, at her beginning of Egress, as seen from the Earth's center; but at that time, she will be quite clear of the Sun, in the line SVO, as seen from St. Cruz then at S; and as seen from Wardhuys. Then at W, she will appear on the Sun at N, shortly of her beginning of Egress, which will be later at Wardhuys, and sooner at St. Cruz, than as seen from the Earth's center.

J. Mynde Jr.

An Appulse of the Moon to the Planet Jupiter, 25.



IX. *An Account of an Appulse of the Moon
to the Planet Jupiter, observed at Chelsea,
by Mr. Samuel Dunn.*

Read Feb. 17, 1763. **T**HE alteration of the angles of position made by the cusps of the Moon, and a planet to which the Moon makes a near appulse, will always enable the astronomically inclined to determine from observation, the longitudes of places, by the naked eye and a clock or watch set to apparent or equal time.—Such an observation I made at Chelsea 25th Dec. 1762 at 11^h - 0' - 30" apparent time, the satellites being as in the figure at the same time. — Jupiter's distance from the Moon half a degree. TAB. II.

Lat. 51° 29' 5", Long. 41" West of Greenwich.

X. *A Cata-*

X. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1762, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. et Soc. Reg. nuper Praeses; by John Wilmer, M. D. clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Praefectus et Praelector Botanic.*

- Read Feb. 24, } 2001 A *Maryllis spatha uniflora, co-*
 } 1763. *rolla æquali pistillo declinato.*
 } *Hort Cliff. 135.*
 } *Lilio-narcissus Virginiensis. Catesby Carolin. 3.*
 } *p. 12.*
 2002 *Antirrhinum foliis lanceolatis acuminatis, pani-*
 cula virgata. Linn. Spec. 616.
 Linaria genistæ folio glauco. Herm. Par. App. 9.
 2003 *Asclepias foliis ovatis rugosis nudis, caule sim-*
 plici umbellis subsessilibus, pedicellis tomen-
 tos. Linn. Sp. 215.
 2004 *Calamintha montana præalta, pulegii odore,*
 dentatis foliis, floribus dilute cæruleis, exlon-
 go ramoso et brachiato pediculo prodeunti-
 bus. Boccon. Musæum, p. 2, 45.
 2005 *Canna foliis lanceolatis petiolatis enervibus.*
 Prod. Leyd. 11.
 2006 *Chenopodium foliis filiformibus acutiusculis,*
 caule erecto. Hort. Upfal. 55.

[33]

- 2007 *Cistus arborescens*, foliis cordatis lævibus acuminatis petiolatis. Hort. Cliff. 205.
Cistus Ledon foliis populi nigri major. C. B. P. 474.
- 2008 *Cistus suffruticosus* stipulatus, foliis ovatolanceolatis caulibus hirsutis decumbentibus.
- 2009 *Clypeola perennis* siliculis bilocularibus ovatis dispermis. Sauv. Monsp. 71.
- 2010 *Colutea foliolis ovatis*, racemis lateralibus secundis siliculis compressis.
- 2011 *Crotalaria foliis ternatis ovatis acuminatis*, stipulis nullis, leguminibus pedicellatis. Fl. Leyd. 278.
Crotalaria Afiatica frutescens floribus luteis amplis, trifoliata. Hort. Lugd. 196.
- 2012 *Cytisus Lusitanicus* foliis exiguis, magno flore, siliquis latis et tomentosis. Tourn. Inst. 648.
- 2013 *Datura* foliis obcordatis villosis integris, floribus oblongis erectis, pericarpis spinosis nutantibus.
- 2014 *Daucus vulgaris*. Clus. Hist. 198.
- 2015 *Echinops* floribus fasciculatis, calycibus multi-floris. Hort. Cliff. 391.
Chameleon niger, umbellatus flore cæruleo-Hyacinthino. C. B. P. 380.
- 2016 *Erica antheris* bifidis inclusis, corollis cylindricis longioribus aggregatis, foliis verticillatis, dentato aculeatis. Lin. Spec. 355.
- 2017 *Gaultheria*. Kalm. Linn. gen. nov. 1080. Sp. Pl. 395.
- 2018 *Hedysarum* foliis simplicibus ovatis acuminatis, spicis longissimis nudis terminalibus.

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F

2019

- 2019 *Hypericum* floribus monogynis foliis linearis-lanceolatis obtusis caule fruticoso.
- 2020 *Ipomaea* foliis trilobis cordatis pedunculis trifloris. Linn. Sp. 151.
- 2021 *Leonurus* foliis tripartitis multifidis linearibus obtusifusculis. Hort. Upsal. 170.
Ballote inodora foliis Coronopi. Amman. Ruth. 48.
- 2022 *Ligusticum Pyrenaicum* sceniculi folio, lucidum. Tourn. Inst. 324.
- 2023 *Linnia* caule erecto hirsuto foliis ovato-lanceolatis sessilibus, floribus terminalibus.
- 2024 *Martynia* caule ramoso, foliis cordato-ovatis pilosis. Miller's Icons 286.
- 2025 *Mespilus inermis* foliis ovato oblongis glabris serratis; caule inermi. Lin. Sp. 478.
- 2026 *Ocymum caryophyllatum maximum*. C. B. P. 225.
- 2027 *Paeonia* foliis quinque lobatis lanceolatis ex Tartaria.
- 2028 *Perclymenum capituliflorum* ovatis imbricatis terminalibus foliis summis corato perfoliatis subtus glaucis.
- 2029 *Pisum* caule angulato procumbente foliolis inferioribus lanceolatis acute dentatis, summis sagittatis. Dict. Hort.
- 2030 *Polygonum candidum* tenellum tomentosum flore purpureo. J. B. 3—300.
- 2031 *Prenanthes* flosculis plurimis floribus nutantibus subumbellatis, foliis hastato angulatis. Hort. Cliff. 383.
- 2032 *Ricinoides*, ex qua paratur Turnsol. Gallorum. Tourn. Inst. 655.

2033.

[36]

- 2033 *Ruellia* foliis petiolatis, fructu sessili confecto.
Hort. Cliff. 318.
- Ruellia strepens* capitulis compatis. Hort. Elt.
300.
- 2034 *Ruta sylvestris* minor. C. B. P. 336.
- 2035 *Scabiosa Africana* frutescens, folio rigido splen-
dente serrato, flore albicante. H. Amt.
2. 185.
- 2036 *Sibthorpia* folijs reniformi subpeltatis crenatis.
Lin. Gen. nov. 1999.
Chrysosplenium Coriubicense Retiver. Herb.
Tab. 6. Fig. 11.
- 2037 *Sida tomentosa* foliis cordatis serratis subtilis
nervosis.
- 2038 *Solanum* caule aculeato, foliis pinnato-sinuatis,
fructu racemoso. Schiru Schuna Hort. Ma-
lab. Vol. 2. Tab. 36.
- 2039 *Solanum* caule aculeato herbaceo, foliis cor-
datis sinuatis calycibus aculeatis. Virid.
Cliff. 16.
- 2040 *Solidago* caule paniculato racemis confertis,
foliis inferioribus linear-lanceolatis petiolatis
caulinis sessilibus glabris. Dict. Hort.
- 2041 *Spigelia Anthelmia*. Lin. Sp. 149.
- 2042 *Teucrium* foliis subcordatis inæqualiter serratis
petiolatis, racemis lateralibus secundis caule
erecto.
- 2043 *Trianthema* foliis ovatis petiolatis, floribus con-
fertis sessilibus, caule diffuso.
- 2044 *Trianthema* foliis abovatis petiolatis, floribus
sessilibus caulibus procumbentibus.
Trianthema. Sauv. Meth. 127. Linn. Sp. 223.

F 2

2045

- 2045 *Turritis minor.* Botan. monsp.
- 2046 *Vaccinium foliis integerrimis revolutis ovatis,
caulibus repentibus filiformibus nudis.* Lin.
Sp. 351.
- Vitis Idaea palustris.* C. B. P. 471.
- 2047 *Viburnum foliis ferrulatis ovatis acuminatis gla-
bris, petiolis glandulosis.* Lin. Sp. 268.
- 2048 *Urtica foliis alternis ovato cordatis ferratis ra-
cemis compositis erectis.* Miller.
- 2049 *Wachendorfia foliis lanceolatis quinquenerviis
canaliculato-spicatis floribus in thyrsum col-
lectis.* Burman. Fig.
- 2050 *Walkeria.* Gen. nov.

XI. Observations made by Mr. John Bartram, at Pensilvania, on the Yellowish Wasp of that Country: In a Letter to Mr. Peter Collinson, F. R. S.

Read Feb. 24. 1763. I Saw several of these wasps flying about a heap of sandy loam: they settled on it, and very nimbly scratched away the sand with their fore feet, to find their nests, whilst they held a large fly under their wings with one of their other feet: they crept with it into the hole, that lead to the nest, and staid there about three minutes, when they came out. With their hind feet, they threw the sand so dexterously over the hole, as not to be discovered: then taking flight, soon returned with more flies, settled down, uncovered the hole, and entered in with their prey.

This extraordinary operation raised my curiosity to try to find the entrance, but the sand fell in so fast, that I was prevented, until by repeated essays I was so lucky as to find one. It was six inches in the ground, and at the farther end lay a large magot, near an inch long, thick as a small goose quill, with several flies near it, and the remains of many more. These flies are provided for the magot to feed on, before it changes into the nymph state: then it eats no more until it attains to a perfect wasp.

The order of providence is very remarkable, in prescribing the different ways and means for this tribe of insects to perpetuate their several species, no doubt

doubt for good ends and purposes, with which we may not be well acquainted, but most likely, for the prey and food of other animals.

One kind of wasp fabricates an oblong nest of paper-like composition full of cells for the harbour of its young, and hangs it on the branch of a tree.

Some build nests of clay, and feed their young with spiders; others sustain them with large green grasshoppers: then there are those, that build combs on the ground (like ours in England) to nourish a numerous brood.

But this yellowish wasp takes a different method, with great pains digging a hole in the ground, lays its egg, which soon turns to a magot, then catches flies to support it, until it comes to maturity.

The wisdom of Providence is admirable, by giving annually a check to this prolific brood of noxious insects, in permitting all the males to die, which are the most numerous of the family; only reserving a few impregnated females of each species, to continue their race to another year.

Whereas bees, whose labours are so beneficial to mankind, always survive the winter to raise new colonies.

XII. *An Account of the Plague, at Aleppo:*
In a Letter to the Rev. Charles Lyttelton,
L.L. D. Dean of Exeter; now Lord Bishop
of Carlisle, and F.R.S. from the Reverend
Mr. Thomas Dawes, Chaplain to the
Factory at Aleppo.

Sir,

Aleppo, October the 26th, 1762.

Read Feb: 24, 1763. THE unexpected continuance of the plague in this city during the whole past winter having prevented the English ships, that brought me your favour of October 16th 1761, from receiving any thing on board from hence, I have been obliged thus long to defer paying my respects to you, and rendering my grateful acknowledgments for your generous concern, and good wishes for my safety.

Tho' I find by experience, that accounts given in news papers of occurrences in this distant quarter of the Globe seldom deserve much credit; yet I cannot contradict the report you mention of the plague's raging here in the summer of 1761. You probably will have had it confirmed long since, and also have heard of the accumulated distresses we have lately been labouring under: but as the particulars may not have reached you, I will venture to communicate them, tho' it is a subject neither pleasing to me to dwell on, nor can be very agreeable to you to read. Would to God I could even now assure you they are at an end.

On

On the mercy of his protecting Providence has been our sole reliance ; nothing else could have supported us under the many apprehensions and dangers we have been daily exposed to.

This unhappy country for six years past has been in a very terrible situation, afflicted during the greatest part of that time with many of the Almighty's severest scourges. Its troubles were ushered in by a very sharp winter in 1757, which destroyed almost all the fruits of the earth. The cold was so very intense, that the Mercury of Farenheit's thermometer, exposed a few minutes to the open air, sunk entirely into the ball of the tube. Millions of olive-trees, that had withstood the severity of 50 winters, were blasted in this, and thousands of souls perished merely thro' cold. The failure of a crop the succeeding harvest occasioned an universal scarcity, which in this country of indolence and oppression (where provision is only made from hand to mouth, and where, literally speaking, no man is secure of reaping what he has sown) soon introduced a famine with all its attendant miseries. The shocking accounts related to me on this subject would appear fabulous, were they not confirmed by numberless eye-witnesses, both Europeans and natives. In many places the inhabitants were driven to such extremities, that women were known to eat their own children, as soon as they expired in their arms, for want of nourishment.--Numbers of persons from the mountains and villages adjacent came daily to Aleppo, to offer their wives and children to sale for a few dollars, to procure a temporary subsistence for themselves ; and hourly might be seen in our streets dogs and human creatures scratching

scratching together on the same dunghill, and quarrelling for a bone, or piece of carrion, to allay their hunger. A pestilence followed close to the heels of the famine, which lasted the greatest part of 1758, and is supposed to have swept away 50 or 60 thousand souls in this city and its environs. I bless God, I was not a spectator of this complicated scene of misery: the very description of it must distress a compassionate disposition; the sight of it must have made an impression on an heart of flint.

I have already acquainted you, in a former letter, with our troubles by earthquakes, &c. of 1759 and 1760 and therefore shall proceed from the date of my last letter. The latter end of March 1761, the plague, which had lain dormant since the autumn, made its appearance again in this city, and alarmed us considerably. Tho' I confess, it did not surprize me; so far from not expecting its return, I should have looked on it almost as a miracle, if we had escaped, after the little progres it had made among us the preceding year. The infection crept gently and gradually on, confined chiefly to one particular quarter, till the beginning of May, when it began to spread visibly and univerſally. We shut up on the 27th, and our confinement lasted 96 days. The fury indeed of the contagion did not continue longer than the middle of July, and many of our merchants went abroad with caution early in August; but as our consul had no urgent business to induce him to expose himself to any risk, we remained in close quarters till we could visit our friends with tolerable security. As an addition to the uneasiness of our situation, the earthquakes re-

truned the latter end of April, tho' with no great violence; except the first shock, and that much less terrible than those of 1759. We felt 6 or 7 within the week, and 4 more at long intervals during our imprisonment; but as they were all slight, our apprehensions soon subsided. At our release from confinement the last day of August, we flattered ourselves with the hopes of a speedy release from danger; but it pleased God to order it otherwise. In all the plagues, with which Aleppo has been visited in this century, the contagion is said to have regularly and constantly ceased in August or September, the hottest months in the year; and it is pretty certain, that it disappeared about that time in 1742, 1743, 1744 and 1760; but unfortunately for us that now reside here, the year 1761 has proved an instance of the fallacy of general observations on this dreadful subject; for, from the end of March 1761 to the middle of Sept. 1762, scarce a day has passed without some deaths or fresh attacks from the distemper; and tho' the violence of it ceased in the autumn, yet I believe on an average it was fatal to at least 30 persons in every week, from that time to the end of the winter. In February last we were pretty healthy: hearing but of few accidents, and those in the skirts of the city, we once more began to entertain some faint hopes of a farther exemption, but they were of very short duration: in March the infection spread again, and in April increased with such rapidity, that we were obliged to retire to our close quarters on the 26th of that month. I have now the satisfaction of informing you that, by the blessing of Providence, we are once more safe and at liberty,

liberty, tho' after a confinement more tedious, and much more dismal than even that of the last year; we got abroad on the 18th of August, when the burials were reduced to about 20 a day: the infection gradually decreased till the middle of September, since which time we have heard of no accident. May the Almighty graciously be pleased to prevent the return of a distemper, whose very name strikes terror whenever it is mentioned and is undoubtedly one of the most lamentable misfortunes, that mankind is liable to.

I wish I could with any precision determine our loss in the two last summers; but, in times of such general horror and confusion, it is in a manner impossible to come at the exact truth. If you enquire of the natives, they swell the account each year from 40 to 60 thousand, and some even higher; but, as the eastern disposition to exaggeration reigns at present almost universally, little accuracy is to be expected from them: this however is certain, that the mortality of this year has been very considerable, perhaps not much inferior to any in this century. Some of the Europeans have been at no small pains and expence to procure a regular and daily list of the funerals during our confinement, and their account amounts to about twenty thousand, from the 1st of April to the 1st of September this year, and about one third less the preceding summer. This calculation I am inclined to think is pretty right, tho' there are some strong objections against a probability of being able to procure a just one in such circumstances; for the Turks keep no register of the dead, and have

72 different public burial places in the 7 miles circumference of the city, besides many private ones within the walls. The Christians and Jews, who are supposed to be rather less than a seventh part of the number of inhabitants, have registers, and each nation one burial place only: their loss this year is about 3500 in the five months.

I will not shock your compassionate disposition by a detail of the miseries I have been witness to, but only mention, that during the months of June and July, (in the greatest part of which the burials were from 2 to 300 a day,) the noise of men singing before the corps in the day, and the shrieks of women for the dead both day and night, were seldom out of our ears. Custom soon rendered the first familiar to me; but nothing could reconcile me to the last; and as the heat obliges us to sleep on the terrace of our houses in the summer, many of my nights rest was disturbed by these alarms of death.

I bless God, all my countrymen have been so fortunate as to escape any infection in their houses, tho' each year 4 or 5 Europeans have been carried off, and each year the plague broke out in two houses that join to ours. In one of them this year died a Franciscan Priest, after two days illness, whose bed was placed about six yards distance from mine. I believe I was in no great danger, as a wall 9 or 10 feet high separated our terraces; but had I known his situation, I should have moved farther off. The year before, I was thrown into a very great agitation of mind for a few days, by the death of my laundress's husband; for the very day he died of the plague, my servant had

had received my linen from his house, and I had carelessly put on some of it, even without airing. This accident happened many weeks after we were open, and his illness was industriously kept a secret. The last month of my confinement this year passed very heavily with me indeed; for I found my health much disordered. Whether it proceeded from a cold I caught in my head by sleeping in the open air in some very windy nights; from want of exercise; or from the uneasiness of mind naturally attending our melancholy situation, I know not; but my nerves seemed all relaxed, my spirits in a state of dejection unknown to me before, and my head so heavy and confused, that I could neither write nor read for an hour together with application or pleasure. Since our release, I have passed a month at a garden about an hour's ride from the city, for the sake of exercise and fresh air, and find myself much relieved by it, tho' my head is far from being yet clear.

Among many particulars relating to the present plague, that I have heard, the following anecdotes seem somewhat extraordinary; and yet, as they are well attested, I have no reason to doubt of the truth of them; viz. Last year as well as this, there has been more than one instance of a woman's being delivered of an infected child, with the plague sores on its body, tho' the mother herself has been entirely free from the distemper.

A woman, that suckled her own child of 5 months, was seized with a most severe plague, and died after a week's illness; but the child, tho' it sucked her, and lay in the same bed with her during her whole disorder,

order, escaped the infection. A woman upwards of an hundred years of age was attacked with the plague, and recovered: her two grandchildren of 12 and 16 received the infection from her, and were both carried off by it.

While the plague was making terrible ravage in the island of Cyprus, in the spring of 1760, a woman, remarkably sanguine and corpulent, after losing her husband and two children, who died of the plague in her arms, made it her daily employment from a principle of charity to attend all her sick neighbours, that stood in need of her assistance, and yet escaped the infection. Also a Greek lad made it his business for many months to wait on the sick, to wash, dress and bury the dead, and yet he remained unhurt. In that contagion ten men were said to die to one woman; but the persons, to whom it was almost universally fatal, were youths of both sexes. Many places were left so bare of inhabitants, as not to have enough left, to gather in the fruits of the earth: it ceased entirely in July 60, and has not appeared in the island since.

The plague seems this year to have been in a manner general over a great part of the Ottoman empire. We have advice of the havoc it has made at Constantinople, Smyrna, Salonicha, Brusa, Adena, Antioch, Antab, Killis, Ourfah, Diarbekir, Mousel, and many other large towns and villages. Scanderoon, for the first time I believe this century, has suffered considerably: the other Frank settlements on the sea-coast of Syria have been exempted, excepting a few accidents at Tripoli, which drove the English consul,

ful, Mr. Abbott, into a close retirement for a week or two; but the storm soon blew over.

I am, with the greatest respect,

Sir,

Your most obliged and most

obedient humble servant,

Thomas Dawes.

November 4th, 1762.

P.S. Praise be to the Almighty, we still continue free from any bad accident: the 40 days necessary for a clean bill of health are expired; and the Reward, Captain Saunders, is taking in her loading for England.

XIII. Observations on Sand Iron: In a Letter from Mr. Henry Horne, to Mr. John Ellicot, F. R. S.

SIR,

Read March 3,
1763.

AS the affair of the rich American Iron Ore, commonly known by the name of the Virginia black sand, has of late not only engaged the conversation of many of the Virtuosi, but has been taken very particular notice of by the Society for the encouragement of arts and manufactures; I thought myself obliged, for many reasons, to lay before you whatever has come to my knowledge relating to this discovery, either from my own experiments, or from the information of others. And I engage in this service with the greater pleasure, as I look upon it to be one of the most interesting discoveries, with regard to this useful metal, that has come to our knowledge for some ages, and, if rightly conducted, may prove of infinite service to us in this part of the world, as well as to the inhabitants of our colonies, where (as it has been supposed, though without sufficient foundation) this discovery was first made.

Without any farther preface or apology, permit me to remind you, that, in a conversation which formerly passed between us upon this subject, I acquainted you, that, about twenty years since, I was engaged in making a variety of experiments upon the nature of Iron Ores, and Steel; and that I then made a very

very particular enquiry into the nature of this black sand, and, in the course of these experiments, several very interesting phenomena discovered themselves, which, as they might be of great service to the world in general, and more especially to such as are concerned in smelting of the iron from the ore, I had thoughts of communicating to the publick; but, as my business will not permit me to go through the whole at present, I shall confine myself to what relates to the black sand.

I procured, from Mr. Adams the Virginia merchant, a sufficient quantity of the sand, and, in order to estimate its comparative weight with that of iron ore, I procured some of the richest ore I could get, which having reduced to powder, I filled an ordinary tea-cup with it. I afterwards filled the same cup with some of the sand, and upon comparing the weights with each other, I found that the weight of the sand was to that of the ore as 3 to 2; and having taken notice how readily the sand was attracted by the magnet, I was convinced that the sand must certainly contain a very considerable quantity of Iron, and therefore determined to make trial of it. I was however, for some time, interrupted in my design, by information I received from a friend, that such an enquiry had been made many years before, by a member of the Royal Society, and a gentleman of esteem as a chemist, but without success; and that the experiments were published in the 2d vol. of Lowthorp's Abridgment of the Philosophical Transactions. As this account is very short as well as curious, I shall take the liberty to give it you entire, with some few remarks upon it.

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H

A black

"A black shining sand from Virginia examined by Dr. Ali. Moulen.

A small vial filled with ordinary white sand, and containing only 3 i. gr. xi. being filled with the Virginia sand, was found to contain 3ij. 3ij. gr. i.

This sand did apply to the magnet both before and after calcination; but the latter did apply better to it than the former.

A parcel of this sand, mixed and calcined with powdered charcoal, and kept in a melting furnace for about an hour, yielded no regulus: but applied more vigorously to the Loadstone than either of the former.

I fluxed a parcell of this sand with fixed nitre, in a melting furnace, for above an hour, but could obtain no regulus; nor any substance that would apply to the magnet, except a thin crust that stuck firmly to a piece of charcoal that dropt into the crucible when the matter was in fusion.

I fluxed it also with salt-petre and powdered charcoal, dropping pieces of charcoal afterwards into the crucible. It continued about half an hour in the melting furnace in fusion, and that without producing a regulus, or a substance that would apply to the magnet, excepting only what stuck to the charcoal as in the former experiment.

I fluxed another parcel of it with salt-petre and flower of brimstone, without being able to procure any regulus.

I poured good spirit of salt on a parcel of this sand, but could observe no fluctuation thereby produced.

I poured spirit of nitre, both strong and weakened with

with water, on parcels of the same sand, without being able to discover any conflict.

I poured single aqua fortis upon another parcel of it, without being able to perceive any ebullition worth noting.

I used also double aqua fortis upon another parcel of it, which, for ought I could discover, had no more effect on it than the former.

I poured some aqua regia on a parcel of it, without discovering any sensible effect. I poured good oil of vitriol upon another parcel of this sand; but seeing no bubbles thereby produced, I weakened the oil with water, but without any visible effect.

I repeated all the former experiments with the menstruums upon this sand after calcination per se in a crucible, but could scarce observe a bubble produced by any of them.

I poured some of each of the liquors upon parcels of the powder of this sand calcined, without any success.

Note, that I made these experiments both in the cold, and upon a sand furnace. So that to me there seems to be but little wanting to discover any metal known to us, if it contained any such: for there is no metal nor ore that some of these menstruums will not work on.

I powdered a fragment of a loadstone, and poured some of these menstruums upon it, without being able to find that they in the least preyed upon it, any more than they did upon the sand.

I poured some of the aforementioned menstruum upon ordinary sand taken out of a sand furnace, where it must have suffered some calcination; but could

and no more bubbles produced thereby, than what might rationally be supposed to be produced from lime, and other dirt mixed with the sand."

Having thoroughly considered these experiments, they appeared to me far from being decisive, and that if the Doctor had placed more confidence in the power of the magnet, and less in his menstruum, he would rather have concluded that there might be some sorts of iron ore which his menstruum would not touch in the moist way, nor any regulus be produced from them in the dry, as he made use of them, which yet might, under some other hands, be subdued, by more apt and powerful methods than any which at that time he was acquainted with.

However I apprehended I might fairly draw this conclusion from his experiments, viz. that the sand was not altogether and simply iron, but that it was strongly united with a very stubborn, fixed, and permanent earth, which could not be separated from it without some extraordinary, as well as powerful means; but I could not think this a sufficient objection to the prosecution of an experiment, which, if it succeeded, might be attended with very happy consequences. Proceeding therefore upon this supposition, I mixt up about 8 or 9 ounces of the sand, with a proportional quantity of a strong corrosive flux, which I put together into a crucible, and committed it to a very strong fire in an excellent wind-furnace, where I kept it for between two and three hours, hoping by this means to have answered the intended purpose; but I confess I was not a little surprised, that, after the crucible was taken from the fire, I could not find

find one single grain of metal in the remaining contents.

This disappointment greatly puzzled me, till having thoroughly examined into the unexpected event, without being able to discover any reason sufficient to incline me to recede from my former opinion, as to the component parts of the sand, I concluded that the flux might possibly be a very improper one; for though it might have effected the intended separation, yet it might at the same time be sufficiently powerful to divide the particles of the metal, when separated, so very minutely, as to be capable of subliming and carrying them off imperceptibly: And finding the contents greatly diminished, so that the quantity remaining bore but a small proportion to that which was first put into the crucible, I concluded that this must really have been the case, and that some very different method must be pursued in order to produce the desired effects. I immediately determined to make a second trial, in which I proceeded in the following manner. I took the same quantity of sand made use of in the former experiment; and first I spread it, without any addition to it, upon an iron plate over a strong fire, where I gave it a very powerful torrefaction (or roasting) to try if, by that means, I could not relax, and loosen the component parts to such a degree, as to make the separation and reduction of the metal more easy, when I should bring it into the furnace. When I had so done, I mixed it up with a flux of a very peculiar, but gentle nature, which I had before made use of for other purposes with great success, and committed it (as in the former experiment) to the furnace, where I urged it by a very strong fire for about three

three hours, and upon taking it out, I found the event answerable to my most sanguine expectations : for in the bottom of the crucible I found, as near as I can remember, rather more than half of the sand I put into the crucible reduced to a very fine malleable metal.

In this very agreeable experiment I met with a very surprizing phænomenon, which, as I am not at present able to determine whether it was only casual, or what would always happen in the like experiments, you will excuse my divulging at present, especially as you, Sir, by furnishing me with a fresh parcel of the sand, have enabled me to make some farther trials ; which I shall embrace the first opportunity of doing ; and should I be so happy as to confirm what I then observed, or to make any farther discoveries deserving your notice, I shall not fail communicating them to you.

Being fully convinced, by the experiment, that the sand was a very rich iron ore, I acquainted some of my friends with it, who being largely engaged in trade to those parts of our American colonies, where I was informed this sand was to be easily procured, and in very large quantities, I was in great hopes an account of this nature would have inclined some of the gentlemen in that part of the world, to have prosecuted so useful a discovery in a larger way ; and I own I have often wondered, that an affair of such consequence should have lain dormant for so many years.

However I was a few months since pleasingly surprised, to find in the hands of my very ingenious friend Mr. Peter Collinson, not only a pamphlet, but likewise

likewise a letter upon the subject addressed to the Society for encouraging of arts and manufactures, by one Mr. G. Elliott, who relates, that, though previous to his attempt of making iron from this sand, he met with nothing but what was discouraging from the most skillful persons to whom he proposed his design; yet that he had such a persuasion in his own mind of the practicability of the thing, that he could not rest till he had made a trial, and the event proved encouraging much beyond his expectations, insomuch that he could scarcely believe the trial had been fairly made, till a second trial evinced with certainty, that eighty three pounds of the sand would produce a barr of excellent iron weighing fifty pounds: a prodigious yield indeed, and far beyond what I have ever heard of from the richest common ores that are any where to be found; most of the ores I have ever met with or heard of, yield little more than half in pig metal, and which will suffer a waste of near $\frac{1}{2}$ part to make tebleable good barr iron, and much more if I am rightly informed, when the iron is intended for more valuable purposes, such as being drawn into wire, &c.

After I had seen his address in his letter to the Society, and his pamphlet; by the assistance of my friend Mr. Collinson, I sent him over two or three hints, which I judged might be of some service to him; this produced the favour of a letter from him, of which the following is an exact copy.

Mr.

Mr. HENRY HORN,

Sir,

Killingworth, Oct. 4. 1762.

I understand by Mr. Collinson, that you have seen, and greatly approve of, the sample of sand iron which was sent; that you are desirous to know how it was made, and whether it can be made in large barrs. The little barr you saw, was cut off from a barr of 52 pounds and a half, the first that was made at my son's work, the first that was ever made in America, and probably the first that was ever made in the world, in that manner, and so large a barr. I never heard of any attempt made upon the iron sand, till that of yours 20 years ago, of which Mr. Collinson gave me an account in his letter.

As to the manner of making the iron, it is wrought or smelted in a common bloomery, in the same manner as other iron ore is smelted; excepting this difference, this iron sand is so pure, so clean washed, that there is not a sufficient quantity of cinder or flagg to promote and perform the smelting, therefore we add either the flagg which issues from other iron, or else add some bog mine ore, which abounds with cinder; in this way it is as capable of being wrought as rock ore or bog mine.

I was in hopes that if this iron sand could be wrought at all, the particles being so very fine, it would smelt very quick; but herein I found myself mistaken, every particle has a will of its own, and must have its own particular smelting, for instead of its being performed in less time, it took more than common

common iron-ore, but, upon farther experience, and more acquaintance with this sand, the workman has shortened the operation from five hours down to three: if, by any means it might be reduced to the same time with pig-iron, it would be a most useful improvement. If you can afford any directions to hasten the operation, I should be greatly obliged for any instructions.

There is so much of this sand in America, that I am apt to think, that there is more iron ore in this form of sand than in mines.

I have written an essay upon the subject, which I hope Mr. Collinson will let you see, as I hope to see what you are about to publish. My son has a steel furnace, which was erected several years before the act of parliament prohibiting them in the plantations: he has converted some of the sand iron into steel, of which I send you a sample; as also a sample of the iron. As my son had no instructions for making steel, we were forced to hammer out the skill by various trials as we could; so conclude that he is still imperfect, and wants your help and direction to bring it to perfection; in which art I understand that you are a perfect master, and with all kind enough to offer your assistance; for which I am very thankful, and look upon it as an additional favour, if you will be pleased to indulge me with the benefit of your correspondence, for I live in a corner of the world where such information as, I trust, you are able to furnish, will be highly beneficial. Previous to my attempt of making iron from sand, I proposed my project to those who were the most skillful in those affairs, but met with nothing but what was discouraging; yet after all,

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had a persuasion of the practicability of the thing to a degree next to enthusiasm, so that I could not rest till I had made trial. I am glad that the iron has such qualities as to meet with your approbation; I knew that the iron was good, but did not know that it was so good as your superior knowledge has found it. I want to know what such iron will sell for in England, whether it will be worth while to send it. This black sand is a treasure that has long lain hidden from the world, and is what may render the colonies more valuable to Great Britain.

I am, Sir,

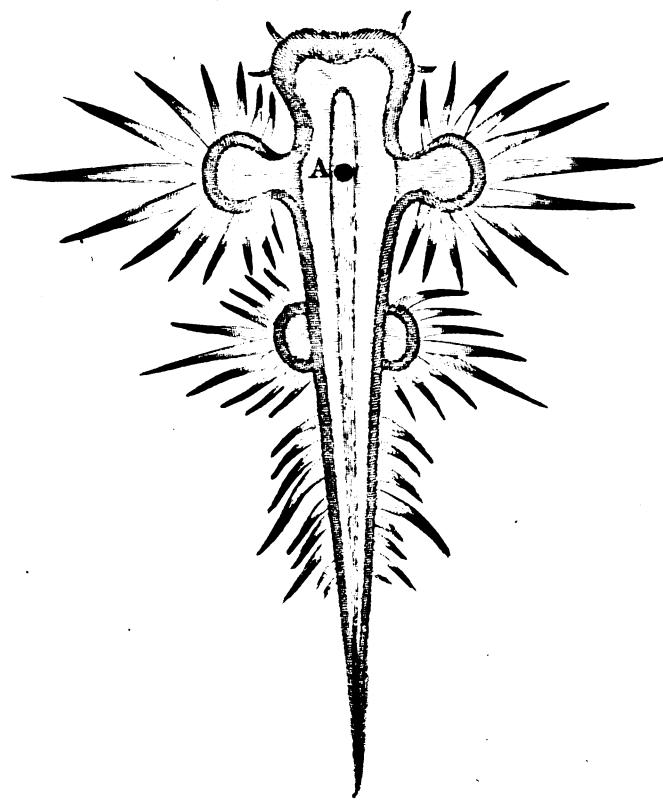
Your most obliged humble servant,

Jared Eliot.

P. S. The bars of iron which have hitherto been made of sand, are from fifty to fifty gross, hope in time to have them reach to seventy pounds weight each; experience must determine that matter, we can do better than at the time the essay was written. We have been visited with a long and sore drought, have done nothing for a long time for want of water.

The samples which accompanied this letter, were two small bars, weighing only a few ounces, one of the iron made from the sand, the other of steel made from the same iron. These bars I have tried, and found that the bar of steel worked extreamly well under the hammer, was very pure and clean, and free

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free from flaws. On the contrary the barr of iron turned out much otherwise, for, though it appeared to bear the force of the hammer, as well as the steel, yet it was not near so pure, but broke out in flaws and hollows, almost through the whole of the barr, and which a welding heat would by no means bring into proper union; this however engaged us to try a different method, which was, when the barr was reduced into a proper size for the purpose, to double it up three times, one part of the barr upon the other, and to try if it would then bear welding and become more consistent, and by this means we found the end perfectly well answered; for it bore the force of the fire and the hammer, and became in a manner perfectly sound. This severe trial proved, to a demonstration, that the iron possess all that agreeable toughness and ductility, for which the Spanish iron is so deservedly famous, without partaking of that vile red-shire quality, for which the latter is very remarkable, and manifestly tends to prove the excellency of this sand iron, when reduced into barr iron under proper care and circumspection.

You will observe, Sir, from the letter, that this sand is so pure, and so clean washed, that their first method of reducing the sand to barr iron proved too tedious, for want of some of those adventitious materials, to promote and perform the smelting, and which always accompanies the common ore, whether it be of the rock or bog kind; which materials, mixing with the matter, made use of by way of flux, and uniting with the ashes of the fuel employed in melting down the ore, is usually run into a thick opaque

glassy substance, forming, as it were, a covering over the metal, which, by its gravity, naturally sinks to the bottom; this the workmen call cinder. Now the want of this matter rendering the operation too tedious, I find they had recourse either to this cinder brought from other iron works, or to a quantity of the bogmine, which, I doubt not, would abundantly furnish matter for cinder. If they had used only the first, and that properly chosen, it might very probably have been of some service, without doing any material injury to the metal; but if the bog mine is used, though the service might be apparently more, yet in all likelihood the injury would be infinitely greater, and I am inclined to believe that something of this kind occasioned the difference observed between the two bars above mentioned, viz. that the one might have been reduced by the help of more pure materials, and the other by the assistance of their bogmine, whose constituent parts abounding with many impurities, some of which, by mixing with the metal, may have occasioned the defects above complained of, and which required so severe an operation both of the fire and hammer to separate from it. I anywherefore of opinion, that as the prosecution of this useful discovery deserves the greatest encouragement, if the Society of arts and manufactures should take it under their patronage, the premium they may think proper to propose should rather be given to the person who shall produce the purest metal, than to him who shall produce the greatest quantity, for otherwise, I am afraid, we shall be deprived of what I should esteem the most valuable part of this discovery, I mean the obtaining

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obtaining a more pure, and better kind kind of iron,
than any we have hitherto been possesst of, - and which
I am certain this sand, under proper management, is
capable of producing.

I am,

Sir, with the greatest respect,

Your most obedient

Feb; 5, 1763.

humble servant,

Henry Horne.

XIV. Ex.

XIV. Extract of a Letter from Simon Peter Pallas, M. D. of Berlin, to Mr. Emmanuel Mendez da Costa, Librarian to the Royal Society, relating to the State of the State of the Cold there last Winter, dated Feb. 12, 1763.

Read March 3, 1763.

WE have had great frosts here, as indeed all over Germany. I have observed myself, on the twenty-seventh of December of last year, a little after seven o'clock in the morning, the Cold to have been so excessive, that the mercury in the thermometer of Fahrenheit stood at four degrees under 0, which is fifteen degrees under 0 of Reaumur's Scale, than which the Cold in 1740 was but very little more intense. Mr. Euler, junior, observed the same day the thermometer at the same degree: about eight and at nine of the clock of that day, the mercury in the barometer stood at the height of 30" 1¹¹¹" the like of which never had been observed at Berlin before.

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XV. An

XV. *An Account of a remarkable Darkness at Detroit, in America: In a Letter from the Rev. Mr. James Stirling, to Mr. John Duncan: communicated by Samuel Mead, Esq; F. R. S.*

Detroit, 25th Oct. 1762.

SIR,

Read March 3,
1763. A Man in business seldom troubles himself about news; yet the following is so uncommon, I cannot neglect acquainting you therewith. Tuesday last, being the 19th instant, we had almost total darkness for the most of the day. I got up at day break: about 10 minutes after I observed it got no lighter than before; the same darkness continued until 9 o'clock, when it cleared up a little. We then, for the space of about a quarter of an hour, saw the body of the Sun, which appeared as red as blood; and more than three times as large as usual. The air all this time, which was very dense, was of a dirty yellowish green colour. I was obliged to light candles to see to dine, at one o'clock, notwithstanding the table was placed close by two large windows. About 3 the darkness became more horrible, which augmented until half past 3, when the wind breezed up from the S. W. and brought on some drops of rain or rather sulphur, and dirt, for it appeared more like the latter than the former, both

both in smell and quality. I took a leaf of clean paper, and held it out in the rain, which rendered it black whenever the drops fell upon it, but, when held near the fire, turned to a yellow colour, and when burned, it fizzed on the paper like wet powder. During this shower, the air was almost suffocating with a strong sulphurous smell; it cleared up a little after the rain. There were various conjectures about the cause of this natural incident. The Indians, and vulgar among the French, said, that the English, which lately arrived from Niagara in the vessel, had brought the plague with them: Others imagined it might have been occasioned by the burning of the woods. But I think it most probable, that it might have been occasioned by the eruption of some volcano, or subterraneous fire, whereby the sulphureous matter may have been emitted in the air, and contained therein, until meeting with some watery clouds, it has fallen down together with the rain.

I am, Sir,

Your most humble servant,

James Stirling,

**XVI. An Account of a remarkable Marine
Insect: In a Letter of Mr. Andrew Pe-
ter Du Pont, to Mr. Emanuel Mendez
da Costa, Librarian to the R. S.**

Dear Sir,

Read March 10,
1763. **M**Y friend Robert Long, Esq; of
Jamaica, favoured me with the
drawing and description of a marine insect he took
up at sea. I believe it is a non-descript, and as you
have often desired me to communicate to you any
observations worthy attention to present to the Royal
Society, I send you this to communicate to that
learned body, if you deem it worthy their notice.
I shall always think it a pleasure to tender my re-
spects in whatever I can to the Society, which pray
assure them of. I am, with great esteem,

Dear Sir,

Your very obliged friend,

and humble servant,

Chiswell-Street,
Jan. 17, 1763.

Andrew Peter Dupont.

August 13, 1762.

IN a calm on my voyage to England, on board the Friendship, captain Thompson, two persons swimming took up this most singular creature floating on the surface. Its motions muscular. Its length a little more than one inch. Four small and short horns, probably its eyes. It protruded them in the water only; an orifice in the front part seeming its mouth. Two round spots opaque, marked A, possibly respiracula. The mid-line of the back part apppeared through a common magnifier like a silver leaf, and was in continual undulating motion, either from the muscles or circulation of juices. Two side lines extending the whole creature's length, and ending in one in the tail of a deep blue. The fingers, or tentacles, end in a deep blue; a silvery cast intermixed with the blue over the whole back, or upper parts, where the blue is lighter. Vide TAB. III.

This figure is a magnified drawing by the common hand-microscope. It can turn itself on the back by a muscular contraction of the head part, the tail and ramified arms inwards. The inferior parts are white.

It died the third day, though the water was shifted once every day.

XVII. *A Letter from Monsieur Wargentin,
Secretary to the Royal Academy of Sciences
in Sweden, to Mr. John Ellicott, F.R.S.
relating to the late Transit of Venus.*

Stockholm, December 24, 1762.

Read March 17, 1763. THE observations upon the last transit of Venus over the Sun made at the Cape of Good Hope are excellent, and seem to decide, that the horizontal parallax of the Sun is $8''$, 1 or $8''$, 3 at most. I had before found it to be that quantity, from the observations made in Europe compared together; but the observations made at the Cape confirm it with the greatest evidence.

It is of importance to be assured of the longitude of the places where the observations were made. I have endeavoured to determine them the best I was able by observations of the eclipses of Jupiter's satellites, made at the same places. That you may the better judge, I thought proper to send you all the observations of these satellites made at different places the last year. I desire you would communicate them to Mr. Mason, and all those who interest themselves in the research of the parallax. It is pity that mes-sieurs L'Abbé Chappe, and Rumoski, did not succeed in observing several eclipses of the satellites, at Tobieské and Selenginsk, the better to confirm the Longitudes of those places. However it appears to me, that the difference between the meridians of

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Greenwich and Tobieské is scarcely more than $4^{\text{h}} 32' 55''$. That between the meridians of Greenwich and Selenginsk, to judge from the three immersions observed there, should be but $7^{\text{h}} 6' 0$, but from other considerations, I think it must be 10 or 15' more. If the longitude of these places should be more exactly determined, I am persuaded that we should obtain the parallax of the Sun to nearly the tenth of a second, so exact the observations made at Selenginsk and Tobieskè and the Cape appear to me.

I see you did not communicate to the Royal Society all the observations made by Mr. Planman at Cajaneberg, they however deserve to be preserved. He observed

| | | | |
|--|----|----|-----|
| The beginning of the entrance - - at | 3 | 59 | 56M |
| Total immersion of Venus or interior contact - - - - - | | 4 | 18 |
| Second interior contact or beginning of the exit - - - - - | | 7 | 59 |
| Total emersion - - - - - | 10 | 26 | 22 |

Mr. Planman made use of a telescope of 20 or 21 feet; the latitude of Cajanebourg is $64^{\circ} 13' 30''$; the difference of meridians between Greenwich and Cajaneburg is sufficiently determined by observations upon the eclipse of the Moon the 18th May 1761, made at Stockholm and Cajaneburg.

I have the honour to be with the greatest esteem,

S I R,

Your very humble and obedient servant,

P. Wargentin.

Eclipses primi satellitis Jovis, observatæ Anno 1761:
addito errore calculi pro quavis observatione, ut ap-
pareat, quæ sint præstantiores. In calculo autem
posui differentiam meridianorum inter

| | h | ' | " |
|-----------------------------------|---|---|-------|
| Observatorium Grenovic. et Paris. | — | 0 | 9 10 |
| Grenov. et Stockholm | — | 1 | 12 0 |
| Grenov. et Upsaliense | — | 1 | 10 20 |
| Grenov. et Massil. | — | 0 | 21 19 |
| Grenov. et Cap. B. Spei | — | 1 | 13 35 |
| Grenov. et Insul. Rodrig. | — | 4 | 12 52 |
| Grenov. et Selenginsk | — | 7 | 6 15 |
| Grenov. et Cajaneburgh | — | 1 | 50 40 |

| | | | | | | |
|--------------|----|----|----|------------------|------|-------|
| Jan. 7. Em. | 5 | 4 | 35 | Stockholmiæ | + 0 | 27 |
| June 13. Im. | 14 | 37 | 47 | Massiliæ | — | 0 21* |
| 20. — | 17 | 22 | 26 | Cap. Bonæ Spei | + 0 | 31 |
| July 22. — | 12 | 45 | 15 | Paris | — | 0 7 |
| | 12 | 35 | 39 | Grenov. | — | 0 33 |
| | 16 | 48 | 55 | Inf. Rodriga | + 0 | 9 |
| 29. — | 14 | 38 | 35 | Paris | — | 0 33 |
| | 14 | 50 | 28 | Massiliæ | — | 0 49 |
| | 15 | 42 | 44 | Cap. B. Spei | — | 0 49 |
| 31. — | 13 | 10 | 29 | Inf. Rodrig. | — | 0 56* |
| Aug. 7. — | 10 | 51 | 52 | Grenovic. | — | 1 8* |
| | 12 | 4 | 51 | Stockholm.bona | + 0 | 9 |
| | 11 | 13 | 49 | Massiliæ | — | 0 30 |
| | 12 | 5 | 46 | Cap. B. Spei | — | 0 49 |
| 14. — | 13 | 59 | 39 | Stockholm.certa. | + 0 | 10 |
| | 14 | 0 | 50 | Cap. B. Sp. | — | 0 34 |
| | | | | | Aug. | |

| | | | | | | | | | |
|--------------|----|----|----|--------------|-------|---|-----|---|-----|
| | | | | | | | | | |
| Aug. 21. Im. | 14 | 51 | 56 | Paris | - - - | - | + 0 | " | 34 |
| | 15 | 56 | 2 | Cap. B. Spei | - | - | + 0 | | 53 |
| 23. — | 9 | 20 | 49 | Paris | - - - | - | + 0 | | 40 |
| | 10 | 22 | 42 | Upsaliæ | - - - | - | - 0 | | 3* |
| | 10 | 25 | 10 | Cap. B. Spei | - | - | + 0 | | 44 |
| 25. — | 10 | 46 | 24 | Selenginsk | - - | - | + I | | 6* |
| 28. — | 16 | 48 | 41 | Paris | - - - | - | - 0 | | 12* |
| 30. — | 11 | 7 | 58 | Grenovici | - - | - | + 0 | | 27 |
| | 12 | 17 | 43 | Upsaliæ | - - - | - | + I | | 2* |
| | 12 | 21 | 32 | Cap. B. Spei | - | - | + 0 | | 28 |
| | 12 | 58 | 50 | Cajaneburgi | - | - | + 0 | | 15 |
| Sept. 1. — | 9 | 49 | 40 | Inf. Rodrig. | - | - | + 0 | | 40 |
| 8. — | 7 | 55 | 37 | Massiliæ | - - - | - | - 0 | | 17* |
| | 9 | 23 | 40 | Cajaneburgi | - | - | + I | I | * |
| 15. — | 9 | 39 | 55 | Paris | - - - | - | + 0 | | 5 |
| 24. Em. | 8 | 7 | 46 | Grenovici | - - | - | + I | I | |
| | 8 | 29 | 11 | Massiliæ | - - - | - | + 0 | | 55 |
| | 9 | 21 | 35 | Cap. B. Spei | - | - | + 0 | | 47 |
| Oct. 1. — | 10 | 13 | 56 | Paris | - - - | - | + 0 | | 59 |
| 10. — | 6 | 40 | 8 | Paris | - - - | - | + 0 | | 45 |
| 17. — | 8 | 36 | 38 | Paris | - - - | - | + 0 | | 46 |
| Nov. 9. — | 8 | 51 | 19 | Paris | - - - | - | + I | | 48* |
| 16. — | 10 | 46 | 26 | Paris | - - - | - | + I | | 6 |
| 18. — | 5 | 27 | 40 | Massiliæ | - - - | - | + 0 | | 32* |
| Dec. 2. — | 10 | 5 | 4 | Stockholmiæ | - | - | + 0 | | 32* |
| 4. — | 4 | 33 | 8 | Stockholmiæ | - | - | + 0 | | 39 |
| 11. — | 6 | 25 | 44 | Stockholmiæ | - | - | + 0 | | 43 |

Obser-

Observationes secundi satellitis, eodem anno habitæ.

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|--------------|----|----|----|----------------|-------|-----|--------------|
| June 11. Im. | 14 | 51 | 24 | Cap. B. Spei | - | + 0 | 14 |
| 18. — | 17 | 27 | 11 | Cap. B. Spei | - | + 0 | 4 |
| July 13. — | 13 | 28 | 53 | Paris | - - - | + 0 | 9 |
| 20. — | 16 | 5 | 1 | Paris | - - - | + 0 | 35 |
| | 15 | 54 | 28 | Grenovici | - - | + 1 | 58* |
| | 17 | 9 | 42 | Caput B. Spei | + 0 | 19 | |
| 31. — | 14 | 57 | 17 | Selenginsk | - - | + 1 | 41* |
| Aug. 7. — | 10 | 30 | 9 | Grenovici | - - | + 0 | 59 |
| | 11 | 43 | 1 | Stockh. optima | + 0 | 7 | |
| | 11 | 43 | 26 | Cap. B. Spei | - + 1 | 17* | |
| 14. — | 14 | 21 | 37 | Stockh. certa | - + 0 | 17 | |
| | 13 | 18 | 39 | Paris | - - - | + 0 | 25 |
| 25. — | 12 | 15 | 4 | Selenginsk | - - | + 0 | 9 |
| Sept. 1. — | 9 | 1 | 6 | Stockh. certa | - - 0 | 5 | |
| | 9 | 2 | 20 | Cap. B. Spei | - + 0 | 16 | |
| | 8 | 59 | 19 | Upsaliæ | - - - | + 1 | 2* |
| 8. — | 10 | 37 | 59 | Parisiis | - - - | + 0 | 26 |
| | 10 | 29 | 30 | Grenovici | - - - | - 0 | 15 |
| | 10 | 50 | 40 | Massiliæ | - - - | - 0 | 6 |
| | 11 | 42 | 20 | Cap. B. Spei. | - + 0 | 30 | |
| 15. — | 13 | 6 | 36 | Grenovici | - - | + 3 | 12* |
| Oct. 28. Em. | 8 | 9 | 51 | Massiliæ | - - - | - 0 | 7 |
| Nov. 11. — | 13 | 12 | 43 | Paris | - - - | - 0 | 2 |
| 22. — | 5 | 18 | 12 | Massiliæ | - - - | + 0 | 49 |
| | 6 | 9 | 24 | Stockh. dub. | - + 0 | 18* | |
| Dec. 6. — | 10 | 15 | 59 | Paris | - - - | + 0 | 46 |

Eclipses

Eclipses tertii satellitis Jovis, observatæ anno 1761.

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|--------------|-------|-----|----------------|-----------|-----|------|
| Maj. 11. Im. | 15 19 | " 3 | Paris | - - - - - | 3 | 30 " |
| Jun. 16. Em. | 15 26 | 32 | Cap. B. Spei | - - 2 | 21 | * |
| 23. Im. | 15 20 | 6 | Paris | - - - - - | 5 | 20 |
| | 15 32 | 20 | Maffiliæ | - - - - - | 5 | 25 |
| | 15 10 | 24 | Grenovici | - - - - - | 4 | 48 |
| Jul. 29. — | 11 18 | 49 | Paris | - - - - - | 4 | 10* |
| | 12 23 | 23 | Stockh. certa | - - - - - | 5 | 54 |
| | 12 24 | 0 | Cap. B. Spei | - - - - - | 5 | 8 |
| Em. | 15 17 | 13 | Cap. B. Spei | + 0 | 23 | |
| | 15 15 | 22 | Stockh. certa | + 0 | 51 | |
| Aug. 5. Im. | 15 34 | 51 | Paris | - - - - - | 6 | 18 |
| Sept. 3. — | 8 36 | 36 | Cap. B. Spei | - - - - - | 5 | 25 |
| | 9 13 | 39 | Cajaneburg | - - - - - | 5 | 4 |
| 10. — | 11 27 | 6 | Grenovici | - - - - - | 4 | 53 |
| Nov. 21. Em. | 6 41 | 9 | Paris | - - - - - | + 0 | 15 |
| | 6 52 | 44 | Maffiliæ | - - - - - | + 0 | 51 |
| 28. Im. | 8 10 | 29 | Paris | - - - - - | 5 | 24 |
| | 8 23 | 31 | Maffiliæ | - - - - - | 6 | 18 |
| | 9 14 | 22 | Stockh. optima | - - - - - | 6 | 28 |
| Em. | 10 40 | 57 | Paris | - - - - - | 0 | 16 |
| | 10 52 | 55 | Maffiliæ | - - - - - | 0 | 6 |
| | 11 44 | 34 | Stockh. dub. | - - - - - | 1 | 4* |

Eclipses

Eclipses quarti satellitis, observatae Anno 1761.

| | | | | | | | |
|--------------|----|----|----|-------------------|-------------|----|----|
| Aug. 10. Im. | 14 | 25 | 15 | Parisiis | - - - - + | 2 | 18 |
| | 15 | 32 | 57 | Cap. bonæ Spei | - - 0 | 59 | |
| 27. — | 8 | 41 | 40 | Grenovici | - - - 0 | 21 | |
| | 9 | 2 | 3 | Maffiliæ | - - - + 0 | 35 | |
| | 9 | 53 | 32 | Cap. B. Spei | - + 1 | 22 | |
| | 9 | 50 | 18 | Upsaliæ | - - - + 1 | 21 | |
| | 10 | 16 | 23 | Tornæ | - - - - + 1 | 34 | |
| Em. | 11 | 47 | 41 | Paris | - - - - + 3 | 10 | |
| | 12 | 1 | 52 | Maffiliæ | - - - + 1 | 8* | |
| | 12 | 49 | 0 | Cap. B. Spei. | - + 6 | 16 | |
| | 12 | 48 | 7 | Upsaliæ | - - - + 3 | 54 | |
| | 12 | 47 | 31 | Stockh. certa | - + 6 | 10 | |
| | 13 | 15 | 13 | Tornæ | - - - - + 3 | 6 | |
| Nov. 2. Im: | 10 | 51 | 47 | Maffiliæ | - - - + 5 | 50 | |
| Em. | 12 | 57 | 37 | Maffiliæ | - - - + 7 | 44 | |
| 19. Im. | 6 | 6 | 34 | Stockh. tub. max. | - 8 | 51 | |
| | 6 | 2 | 30 | Ibid. tub. minore | - 4 | 47 | |
| | 4 | 57 | 16 | Paris | - - - - - 2 | 23 | |
| Em. | 6 | 56 | 29 | Paris | - - - - + 6 | 32 | |

Observationes asterisco * notatæ, minus certas suspicor. Differentiæ inter reliquas tribuendæ videntur differentiis telescopiorum, majori minorive aëris serenitati, et fortassis cuidam incertitudini circa differentias meridianorum.

Observationes Stockholmenses habitæ sunt telescopio novo Dollondiano 10 ped. objecta 120^{es} secundum diametros amplificante.

both in smell and quality. I took a leaf of clean paper, and held it out in the rain, which rendered it black whenever the drops fell upon it; but, when held near the fire, turned to a yellow colour, and when burned, it fizzed on the paper like wet powder. During this shower, the air was almost suffocating with a strong sulphurous smell; it cleared up a little after the rain. There were various conjectures about the cause of this natural incident. The Indians, and vulgar among the French, said, that the English, which lately arrived from Niagara in the vessel, had brought the plague with them: Others imagined it might have been occasioned by the burning of the woods. But I think it most probable, that it might have been occasioned by the eruption of some volcano, or subterraneous fire, whereby the sulphureous matter may have been emitted in the air, and contained therein, until meeting with some watery clouds, it has fallen down together with the rain.

I am, Sir,

Your most humble servant,

James Stirling.

XVI. An Account of a remarkable Marine Insect: In a Letter of Mr. Andrew Peter Du Pont, to Mr. Emanuel Mendez da Costa, Librarian to the R. S.

Dear Sir,

Read March 10,
1763. **M**Y friend Robert Long, Esq; of Jamaica, favoured me with the drawing and description of a marine insect he took up at sea. I believe it is a non-descript, and as you have often desired me to communicate to you any observations worthy attention to present to the Royal Society, I send you this to communicate to that learned body, if you deem it worthy their notice. I shall always think it a pleasure to tender my respects in whatever I can to the Society, which pray assure them of. I am, with great esteem,

Dear Sir,

Your very obliged friend,

and humble servant,

Chiswell-Street,
Jan. 17, 1763.

Andrew Peter Dupont.

August 13, 1762.

IN a calm on my voyage to England, on board the Friendship, captain Thompson, two persons swimming took up this most singular creature floating on the surface. Its motions muscular. Its length a little more than one inch. Four small and short horns, probably its eyes. It protruded them in the water only ; an orifice in the front part seeming its mouth. Two round spots opaque, marked A, possibly respiracula. The mid-line of the back part apppeared through a common magnifier like a silver leaf, and was in continual undulating motion, either from the muscles or circulation of juices. Two side lines extending the whole creature's length, and ending in one in the tail of a deep blue. The fingers, or tentacles, end in a deep blue ; a silvery cast intermixed with the blue over the whole back, or upper parts, where the blue is lighter. Vide TAB. III.

This figure is a magnified drawing by the common hand-microscope. It can turn itself on the back by a muscular contraction of the head part, the tail and ramified arms inwards. The inferior parts are white.

It died the third day, though the water was shifted once every day.

**XVII. A Letter from Monsieur Wargentin,
Secretary to the Royal Academy of Sciences
in Sweden, to Mr. John Ellicott, F.R.S.
relating to the late Transit of Venus.**

Stockholm, December 24, 1762.

Read March 17, 1763. THE observations upon the last transit of Venus over the Sun made at the Cape of Good Hope are excellent, and seem to decide, that the horizontal parallax of the Sun is $8''$, 1 or $8''$, 3 at most. I had before found it to be that quantity, from the observations made in Europe compared together; but the observations made at the Cape confirm it with the greatest evidence.

It is of importance to be assured of the longitude of the places where the observations were made. I have endeavoured to determine them the best I was able by observations of the eclipses of Jupiter's satellites, made at the same places. That you may the better judge, I thought proper to send you all the observations of these satellites made at different places the last year. I desire you would communicate them to Mr. Mason, and all those who interest themselves in the research of the parallax. It is pity that messieurs L'Abbé Chappe, and Rumoski, did not succeed in observing several eclipses of the satellites, at Tobieské and Selenginsk, the better to confirm the Longitudes of those places. However it appears to me, that the difference between the meridians of

[60]

Greenwich and Tobieské is scarcely more than $4^{\text{h}} 32' 55''$. That between the meridians of Greenwich and Selenginsk, to judge from the three immersions observed there, should be but $7^{\text{h}} 6' 0$, but from other considerations, I think it must be 10 or 15' more. If the longitude of these places should be more exactly determined, I am persuaded that we should obtain the parallax of the Sun to nearly the tenth of a second, so exact the observations made at Selenginsk and Tobieskè and the Cape appear to me.

I see you did not communicate to the Royal Society all the observations made by Mr. Planman at Cajaneberg, they however deserve to be preserved. He observed

| | | |
|--|----|--------------------------------|
| | | $\text{h } \text{m } \text{s}$ |
| The beginning of the entrance - - | at | 3 59 56M |
| Total immersion of Venus or interior contact - - - - - | } | — 4 18 5 |
| Second interior contact or beginning of the exit - - - - - | } | — 10 7 59 |
| Total emersion - - - - - | — | 10 26 22 |

Mr. Planman made use of a telescope of 20 or 21 feet; the latitude of Cajanebourg is $64^{\circ} 13' 30''$; the difference of meridians between Greenwich and Cajaneburg is sufficiently determined by observations upon the eclipse of the Moon the 18th May 1761, made at Stockholm and Cajaneburg.

I have the honour to be with the greatest esteem,
S I R,

Your very humble and obedient servant,

P. Wargentin.

Eclipses primi satellitis Jovis, observatæ Anno 1761:
 addito errore calculi pro quavis observatione, ut ap-
 pareat, quæ sint præstantiores. In calculo autem
 posui differentiam meridianorum inter

| | ^h | ['] | ["] |
|-----------------------------------|--------------|--------------|--------------|
| Observatorium Grenovic. et Paris. | — | 0 | 9 10 |
| Grenov. et Stockholm | — | 1 | 12 0 |
| Grenov. et Upsaliense | — | 1 | 10 20 |
| Grenov. et Massil. | — | 0 | 21 19 |
| Grenov. et Cap. B. Spei | — | 1 | 13 35 |
| Grenov. et Insul. Radrig. | — | 4 | 12 52 |
| Grenov. et Selenginsk | — | 7 | 6 15 |
| Grenov. et Cajaneburgh | — | 1 | 50 40 |

| | | | |
|--------------|----------|------------------|--------------|
| Jan. 7. Em. | 5 4 35 | Stockholmiæ | + 0 27 |
| June 13. Im. | 14 37 47 | Massiliæ | — — — 0 21* |
| 20. — | 17 22 26 | Cap. Bonæ Spei | + 0 31 |
| July 22. — | 12 45 15 | Paris | — — — + 0 7 |
| | 12 35 39 | Grenov. | — — — + 0 33 |
| | 16 48 55 | Ins. Rodriga | — + 0 9 |
| 29. — | 14 38 35 | Paris | — — — + 0 33 |
| | 14 50 28 | Massiliæ | — — — + 0 49 |
| | 15 42 44 | Cap. B. Spei | — + 0 49 |
| 31. — | 13 10 29 | Ins. Rodrig. | — + 0 56* |
| Aug. 7. — | 10 51 52 | Grenovic. | — — + 1 8* |
| | 12 4 51 | Stockholm.bona | + 0 9 |
| | 11 13 49 | Massiliæ | — — — + 0 30 |
| | 12 5 46 | Cap. B. Spei | — + 0 49 |
| 14. — | 13 59 39 | Stockholm.certa. | + 0 10 |
| | 14 0 50 | Cap. B. Sp. | — + 0 34 |
| | | | Aug. |

| | | | | | | | | | | | | | | |
|-------|-----|-----|----|----|----|--------------|---|---|---|---|----|-----|---|---|
| | | | | | | | | | | | | | | |
| Aug. | 21. | Im. | 14 | 51 | 56 | Paris | - | - | - | - | +○ | 34 | " | " |
| | | | 15 | 56 | 2 | Cap. B. Spei | - | - | - | - | +○ | 53 | | |
| | 23. | - | 9 | 20 | 49 | Paris | - | - | - | - | +○ | 40 | | |
| | | | 10 | 22 | 42 | Upfaliae | - | - | - | - | -○ | 3* | | |
| | | | 10 | 25 | 10 | Cap. B. Spei | - | - | - | - | +○ | 44 | | |
| | 25. | - | 10 | 46 | 24 | Selenginsk | - | - | - | - | +I | 6* | | |
| | 28. | - | 16 | 48 | 41 | Paris | - | - | - | - | -○ | 12* | | |
| | 30. | - | 11 | 7 | 58 | Grenovici | - | - | - | - | +○ | 27 | | |
| | | | 12 | 17 | 43 | Upfaliae | - | - | - | - | +I | 2* | | |
| | | | 12 | 21 | 32 | Cap. B. Spei | - | - | - | - | +○ | 28 | | |
| | | | 12 | 58 | 50 | Cajaneburgi | - | - | - | - | +○ | 15 | | |
| Sept. | 1. | - | 9 | 49 | 40 | Ins. Rodrig. | - | - | - | - | +○ | 40 | | |
| | 8. | - | 7 | 55 | 37 | Maffiliae | - | - | - | - | -○ | 17* | | |
| | | | 9 | 23 | 40 | Cajaneburgi | - | - | - | - | +I | 1* | | |
| | 15. | - | 9 | 39 | 55 | Paris | - | - | - | - | +○ | 5 | | |
| | 24. | Em. | 8 | 7 | 46 | Grenovici | - | - | - | - | +I | I | | |
| | | | 8 | 29 | 11 | Maffiliae | - | - | - | - | +○ | 55 | | |
| | | | 9 | 21 | 35 | Cap. B. Spei | - | - | - | - | +○ | 47 | | |
| Oct. | 1. | - | 10 | 13 | 56 | Paris | - | - | - | - | +○ | 59 | | |
| | 10. | - | 6 | 40 | 8 | Paris | - | - | - | - | +○ | 45 | | |
| | 17. | - | 8 | 36 | 38 | Paris | - | - | - | - | +○ | 46 | | |
| Nov. | 9. | - | 8 | 51 | 19 | Paris | - | - | - | - | +I | 48* | | |
| | 16. | - | 10 | 46 | 26 | Paris | - | - | - | - | +I | 6 | | |
| | 18. | - | 5 | 27 | 40 | Maffiliae | - | - | - | - | +○ | 32* | | |
| Dec. | 2. | - | 10 | 5 | 4 | Stockholmiae | - | - | - | - | +○ | 32* | | |
| | 4. | - | 4 | 33 | 8 | Stockholmiae | - | - | - | - | +○ | 39 | | |
| | 11. | - | 6 | 25 | 44 | Stockholmiae | - | - | - | - | +○ | 43 | | |

Obser-

Observationes secundi satellitis, eodem anno habitæ.

| June 11. Im. | 14 | 51 | 24 | Cap. B. Spei | - + o | 14 | " |
|--------------|----|----|----|----------------|-------|-----|-----|
| 18. — | 17 | 27 | 11 | Cap. B. Spei | - + o | 4 | |
| July 13. — | 13 | 28 | 53 | Paris | - - - | + o | 9 |
| 20. — | 16 | 5 | 1 | Paris | - - - | + o | 35 |
| | 15 | 54 | 28 | Grenovici | - - | + i | 58* |
| | 17 | 9 | 42 | Caput B. Spei | + o | 19 | |
| 31. — | 14 | 57 | 17 | Selenginsk | - - | + i | 41* |
| Aug. 7. — | 10 | 30 | 9 | Grenovici | - - | + o | 59 |
| | 11 | 43 | 1 | Stockh. optima | + o | 7 | |
| | 11 | 43 | 26 | Cap. B. Spei | - + i | 17* | |
| 14. — | 14 | 21 | 37 | Stockh. certa | - + o | 17 | |
| | 13 | 18 | 39 | Paris | - - - | + o | 25 |
| 25. — | 12 | 15 | 4 | Selenginsk | - - | + o | 9 |
| Sept. 1. — | 9 | 1 | 6 | Stockh. certa | - - | o | 5 |
| | 9 | 2 | 20 | Cap. B. Spei | - + o | 16 | |
| | 8 | 59 | 19 | Upsaliæ | - - - | + i | 2* |
| 8. — | 10 | 37 | 59 | Parisiis | - - - | + o | 26 |
| | 10 | 29 | 30 | Grenovici | - - - | o | 15 |
| | 10 | 50 | 40 | Massiliæ | - - - | o | 6 |
| | 11 | 42 | 20 | Cap. B. Spei. | - + o | 30 | |
| 15. — | 13 | 6 | 36 | Grenovici | - - | + 3 | 12* |
| Oct. 28. Em. | 8 | 9 | 51 | Massiliæ | - - - | o | 7 |
| Nov. 11. — | 13 | 12 | 43 | Paris | - - - | o | 2 |
| 22. — | 5 | 18 | 12 | Massiliæ | - - - | + o | 49 |
| | 6 | 9 | 24 | Stockh. dub. | - + o | 18* | |
| Dec. 6. — | 10 | 15 | 59 | Paris | - - - | + o | 46 |

Eclipses

Eclipses tertii satellitis Jovis, observatae anno 1761.

| Maj. 11. Im. | 15 | 19 | 3 | Paris | - - - - - | - | 3 | 30 | 1 | 11 | " | | | |
|--------------|----|----|----|----------------|-----------|-----|-----|-----|---|----|---|--|--|--|
| Jun. 16. Em. | 15 | 26 | 32 | Cap. B. Spei | - - - - - | 2 | 21* | | | | | | | |
| 23. Im. | 15 | 20 | 6 | Paris | - - - - - | - | 5 | 20 | | | | | | |
| | 15 | 32 | 20 | Massiliæ | - - - - - | - | 5 | 25 | | | | | | |
| | 15 | 10 | 24 | Grenovici | - - - - - | - | 4 | 48 | | | | | | |
| Jul. 29. — | 11 | 18 | 49 | Paris | - - - - - | - | 4 | 10* | | | | | | |
| | 12 | 23 | 23 | Stockh. certa | - - - - - | - | 5 | 54 | | | | | | |
| | 12 | 24 | 0 | Cap. B. Spei | - - - - - | - | 5 | 8 | | | | | | |
| Em. | 15 | 17 | 13 | Cap. B. Spei | - | + 0 | 23 | | | | | | | |
| | 15 | 15 | 22 | Stockh. certa | - | + 0 | 51 | | | | | | | |
| Aug. 5. Im. | 15 | 34 | 51 | Paris | - - - - - | - | 6 | 18 | | | | | | |
| Sept. 3. — | 8 | 36 | 36 | Cap. B. Spei | - - - - - | - | 5 | 25 | | | | | | |
| | 9 | 13 | 39 | Cajaneburg | - - - - - | - | 5 | 4 | | | | | | |
| 10. — | 11 | 27 | 6 | Grenovici | - - - - - | - | 4 | 53 | | | | | | |
| Nov. 21. Em. | 6 | 41 | 9 | Paris | - - - - - | + 0 | 15 | | | | | | | |
| | 6 | 52 | 44 | Massiliæ | - - - - - | + 0 | 51 | | | | | | | |
| 28. Im. | 8 | 10 | 29 | Paris | - - - - - | - | 5 | 24 | | | | | | |
| | 8 | 23 | 31 | Massiliæ | - - - - - | - | 6 | 18 | | | | | | |
| | 9 | 14 | 22 | Stockh. optima | - - - - - | - | 6 | 28 | | | | | | |
| Em. | 10 | 40 | 57 | Paris | - - - - - | - | 0 | 16 | | | | | | |
| | 10 | 52 | 55 | Massiliæ | - - - - - | - | 0 | 6 | | | | | | |
| | 11 | 44 | 34 | Stockh. dub. | - - - - - | - | 1 | 4* | | | | | | |

Eclipses

Eclipses quarti satellitis, observatae Anno 1761.

| | | | | | | |
|--------------|----|----|----|-------------------|-------------|------|
| | | | | | | |
| Aug. 10. Im. | 14 | 25 | 15 | Parisiis | - - - - + | 2 18 |
| | 15 | 32 | 57 | Cap. bonæ Spei | - - o | 59 |
| 27. — | 8 | 41 | 40 | Grenovici | - - - - o | 21 |
| | 9 | 2 | 3 | Maffiliæ | - - - + o | 35 |
| | 9 | 53 | 32 | Cap. B. Spei | - + I | 22 |
| | 9 | 50 | 18 | Upsaliæ | - - - + I | 21 |
| | 10 | 16 | 23 | Tornæ | - - - - + I | 34 |
| Em. | 11 | 47 | 41 | Paris | - - - - + | 3 10 |
| | 12 | 1 | 52 | Maffiliæ | - - - + I | 8* |
| | 12 | 49 | 0 | Cap. B. Spei. | - + 6 | 16 |
| | 12 | 48 | 7 | Upsaliæ | - - - + 3 | 54 |
| | 12 | 47 | 31 | Stockh. certa | - + 6 | 10 |
| | 13 | 15 | 13 | Tornæ | - - - - + 3 | 6 |
| Nov. 2. Im: | 10 | 51 | 47 | Maffiliæ | - - - + 5 | 50 |
| Em. | 12 | 57 | 37 | Maffiliæ | - - - + 7 | 44 |
| 19. Im. | 6 | 6 | 34 | Stockh. tub. max. | - 8 | 51 |
| | 6 | 2 | 30 | Ibid. tub. minore | - 4 | 47 |
| | 4 | 57 | 16 | Paris | - - - - - 2 | 23 |
| Em. | 6 | 56 | 29 | Paris | - - - - + 6 | 32 |

Observationes asterisco * notatæ, minus certas suspicor. Differentiæ inter reliquas tribuendæ videntur differentiis telescopiorum, majori minorive aëris serenitati, et fortassis cuidam incertitudini circa differentias meridianorum.

Observationes Stockholmenses habitæ sunt telescopio novo Dollondiano 10 ped. objecta 120^o secundum diametros amplificante.

XVIII. Remarks on the Censure of Mercator's Chart, in a posthumous Work of Mr. West, of Exeter : In a Letter to Thomas Birch, D. D. Secretary to the Royal Society, from Mr. Samuel Dunn.

Rev. Sir,

Read Nov. 11, 1762. I Should not be so ready to trouble you with the contents of this letter, had I not the highest opinion of your readiness to assist the scientific, in all matters wherein you are able.

I request therefore your consideration, between this time and the next when I have the pleasure to see you, if any paper has been printed in the Philosophical Transactions, concerning a sphere being inscribed in a hollow cylinder, and swelling its surface to the sides of the cylinder, to construct thereby a more true and accurate chart for the purposes of navigation, than that which was invented by Edward Wright, and hath long gone under the name of Mercator.

The reason why I ask this is, because there is lately published, a posthumous work of one Mr. West of Exeter, revised by J. Rowe, in which it is strongly insisted on, that the graduation of Mercator's chart is erroneous, and that the same, if rightly correspondent with the loxodromiques or rhumbs, should be graduated as a line of natural tangents, from the equinoctial to the poles.

Now this error might have past the less observed, but the Critical Review of last month sets it forth as a masterly

masterly performance, and a thing of the greatest merit and importance in navigation.

That there is a respect due to Edward Wright for his invention, that his principles are true, that Mr. West or his editor, and both (if both of the same opinion) are false, is most certain.

That the characters and abilities of Dr. Halley, Sir Jonas Moore, Mr. William Jones, Mr. James Hodgson, Mr. Hafelden, and many others, for they are almost numberless, both of higher and lower mathematicians, who have wrote on the certainty and utility of Wright's chart, I say, that the characters and abilities of these able geometricians are attacked by Mr. West and his editor, and by the Critical Reviewers, is plain, and that this will have great weight with many not over well acquainted with geometry is no less plain. And what will an honest seaman say, who knows but just to make his calculations, when he reads the account given in this book, of Mercator's chart? And what must those gentlemen among the subscribers to Mr. West's book say or think, who, not being quite masters of geometry, are at liberty to believe or disbelieve Dr. Halley and many others, or Mr. West and his editor? Those who are masters of geometry must see the error.

But there are other circumstances; Edward Wright himself gives the very same construction by his words, as Mr. West doth, although his tables make out quite another thing, that is, both Wright and West say expressly, the sphere being inscribed in the hollow cylinder, and the equinoctial remaining fixed without swelling whilst the other parts swell towards the poles, the chart will be formed. But in this, Wright

has badly expressed his own thoughts, for his tables make it that the equinoctial must either fwell or contract itself. And this is very excusable in Edward Wright, for at that time geometricians had no notion of Fluxions, or the increase of magnitude by local motion.

Mr. West and his editor have therefore fallen into this error; they have taken the words but not the sense of Edward Wright, and the Critical Reviewers vindicate them, and make it as though this property had been communicated to the Royal Society by Mr. West, the particulars of which may be seen in the Review just now mentioned.

The proposed demonstration of this tangential property at page 58 of Mr. West's book, is no demonstration at all, there is nothing more plain, than that, in order to have the meridians at equal distances, the degrees of latitude must be enlarged to the same proportion in every part, as the circular meridians are nearer towards the poles, which proportion is as the cosine of the latitude to the radius.

I am,

Rev. Sir,

Your most obedient servant,

Chelsea, Sept. 4, 1762.

Samuel Dunn.

**XIX. A Defence of Mercator's Chart against
the Censure of the late Mr. West of Ex-
eter : In a Letter to Charles Morton,
M. D. Secret. R. S. from Mr. William
Mountaine, F. R. S.**

To Doctor Morton, Secretary to the Royal Society.

Dear Sir,

Read March 17. 1763. I Received your favour with Mr. Samuel Dunn's letter, touching Mr. West's method of constructing a nautical planisphere, referred to me by the Royal Society, which I now beg leave to return with the following account.

As this island is situate by nature, not only for coasting trade, but foreign commerce, so every real improvement in the art of navigation has always met with the approbation and encouragement of the ingenious and sensible part of these kingdoms.

The greatest single advantage that this important business ever received, was from the invention of the mariner's compass; and next to this, the projection of a true nautic practical chart claims place; — this last was performed by that great improver of navigation, Mr. Edward Wright, as appears by his book intitled "certain errors in navigation detected and corrected", published about the year 1599.

In chapter 2d, of said book, he tells us, "that the errors in the plain chart had been complained of.

“ of by divers, as namely by Martin Cortese, Petrus Nonius, and even Gerardus Mercator seemeth to have corrected them, in his Universal Map of the World ; yet none of them had taught any certain way how to amend such gross faults : ” And, in his Preface, he declares, “ that, by occasion of Mercator’s map, he first thought of correcting so many and great absurdities in the common Sea Chart, but the way how this was by him done, he neither learnt of Mercator, nor of any man else.”

Wright’s method (erroneously called Mercator’s) was at this time then adopted, has continued ever since in use, and has been improved by some of the greatest mathematicians who have flourished since that time, and although sometimes attacked, yet it has been found impregnable.

The first person (that I am aware of) who charged Mr. Wright with errors in his tables of rhumbs, is Simon Stevens, in his large volume of mathematical remembrances, which Wright himself plainly confutes in a subsequent edition of his book : now, Stevens does not condemn the principles, but only asserts that his tables have some faults in them, and endeavours to prove that the fourth rhumb at 78 deg. of longitude ought to have 61^d. 26^m. of latitude, whereas Wright makes it only 61^d. 14^m. Hence, the great difference is no more than 12 minutes ; and what inconvenience can arise hereby to the mariner in such a run, was this the fact ? But it turns out otherwise, for this difference is reduced to less than one minute (even according to Stevens own way) as evidently appears from Wright’s answer in page 214.

If

If every rhumb is then found to possess its true latitude in this chart at every degree and minute of longitude, without any sensible or explicable error (to make use of our author's own words) it follows, that the degrees of latitude are duly increased, or that the table of meridional parts are true.

The great Doctor Halley has given us a curious method of dividing the nautical meridian, and of performing the problems in sailing according to the true chart, in Philosophical Transactions, N^o. 219. by a method different from Mr. Wright's, but so nearly corresponding in practice, that this alone is a sufficient testimony in favour of my author.

Our worthy brother Mr. John Robertson, in his excellent Elements of navigation, vol. II. page 358, expresses himself thus: " Now although a table thus made (Wright's table of meridional parts constructed to minutes) be abundantly sufficient for all nautical purposes, yet had the secants of smaller parts than minutes been taken, the table would have been more correct; and therefore Mr. Oughtred, Sir Jonas Moore, Doctor Wallis, Doctor Halley, and others, have been induced to find methods of constructing those tables with more accuracy than by the addition of secants to every minute.

" But a table of meridional parts, constructed by the most accurate method, only shews that Mr. Wright's tables do no where exceed the true meridional parts by half a minute, and this only near the pole; for in latitudes as far as navigation is practicable the difference is scarce sensible".—

About

About the year 1720, a curvilinear sea chart made its appearance, said to be done by Henry Wilson, the publishers whereof represented Wright's chart as puzzling, difficult, and false.

But these groundless assertions were rationally answered by Mr. Thomas Haselden, afterwards master of the Royal Academy at Portsmouth, in a letter and pamphlet addressed to Dr. Halley about the year 1722.

In the year 1755 was published a book intitled, "The art of sailing upon the Sea", by W. E. which initial letters are sufficient to point out the ingenious author.—In page 74 he faies, "It is demonstrable (by the method of fluxions) that the length of the part of the meridian line in Mercator's chart, which represents the difference of latitude of two places upon the globe, is equal to the difference of the log. tangents of half the complements of the two latitudes, multiplied into the number 2,30258509, and that product into the radius of the sphere".

And in Scholium to his Fundamental principles, page 75. "In the few foregoing propositions, I have demonstrated the truth of the chief methods of sailing now in use; and deduced them from their genuine principles, and fixt them upon their proper foundations: By which the reader will be enabled to see that this theory is not founded upon false principles; but upon such as are solid and true; and consequently that all calculations built hereon may be depended on as exact".

Notwithstanding these, Wright's method is charged with great imperfection by the late Mr. West of Exeter, in his posthumous work, referred unto by

Mr. Dunn.—**Mr. West** therein declares that “the errors of the plain chart are corrected, in a great measure, by Mercator’s or Wright’s chart; tho’ the latter is not a true projection of the sphere in any shape; nor indeed is it pretended to be such by Mr. Wright, one of its inventors”. — The first part of this paragraph surely contains a contradiction; for how can the errors in the plain chart be in a great measure corrected by a projection that is not true in any shape? And in answer to the latter part,— **Mr. Wright** has no where made such concessions that I know of;—and further **Mr. West** blends Wright and Mercator together, when at the same time it does not appear that the latter ever published any principles of this kind of projection to the world.

In the 20th article of the beforementioned book, **Mr. West** has laid down a method of constructing a nautical chart, which he asserts to be “the first representation of the terraqueous globe ever yet invented, in which the meridians, parallels and rhumbs, are justly and truly projected in *right lines*, for the latter cannot be *so* projected in Mercator.”—If they cannot be *so* projected in Wright’s, they cannot in *bis*; for in both, the meridians are said to be right lines and parallel, and therefore the rhumbs must be right lines also, or how can they intersect the meridians so situate at equal angles?

He also saies in his scholium, that “It does not appear that Mercator or Wright ever thought of this projection; for the meridian line here is manifestly a line of tangents; whereas in their projection, it is a collection of secants.”

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What Mercator's thoughts were upon this matter when he formed his universal map, I know not, as he has left us no account thereof; but what Wright's were, he has very plainly told us in his aforesaid book; and whether his primary conceptions, and preparative modulus, do not only take in the whole, but also the very manner, of Mr. West's construction, will better appear upon a due comparison of their respective methods, which I shall beg leave here to introduce in their own words.

Mr. WEST'S PROPOSITION.

“ If a rectangular piece of paper be turned into the form of a right cylindrical tube, and a sphere be inscribed therein, so as that the axes of the sphere and cylinder do coincide, or that the equator be the line of contact between the said tube and sphere, and all the points of the spheric surface be projected or transferred to the concave surface of the tube, by right lines proceeding from the center of the sphere, and terminating in the said concave surface of the tube: And then, if the paper be opened, and stretched upon a plane, it will present a chart, in which the meridians, parallels of latitude, and rhumbs are all truly and geometrically projected in right lines”.

In Wright's Correction of errors, reprinted by Moxon in 1657, not having the original edition by me, in Chap. 2. we have the following account —
“ Whereas the spaces betwixt the parallels should increase

" crease more and more as you go from the equinoctial toward either of the poles, which Martin Cor-
 " tese also noteth in his third book and second chap-
 " ter of the art of navigation; But he omitted *that*
 " wherein all the difficulty lyeth; that is, how much,
 " or in what proportion those spaces should increase:
 " Which that it may the better be perceived, I think
 " it not unmeet firt to shew by what kind of pro-
 " jection (or extension rather) the nautical planisphere
 " may not unfitly be conceived to be geometrically
 " made, after this manner.

Mr. WRIGHT's METHOD.

" Suppose a spherical superficies, with meridians,
 " parallels, rhumbs, and the whole hydrographical
 " description drawn thereupon, to be inscribed into
 " a concave cylinder, their axes agreeing in one."

" Let this spherical superficies swell like a bladder
 " (whiles it is in blowing) equally always in every
 " part thereof (that is, as much in longitude as in
 " latitude) till it apply, and join itself (round about,
 " and all along also towards either pole) unto the
 " concave superficies of the cylinder: each parallel upon
 " this spherical superficies, increasing successively from
 " the equinoctial towards either pole, until it come to
 " be of equal diameter with the cylinder, and con-
 " sequently the meridians still widening themselves,
 " till they come to be so far distant every where each
 " from the other, as they are at the equinoctial. Thus
 " it may most easily be understood, how a spherical
 " superficies may (by extension) be made a cylindri-
 " cal, and consequently a plane parallelogram super-
 " ficies;

“ ficies ; because the superficies of a cylinder is nothing else but a plain parallelogram wound about two equal equidistant circles that have one common axtree perpendicular upon the centers of them both” &c — “ So as the nautical planisphere may be defined to be nothing else but a parallelogram made of the spherical superficies of an hydrographical globe inscribed into a concave cylinder, both their axes concurring in one ; and the spherical superficies swelling in every part equally in longitude and latitude, till every one of the parallels thereupon be inscribed into the cylinder (each parallel growing as great as the equinoctial) or till the whole spherical superficies touch and apply itself every where to the concavity of the cylinder”. —

“ In this nautical planisphere thus conceived to be made, all places must needs be situate in the same longitudes, latitudes, and directions or courses, and upon the same meridians, parallels, and rhumbs, that they were in the globe ; because, that at every point between the equinoctial and the pole, we understand the spherical superficies whereof this planisphere is conceived to be made, to swell equally as much in longitude as in latitude (till it join itself unto the concavity of the cylinder) so as hereby no part thereof is any way distorted or displaced out of his true and natural situation upon his meridian, parallel or rhumb, but only dilated and enlarged : the meridians also, parallels and rhumbs dilating and enlarging themselves likewise, at every point of latitude in the same proportion”.

By comparing these two modes of construction together I think it is not very difficult to discover that Mr. West's derives its original from Wright's ; for right lines

lines drawn from the center thro' all the points in the spheric surface, and terminating in the concave surface of the tube, are secants, and the tube becomes a tangent line to all those respective secants: And, does not Wright's uniform dilatation, by the second law of motion, produce the same?

West stops here, and gives us a chart at once; Wright calls these his geometrical lineaments only, by which he obtains a rectilinear planisphere, and from whence he demonstrates the principles upon which his table of meridional parts are founded.

And that he does not esteem this as a chart completed, but only his apparatus, and preparative work, which requires yet to be applied and moulded into a true nautical chart, is evident from the next paragraph,
 " Now then (saies he) let us diligently consider of the
 " geometrical lineaments, that is, the meridians, thumbs,
 " and parallels of this *imaginary* nautical planisphere,
 " that we may in like manner express the same in
 " the mariner's chart: For so undoubtedly we shall
 " have therein a true hydrographical description of
 " all places in their longitudes, latitudes, and directions,
 " or respective situations each from other according
 " to the points of the compass in all things corre-
 " pondent to the globe, without either sensible, or
 " explicable error".

And hence he proceeds to the proof and application of these his lineaments, to the construction of his table of latitudes, as he calls it; which is, in this edition, computed to minutes of parallel-distance, but with a little contrivance in the calculus to reduce the same yet somewhat nearer the truth.

Notwithstanding this care and nicety in computation, he is duly sensible that his increments of latitude calculated

calculated to minutes, altho' without any sensible error, are yet not absolutely true, because they ought to flow with an uniform, and uninterrupted motion; He therefore cautiously guards against critical remarks hereupon, in the following paragraph :

" In this table it was thought sufficient to use such exactness, as that thereby (in drawing the lineaments of the nautical planisphere) sensible error might be avoided. He that listeth to be more precise may make the like table to decades, or tens of seconds, out of Joachimus Rheticus his Canon Magnus Triangulorum: notwithstanding the geometrician that desireth exact truth, cannot be so satisfied neither; for whose sake and further satisfaction, I thought good to adjoin also this geometrical conceit of dividing a meridian of the nautical planisphere."

" Let the equinoctial and a meridian be drawn upon a globe: Let the meridian, divided into degrees, minutes, seconds, &c. roul upon a streight line, beginning at the equinoctial, the globe swell-
ing in the mean time in such sort, that the semidi-
ameter thereof may be always equal to the secant
of the angle, or arch contained between the equi-
noctial and semidiameter insliting at right angles
upon the foresaid streight line: The degrees, mi-
nutes, seconds, &c. of the meridian, noted in
the streight line, as they come to touch the same,
are the divisions of the meridian in the nautical
planisphere: And this conceit of dividing the meri-
dian of the nautical planisphere may satisfy the curi-
ous exactness of the geometrician; but for mecha-
nical use, the table before mentioned may suffice."

More might be said in favour of Wright's chart, but I think it is altogether unnecessary; if his own

principles and documents be duly considered; if almost general practice and experience for a century and an half past can have any weight; and, if the concurrent testimony and authority of so many eminent mathematicians who have handled this subject since his time, and some of them in a quite different method, can have any poise in the scale of reason.

I have carefully endeavoured not to mistake the true sense and meaning of Mr. West's proposition in any part thereof; if I have not, I cannot pronounce what kind of chart may be formed from his tangent line being made the line of latitudes, or that meridian line whereupon the tangents are to determine the sections of their respective parallels: I shall only observe; that, if the meridians be right lines, and parallel to each other, the rhumbs must be right lines also; but by this tangential projection, *these* will be deflected from their true bearings, or make the angles of the courses too great, unless some expedient be devised to accommodate this error; and if the rhumbs be not right lines, such chart will then be embarrassed with more difficulties in practice than Mr. Wright's.

Upon the whole, it seems as if the editor, confiding in Mr. West's abilities, hath, without examination, published this proposition (found amongst many other loose papers, none of which were, perhaps, ever intended for public inspection, as himself saies in his apology) just as he found it; and that the Reviewers in good opinion of both, and out of tenderness to the widow and family, the book being published for their benefit, have not so critically examined and compared it with what has been already done.—But, notwithstanding what is spoken in favour thereof, I suspect it will have little weight with the mariners, who very well know the value

value of the Mercator's chart (as they call it) nor are they ever very easily induced to adopt new notions or inventions, and those contrary to what they are familiarized unto by constant practice.

The Critical Reviewers do indeed hint as if this paper had been heretofore communicated by Mr. West to the Royal Society, and that in the following terms: — “Mr. West lays down the following very ingenious proposition, which, if we do not greatly mistake, we have seen, with little variation, in the Philosophical Transactions, communicated possibly by the same hand”. — In this, I believe, they are mistaken, for I cannot find any thing like it in the transactions since the date 1746, the year in which it is said to be wrote.

I am duly sensible of the frequent monitions, and sincere desire of the Royal Society, that its members may avoid all possible occasions of controversy; and whether this account has not a tendency thereunto, if it should, in other respects, be thought worthy of a place in the public Transactions, is submitted, with all due respect, to the determination of the Committee of papers.

I have the honour to be, Sir,

With the greatest esteem,

The Royal Society's,

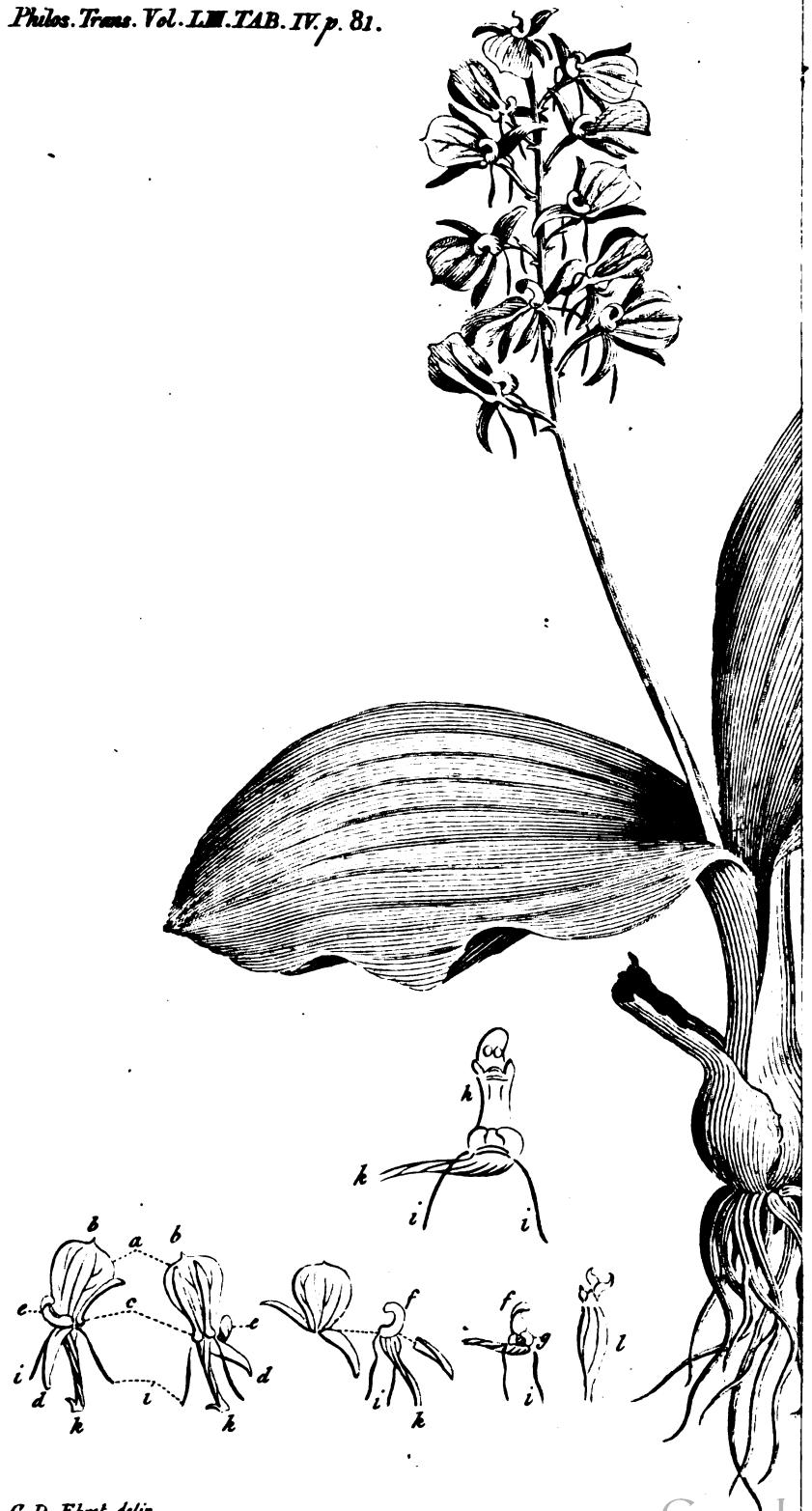
Gainsford - Street,
Southwark; Jan.
26th, 1763.

and also

your most obedient, and
faithful humble servant,

William Mountaine.

XXI. An



XXI. *An Account of a Species of Ophris,
supposed to be the Plant, which is mentioned
by Gronovius in the Flora Virginica, p. 185,
under the Name of Ophris Scapo nudo foliis
radicalibus ovato-oblongis, dimidii Scapi lon-
gitudine: By George Dionysius Ehret,
F. R. S.*

Read April 14, 1762. **T**HE root of this plant, from
which many fleshy fibres branch, is composed of the foot stalks of the leaves, which envelope each other in such a manner, that they form a kind of bulbous root. From the said bulb proceed two oval-shaped, nervous, smooth leaves, having membranous convolute petioli or footstalks. These encompass a triquetrous scapus, or a single stalk arising from the centre of this root, which produces many flowers of a singular construction. These flowers are supported by small pedunculi, or flower stalks, of a bloody-red colour, which swell into seed-vessels, having at their base an acute denticle.

This very singular plant blew (for the first time in England) in the Year 1758, in the curious exotic garden of Mr. Peter Collinson; who received it from Mr. Bertram of Philadelphia.

Mr. Clayton has described a plant, in the Flora Virginica page 185, under the name of "Bifolium Scapo e medio duorum foliorum nudo, aphylo, ad exortum tenui, paulatim versus apicem accrescente, sex

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vel septem capsulas sustinente: radice fibroſa carnosa viridi, foliis obvolutâ, humi jacente; fibras paucas emittente, cui radix anni superioris contigua et integumentis marcidis evoluta pellucida adhæret."

This description seemingly corresponds with the present plant; but yet Mr. Clayton's character of the several parts of the flower is very different from those, which I have observed, and represented in this drawing; and although it may be thought to come near to an epidendrum, yet it is neither an epidendrum nor a bifolium, as the following description of the characters will sufficiently indicate.

This plant, however, should be ranged amongst the first order of Dr. Linnæus's clas of *guinandria diandria*, which consists of several genera.

The description of the characters.

TAB. III. Fig. *a*, represents a front and side view of the flower, which has but one broad, flat, oval-shaped petal, or leaf, of a pale red colour, marked with three veins. It has also a small point or denticle projecting at the top of the limbus or margin, Fig. *b*. At the base of this broad petal is situated an irregular unequal-divided triphyllus periantheum, consisting of three narrow separate leaves, of a pale-green colour, and almost of equal length with the petalum. Two of these leaves are erect, and both gibbous at their insertion as in Fig. *c*, and placed at the back of this petal. It would seem as if these two leaves were supporters of the corolla: the third leaf is fixed at the opposite side fronting the flower, opening and bending downwards, see Fig. *d*.

In

In the centre of this flower is situated a leafy style, of a convex figure, projecting outwards, and facing the corolla as in Fig. *e*, the top of which is a membranous foliaceous stigma, which reflects downwards to protect the male sperm, and forms (as it were) a caliptra or lap, under which are inserted two yellow globular apices (containing the farina) without filaments, which are the stamina of the flower.

Fig. *f.* explains a front and side view of the style by itself, (the petal and calyx are separated from these figures:) the base of it is a gibbous fleshy substance, which shews apparently two bodies of nectaria, of a crimson-red colour, see Fig. *g.*

The magnified figure above Fig. *b*, represents the inside, and describes the parts more distinctly; whose leafy stigma is laid open, to expose to view the insertion of the two apices: beneath these globules appears a cross line, on which is placed, on each side, a small pointed leaf. From the corners of this fleshy nectarium thus magnified, come forth two threads, Fig. *i*, hanging downwards: they are of equal length with the peduncle, of a skinny substance, and of a bloody-red colour.

The german, which is twisted like a screw, represents the footstalk, which supports the flower, Fig. *k*: this swells into an oblong, oval striated unilocular seedvessel, Fig. *l*, which contains an innumerable number of dust-like seeds, as the open longitudinal section Fig. *m*, represents.

**XXII. New Experiments in Electricity: In
a Letter from Mr. Ebenezer Kinnerley,
to Benjamin Franklin, LL. D. F. R. S.**

Read Nov. 18, 1762, March 24, and April 14, 1763.

S I R,

Philadelphia, Mar. 12, 1761.

HA VING lately made the following experiments, I very chearfully communicate them, in hopes of giving you some degree of pleasure, and exciting you to further explore your favourite, but not quite exhausted, subject, ELECTRICITY.

E X P. I.

I placed myself on an electric stand, and, being well electrised, threw my hat to an unelectrised person, at a considerable distance, on another stand, and found, that the hat carried some of the electricity with it; for, upon going immediately to the person, who received it, and holding a flaxen thread near him, I perceived he was electrised sufficiently to attract the thread.

E X P. II.

I then suspended, by silk, a broad plate of metal, and electrised some boiling water under it, at about four feet distance, expecting that the vapour, which ascended plentifully to the plate, would, upon the principle of the foregoing experiment, carry up some of the electricity with it; but was at length fully convinced, by several repeated trials, that it left all its share

share thereof behind. This I know not how to account for ; but does it not seem to corroborate your hypothesis, that the vapors, of which the clouds are formed, leave their share of electricity behind in the common stock, and ascend in a negative state ?

E X P. III.

I put boiling water into a coated Florence flask, and found that the heat so enlarged the pores of the glass, that it could not be charged. The electricity passed thro' as readily, to all appearance, as thro' metal ; the charge of a three-pint bottle went freely thro' without injuring the flask in the least. When it became almost cold, I could charge it as usual. Would not this experiment convince the Abbé Nollet of his egregious mistake ? For, while the electricity went fairly thro' the glass, as he contends it always does, the glass could not be charged at all.

E X P. IV.

I took a slender piece of cedar, about eighteen inches long, fixed a brass cap in the middle, thrust a pin, horizontally and at right angles, thro' each end, (the points in contrary directions) and hung it, nicely balanced like the needle of a compass, on a pin, about six inches long, fixed in the center of an electric stand. Then electrising the stand, I had the pleasure of seeing what I expected ; the wooden needle turned round, carrying the pins with their heads foremost. I then electrised the stand negatively, expecting the needle to turn the contrary way ; but was extremely disappointed, for it went still the same way as before.

When the stand was electrified positively, I suppose, that the natural quantity of electricity in the air being increased on one side, by what issued from the points, the needle was attracted by the lesser quantity on the other side. When electrified negatively, I suppose, that the natural quantity of electricity in the air was diminished near the points; in consequence whereof, the equilibrium being destroyed, the needle was attracted by the greater quantity on the opposite side.

The doctrine of repulsion in electrified bodies, I begin to be somewhat doubtful of. I think all the phenomena, on which it is founded, may be well enough accounted for without it. Will not cork balls, electrified negatively, separate as far as when electrified positively? And may not their separation, in both cases, be accounted for upon the same principle; namely, the mutual attraction of the natural quantity in the air, and that which is denser, or rarer in the cork ball? It being one of the established laws of this fluid, that quantities of different densities shall mutually attract each other, in order to restore the equilibrium.

I can see no reason to conclude, that the air has not its share of the common stock of electricity as well as glass, and, perhaps, all other electrics per se. For tho' the air will admit bodies to be electrified in it either positively or negatively, and will not readily carry off the redundancy in the one case, or supply the deficiency in the other;

E X P.

E X P. V.

Yet let a person in the negative state, out of doors in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards; and he will soon be convinced, that electricity may be drawn out of the air; not very plentifully, for, being a bad conductor, it seems loth to part with it; but yet some will evidently be collected. The air near the person's body, having less than its natural quantity, will have none to spare; but, his arm being extended as above, some will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

Let a person electrified negatively present the point of a needle, horizontally, to a cork ball suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity as to be in the negative state, in the same degree with the person who holds the needle: then it will recede from the point; being, as I suppose, attracted the contrary way by the electricity of greater density in the air behind it. But, as this opinion seems to deviate from electrical orthodoxy, I should be glad to see these phenomena better accounted for by your superior and more penetrating genius.

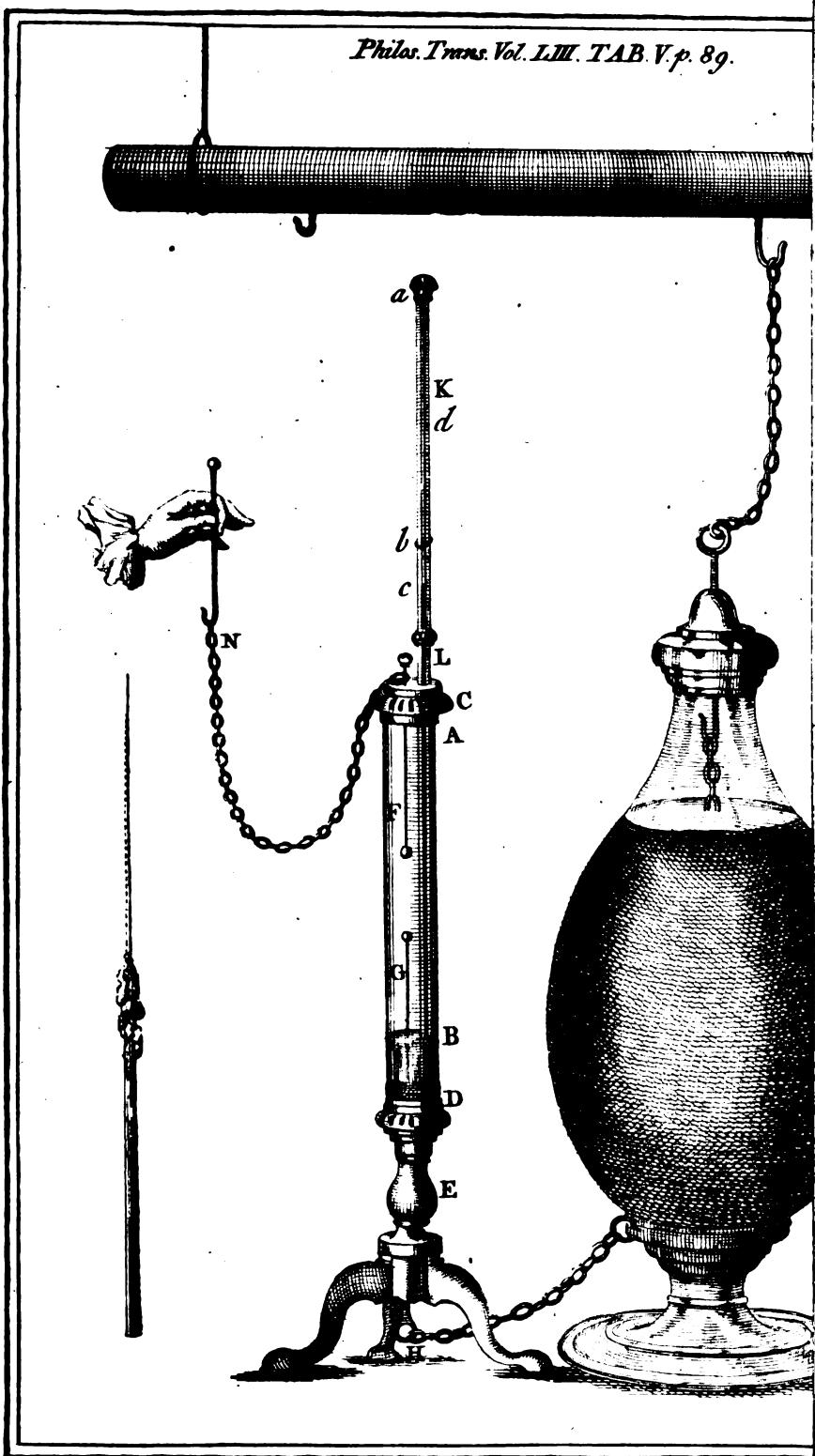
Whether the electricity in the air, in clear dry weather, be of the same density at the height of two or three hundred yards, as near the surface of the earth, may be satisfactorily determined by your old experiment of the kite.

E X P.

E X P. VI.

The twine should have, through-out, a very small wire in it; and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread to prevent their acting in the manner of points. I have tried the experiment twice, when the air was as dry as we ever have it, and so clear that not a cloud could be seen; and found the twine each time, in a small degree electrified positively. The kite had three metalline points fixed to it; one on the top, and one on each side. That the twine was electrified, appeared by the separating of two small cork balls suspended on the twine by fine flaxen threads, just above where the silk was tied to it, and sheltered from the wind. That the twine was electrified positively, was proved by applying to it the wire of a charged bottle; which caused the balls to separate further, without first coming nearer together. This experiment shewed, that the electricity in the air, at those times, was denser above than below. But that cannot be always the case; for you know we have frequently found the thunder clouds in the negative state, attracting electricity from the earth. Which state it is probable they are always in when first formed, and till they have received a sufficient supply. How they come afterwards, towards the latter end of the gust, to be in the positive state, which is sometimes the case, is a subject for further enquiry.

After the above experiments with the wooden needle, I formed a cross of two pieces of wood, of equal



equal length, intersecting each other at right angles in the middle; hung it, horizontally, on a central pin, and set a light horse, with his rider, upon each extremity; whereupon, the whole being nicely balanced, and each courser urged on by an electrified point, instead of a pair of spurs, I was entertained with an electrical horse-race.

I have contrived an electrical air thermometer, and made several experiments with it, that have afforded me much satisfaction and pleasure. It is extremely sensible of any alteration in the state of the included air, and fully determines that controverted point, whether there be any heat in the electric fire. By the Plate [TAB. IV.] and the following description, you will readily apprehend the construction of it.

A. B. is a glass tube about eleven inches long, and one inch diameter in the bore. It has a brass ferril cemented on each end, with a top and bottom part, C and D to be screwed on, air-tight, and taken off at pleasure. In the center of the bottom part D, is a male screw, which goes into a brass nut in the mahogany pedestal E. The wires F and G are for the electric fire to pass through, darting from one to the other. The wire G extends through the pedestal to H; and may be raised or lowered by means of a male screw on it. The wire F may be taken out, and the hook I be screwed into the place of it. K is a glass tube with a small bore, open at both ends, cemented in the brass tube L, which screws into the top part C. The lower end of the tube K is immersed in water, coloured with cocheneal, at the bottom of the tube A B. (I used at first coloured spirits of wine; but, in one of the experiments I made, it took fire.)

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On the top of the tube K is cemented, for ornament, a brass feril, with a head screwed on it, which has a small air hole thro' its side at α . The wire b is a small round spring, that embraces the tube K so as to stay wherever it is placed. The weight M is to keep strait whatever may be suspended in the tube A B. on the hook I. Air must be blown thro' the tube K into the tube A B, 'till enough is intruded to raise, by its elastic force, a column of the coloured water, in the tube K up to c, or thereabouts; and then, the gage wire b being slipt down to the top of the column, the thermometer is ready for use.

E X P. VII.

I set the thermometer on an electric stand, with the chain N fixed to the prime conductor, and kept it well electrified a considerable time; but this produced no sensible effect. Which shews, that the electric fire, when in a state of rest, has no more heat than the air and other matter wherein it resides.

E X P. VIII.

When the wires F and G are in contact, a large charge of electricity sent thro' them, even that of my case of five and thirty bottles, containing above thirty square feet of coated glafs, will produce no rarefaction of the air included in the tube A B. Which shews, that the wires are not heated by the fires passing thro' them.

E X P. IX.

When the wires are about two inches apart, the charge of a three pint bottle, darting from one to the

the other, rarefies the air very evidently. Which shews, I think, that the electric fire must produce heat in itself, as well as in the air, by its rapid motion.

The charge of one of my glass jars, which will contain about five gallons and a half, wine measure, darting from wire to wire, will, by the disturbance it gives the air in the explosion repelling it in all directions, raise the column in the tube K up to d , or thereabouts; and the charge of the above-mentioned case of bottles will raise it to the top of the tube. Upon the air's coalescing, the column, by its gravity, instantly subsides till it is in equilibrio with the rarefied air; it then gradually descends, as the air cools, and settles where it stood before. By carefully observing at what height above the gage-wire the descending column first stops, the degree of rarefaction is discovered; which, in great explosions, is very considerable.

E X P. X.

I hung in the thermometer, upon the hook I, successively, a strip of wet writing paper, a wet flaxen and woolen thread, a blade of green grass, a filament of green wood, a fine silver thread, a very small brass wire, and a strip of gilt paper; and found that the charge of the glass jar, passing thro' each of these, especially the last, produced heat enough to rarify the air very perceptibly. The charge of the case of bottles sent thro' the brass wire consumed great part of it into smoke. The thermometer appeared quite opaque with it.

O 2

E X P.

E X P. XI.

I then suspended, out of the thermometer, a piece of bras's wire, not quite so small as the former, about twenty four inches long, with a pound weight at the lower end; and, by sending the charge of the case of bottles thro' it, discovered a new method of wire-drawing. The wire was red hot, the whole length well annealed, and above an inch longer than before. A second charge melted it; it parted near the middle, and measured, when the ends were put together, four inches longer than at first. This experiment I remember you proposed to me, as worth trying, before you left Philadelphia; in order to find, whether the electricity, in passing thro' the wire, would so relax the cohesion of its constituent particles, as that the weight might produce a separation; but neither of us had the least suspicion, that any heat would be produced.

E X P. XII.

That I might have no doubt of the wire's being *hot* as well as red, I repeated the experiment on another piece of the same wire, encompassed with a goose-quill filled with loose grains of gun-powder; which took fire as readily, as if it had been touched with a red hot poker. Also tinder, tied to another piece of the wire, kindled by it. I tried a wire about twice as big, but could produce no such effects with that.

Hence it appears, that the electric fire, tho' it has no sensible heat when in a state of rest, will, by its violent

violent motion, and the resistance it meets with, produce heat in other bodies, when passing thro' them, provided they be small enough. A large quantity will pass thro' a large wire without producing any sensible heat; when the same quantity passing thro' a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with greater resistance, will make it red hot, and even melt it.

Hence lightning does not melt metal by a cold fusion, as we formerly supposed. But when it passes thro' the blade of a sword, if the quantity be not very great, it may heat the point so as to melt it, while the broadest and thickest part may not be sensibly warmer than before.

And, when trees or houses are set on fire by the dreadful quantity, which a cloud, or the earth sometimes discharges, must not the heat, by which the wood is first kindled, be generated by the lightning's violent motion thro' the resisting combustible matter?

If lightning, by its rapid motion, produces heat in itself as well as in other bodies, (and that it does, I think, is evident from some of the foregoing experiments made with the thermometer) then its sometimes singeing the hair of animals killed by it may easily be accounted for. And the reason of its not always doing so may, perhaps, be this: the quantity, tho' sufficient to kill a large animal, may, sometimes, not be great enough, or not have met with resistance enough, to become by its motion burning hot.

We find, that dwelling houses, struck with lightning, are seldom set on fire by it; but when it passes thro'

thro' barns with hay or straw in them, or store-houses containing large quantities of hemp, or such like matter, they seldom, if ever, escape a conflagration. Which may, perhaps, be owing to such combustibles being apt to kindle with less degree of heat than is necessary to kindle wood.

We had four houses in this city, and a vessel at one of the wharfs, struck, and damaged, by lightning last summer. One of the houses was struck twice in the same storm. But I have the pleasure to inform you, that your method of preventing such terrible disasters, has, by a fact, which had like to have escaped our knowledge, given a very convincing proof of its great utility, and is now in higher repute with us than ever.

Hearing, a few days ago, that Mr. William West, merchant in this city, suspected, that the lightning, in one of the thunder-storms last summer, had passed through the iron conductor, which he had provided for the security of his house, I waited on him, to enquire what ground he might have for such suspicion. Mr. West informed me, that his family and neighbours were all stunned with a very terrible explosion, and that the flash and crack were seen and heard at the same instant. Whence he concluded, that the lightning must have been very near; and, as no house in the neighbourhood had suffered by it, that it must have passed through his conductor. Mr. White, his clerk, told me, that he was sitting at the time by a window, about two feet from the conductor, leaning against the brick wall, with which it was in contact; and that he felt a smart sensation, like an electric shock, in that part of his body, which touched the wall.

Mr.

Mr. West further informed me, that a person of undoubted veracity assured him, that, being in the door of an opposite house on the other side of Water-Street (which you know is but narrow) he saw the lightning diffused over the pavement, which was then very wet with rain, to the distance of two or three yards from the foot of the conductor. And that another person of very good credit told him, that he, being a few doors off, on the other side of the street, saw the lightning above, darting in such direction, that it appeared to him to be directly over that pointed rod.

Upon receiving this information, and being desirous of further satisfaction, there being no traces of the lightning to be discovered in the conductor, as far as we could examine it below, I proposed to Mr. West our going to the top of the house to examine the pointed rod; assuring him, that, if the lightning had passed thro' it, the point must have been melted; and, to our great satisfaction, we found it so. This iron rod extended in height about nine feet and a half above a stack of chimnies, to which it was fixed; (but I suppose, three or four feet would have been sufficient). It was somewhat more than half an inch diameter, in the thickest part, and tapering to the upper end. The conductor, from the lower end of it to the earth, consisted of square iron nail rods, not much above a quarter of an inch thick, connected together by interlinking joints. It extended down the cedar roof to the eaves, and from thence down the wall of the house, four story and a half, to the pavement in Water-Street; being fastened to the wall, in several places, by small iron hooks. The lower end was fixed,

fixed to a ring in the top of an iron stake, that was driven about four or five feet into the ground. The above mentioned iron rod had a hole in the top of it, about two inches deep, wherein was inserted a bras wire, about two lines thick, and, when first put there, about ten inches long, terminating in a very acute point; but now its whole length was no more than seven inches and a half, and the top very blunt. Some of the metal appears to be missing; the slenderest part of the wire being, as I suspect, consumed into smoke. But some of it, where the wire was a little thicker, being only melted by the lightning, sunk down, while in a fluid state, and formed a rough irregular cap, lower on one side than the other, round the upper end of what remained, and became intimately united therewith.

This was all the damage, that Mr. West sustained by a terrible stroke of lightning. A most convincing proof of the great utility of this method of preventing its dreadful effects. Surely it will now be thought as expedient to provide conductors for the lightning as for the rain.

Mr. West was so good as to make me a present of the melted wire; which I keep as a great curiosity, and long for the pleasure of shewing it to you. In the mean time, I beg your acceptance of the best representation I can give of it; which you will find by the side of the thermometer, drawn in its full dimensions as it now appears. The dotted lines above are intended to shew the form of the wire before the lightning melted it.

And now, Sir, I most heartily congratulate you on the pleasure you must have in finding your great and well

well-grounded expectations so far fulfilled. May this method of security from the destructive violence of one of the most awful powers of nature meet with such further success, as to induce every good and grateful heart to bless God for the important discovery. May the benefit thereof be diffused over the whole globe. May it extend to the latest posterity of mankind; and make the name of FRANKLIN, like that of NEWTON, *immortal.*

I am, Sir, with sincere respect,
 your most obedient, and
 most humble servant,
 Ebenezer Kinnersley.

XXIII. Observations in Electricity and on a Thunder-storm: In a Letter from Mr. Torbern Bergman, to Mr. Benjamin Wilson; F. R. S. Acad. Reg. Upsal. Soc.

Amplissime atque Celeberrime Domine,

Read April 14, 1763. **I**N epistolis recentissimis, quibus me honorasti, experimenta domini Delaval circa electricitatem crystalli Islandicæ commemoras. Pluries hæcce tentamina iteravi, sed constanter eventu prorsus contrario. Scilicet in hunc finem varia hujus crystalli frusta frigori 12 graduum exposui, **VOL. LIII.** **P** in

in thermometro Suecano, mercurio vivo impleto, frigus numero infra punctum congelationis aquæ, quod in nive colliquescente determinatur. (Quivis gradus est pars centesima distantia punctorum congelationis et ebullitionis aquæ). Fricui dein post quarundam horarum spatium, sed nonnisi valde debilem electricitatem elicere potui. Hæc itaque iterum reposui, nondum satis frigefacta putans, et mane sequenti, dum interea mercurius in thermometro quosdam gradus descenderat, in camera non calefacta tentavi, sed adhuc minori successu. Unum igitur frustulum calefeci sperans hoc ipso omnem vim eradicari; sed inopinato non modo non destructam inveni dispositionem electricam, sed valde auctam. Idem mox repetii cum reliquis omnibus, eodemque semper effectu prima tentamina facta sunt cum crystallis objecta duplicantibus, quæ in Suecia erant collectæ: suspicio igitur mihi incident has inter et revera Islandicas essentialiem esse differentiam; itaque comparavi in Islandia natas, sed eadem monstrarunt phænomena *.

Ex hisce tentaminibus cum domini Delaval collatis sequi videtur, diversas hujus crystalli dari species, quæ eo diligentius examinandæ, cum hucusque mineralogi non nisi unicam distinguant.

* Mr. Delaval had already observed, in the letter here referred to by Mr. Bergman, that the property, in Iceland Crystal, of losing its electricity by a moderate heat, was not common to all kinds of it: and mentions in particular a piece of crystal, one part whereof, when heated gently, becomes non electric; while the other part, with the same heat, (or even with a much greater one) remains perfectly an electric. Vide Vol. LII. Part i. p. 355. I have seen the experiments several times which Mr. Delaval mentions, and they always succeeded as he has related them.

B. Wilson,

Memo-

Memorabilem fulminis iustum arcem regiam Upsalensem d. 24 Aug. 1760 ferientem, paucis describere forte non displicebit. Hoc die cœlum, præcedentibus serenum, nubibus atris obducebatur, e quibus pluvia per noctem decidebat. Post medium vero noctem tonitrua audiebantur, quæ, duabus interjectis horis, horrendis fulgurationibus et fragoribus arcem adgrediebantur. Hæc arx insignem et natura et arte habet altitudinem, tectumque e laminis ferreis. Alæ australis paries transversus occidentalis et ipsius arcis murus primarius occidentalis, in contignatione præcipue infima, injuriis violentissimi hujus meteori perturbabantur, unone, an pluribus fulminibus sepe brevi insequentibus, non dicam. Murus sex diversis locis externe Iæsus est, et multis variisque intra arcem cameris, atris vestibulisque percursis, per murum primarium orientalem, urbem spectantem, fulmen exivisse videtur, nam foramen magnum fecerat ibidem. Ipsum ordinem hujus iustus nemo novit: effectus igitur præcipuos tantummodo colligam.

1° Ipse murus foraminibus, rimis, calceaque lapides connectente fracta vel dissipata, violatus est. Lapidès magni cæsi, et adhuc liberi jacentes, diffracti sunt.

2° Ligna sunt tosta, imprimis in confinio ferri, sæpe diffracta, sæpe quoque illæsa hæc fortis electricitas transivit.

3° Sedecim quadrata vitrea unius fenestræ, absque ullo remanente vestigio vel frustulo in confinio, evanescebant. Speculi magni anguli duo oppositi liquefacti erant, foliumque ibidem turbatum, vitrum præterea illæsum, sed macula horizontalis duos pollices longa prope juncturam vitrorum in superiore speculi parte, omni folio carebat.

4° Clavi liquefacti. Picturarum et speculorum margines inaurati quasi tosti. In pavimento inauraturæ frustula conspiciebantur. Cymbalum clausum, quod sub una e picturis læsis confiterat, nullum læsis signum externe monstrabat, nihilominus 15 fides e calybe abscissæ erant, et circa terminos candefactas fuisse signa aderant; in fundo chorda, tres lineas longa, inveniebatur, quam ignitam fuisse lignum tangens tostum indicabat.

5° Parietes imprimis læsi, adeoque directio ictus plerumque obliqua fuit, fluxui quoque aëris obtemperare visa.

6° Odor gravissimus alliaceo-sulphureus omnia implebat loca, sequentique die supellectilia et pavimenta floribus sulphuris adspersa inveniebantur.

7° Nullum animal læsum.

Aestate 1759 fulmen per caminum in parvam hujus urbis domum intrans per quadratum vitreum fenestræ perforatum mox horrendo fragore exivit. Sed præcipue memoratu dignum viros nonnullos, ab hoc loco ultra 250 pedes distantes, in terram decidisse, gravemque concussionem electricam fuisse expertos, dum interea hisce circumstantes nullo modo adficiebantur. Hoc eodem redit, ac dum interdum singuli in circulo explosionis constituti valde inæqualem concussionis gradum sentiunt. Permaneo

Celeberrimi nominis tui

Cultor observantissimus,

Dabam Upsalæ,
J. xx Apr. 1762.

Torbern Bergman.

XXIV. *Remarks*

XXIV. Remarks on Swallows on the Rhine:
In a Letter from Mr. Achard, in Privy-Garden, to Mr. Peter Collinson, F. R. S.

SIR,

Sept. 7, 1762.

Read April 21,
^{1763.} IN the latter end of March I took my passage down the Rhine to Rotterdam: a little below Basil the south bank of the river was very high and steep, of a sandy soil, sixty or eighty feet above the water.

I was surprized at seeing near the top of the cliff some boys tied with ropes hanging down doing something: the singularity of these adventurous boys, and the busineis they so daringly attempted, made us stop our navigation to inquire into the meaning of it. The watermen told us they were searching the holes in the cliff for swallows, or martins, which took refuge in them, and lodged there all the winter, until warm weather, and then they came abroad again.

The boys, being let down by their comrades to the holes, put in a long rammer with a screw at the end, as is used to unload guns, and, twisting it about, drew out the birds. For a trifle I procured some of them. When I first had them, they seemed stiff and lifeless. I put one in my bosom, between my skin and shirt, and laid another on a board, the sun shining full and warm upon it. One or two of my companions did the like.

That in my bosom revived in about $\frac{1}{2}$ of an hour; feeling it move, I took it out to look at it, and saw it

it stretch itself on my hand, but perceiving it not sufficiently come to itself, I put it in again: in about another quarter, feeling it flutter pretty briskly, I took it out and admired it. Being now perfectly recovered, before I was aware, it took its flight, the covering of the boat prevented me from seeing where it went: the bird on the board, though exposed to a full sun, yet, I presume from a chillyness in the air, did not revive to be able to fly.

Remarks by Mr. Collinson.

What I collect from this gentleman's relation is, that it was the practice of the boys, annually to take these birds, by their apparatus and ready method of doing it; and the frequency of it was no remarkable thing to the watermen. Next it confirmed my former sentiments, that some of this *Swallow-tribe* go away, and some stay behind in these dormitories all the winter. If my friend had been particular as to the species, it would have settled that point.

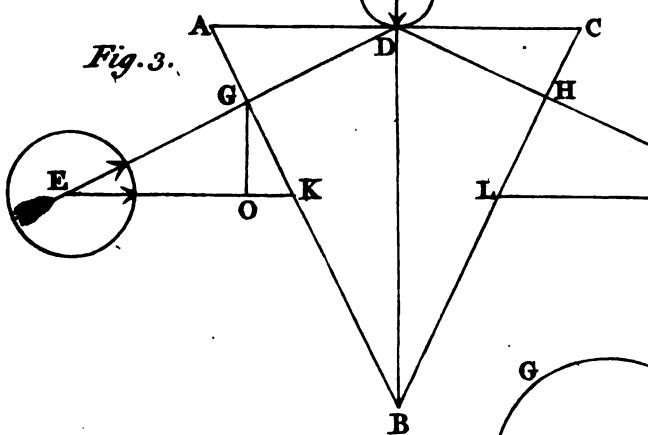
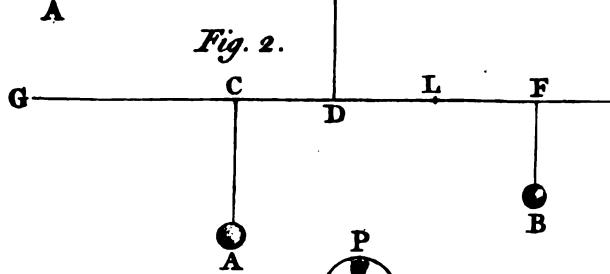
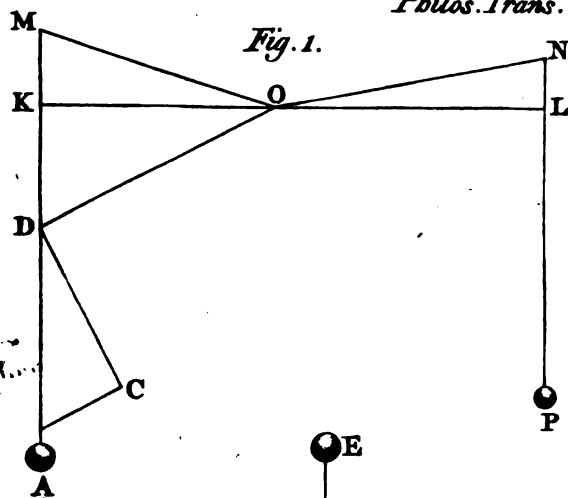
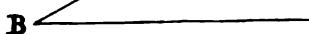


Fig. 4.



XXV. *The Properties of the mechanic Powers demonstrated, with some Observations on the Methods that have been commonly used for that Purpose: in a Letter fr. ; Hugh Hamilton, D. D. F.R.S. and Fellow of Trinity College, Dublin, to Matthew Raper, Esq; F. R. S.*

S I R, Trinity College, Dublin, 13 July, 1762.

Read April 21, and 28.
1763. I Have here ventured to send you some remarks on the methods that have been commonly used in treating of those engines that are called the mechanic powers: and also an account of the principles on which, I think, we may best explain their nature and manner of acting.

The many useful instruments that have been so ingeniously invented, and so successfully executed, and the great perfection to which the mechanic arts are now arrived, would naturally incline one to think that the true principles on which the efficacy and operations of the several machines depend, must long since have been accurately explained. But this is by no means a necessary inference: for, however men may differ in their opinions about the true method of accounting for the effects of the several machines, yet the practical principles of mechanics are so perfectly known by experience and observation, that the artist is thereby enabled to contrive and adjust the movements of

of his engines with as much certainty and success as he could do, was he thoroughly acquainted with the laws of motion, from which these principles may be ultimately derived. However, tho' an enquiry into the true method of deducing the practical principles of mechanics from the laws of motion, should not perhaps contribute much to promote the progress of the mechanic arts, yet it is an enquiry in itself useful, and in some measure necessary: for, since late authors have used very different methods of treating this subject, it may be supposed that no one method has been looked upon as satisfactory and unexceptionable. I should therefore wish to contribute towards having this subject treated with more accuracy than has been hitherto done.

The most general and remarkable theorem in mechanics certainly is this, " That when two weights, by " means of a machine counterpoise each other, and " are then made to move together, their quantities of " motion will be equal". Now an æquilibrium always accompanying this equality of motions, bears such a resemblance to the case wherein two moving bodies stop each other, when they meet together with equal quantities of motion; that Doctor Wallis, and after him most of the late writers, have thought the cause of an æquilibrium in the several machines, might be immediately assigned: by saying, That, since one body cannot produce in another a quantity of motion equal to its own, without losing its own at the same time; two heavy bodies, counteracting each other by means of a machine must continue at rest, when they are so circumstanced that one cannot descend, without causing the other to ascend, at the same time; and with the same

same quantity of motion; and therefore two heavy bodies in such cases must always counterbalance each other. Now, this argument would be a just one, if it could properly be said, that the motion of the ascending body was produced by that of the descending one; but, since the bodies are so connected that one cannot possibly begin to move before the other, I apprehend, that, if the bodies are supposed to move, it cannot be said that the motion of one is produced by that of the other: since whatever force is supposed to move one must be the immediate cause of motion in the other also; that is, both their motions must be simultaneous effects of the same cause, just as if the two bodies were really but one. And therefore if I was to suppose, in this case, that the superior weight of the heavier body (which may be in itself much more than able to sustain the lighter) should overcome the weight of the lighter and produce equal motions in both bodies; I do not think that from thence I could be reduced to the absurdity of supposing, that one body, by its motion, might produce in another, a motion equal to its own, and yet not lose its own at the same time. But those who argue from the equality of motions on this occasion say further, that, since the two bodies must have equal motions when they do move, they must have equal endeavours to move even whilst they are at rest, and therefore these endeavours to move, being equal and contrary, must destroy each other, and the bodies must continue at rest, and consequently ballance each other. In answer to this I must observe, that the absolute force with which a heavy body endeavours to descend from a state of rest can only be proportionable to its weight; and therefore I think

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it is necessary that some cause should be assigned why (for instance) the endeavour of one pound to descend shall be equal to that of four pounds; and especially as the fulcrum on which both weights act requires no greater force to support it than that of five pounds.

From these considerations I infer, that the reason why very unequal weights may ballance each other, should be assigned not from their having equal *momenta* when made to move together, but by proving *a priori* without considering their motions, that either the reaction of the fixed parts of the machine, or some other cause, so far takes off from the weight of the heavier body as to leave it only just able to support the lighter. However, as this equality of *momenta* which always accompanies an *æquilibrium*, affords a very elegant theorem, it ought to be taken notice of in every treatise of mechanics, and may serve as an index of an *æquilibrium*. But I would not have it applied to a purpose for which it is unfit; as it has been in another instance by Doctor Keil, who from thence gives the reason why water stands at the same heighth in a narrow tube and a wide vessel with which it communicates. And an argument of the same kind is applied still more improperly by Dr. Rutherford and others, to shew why a drop of water included in a small conical tube will move towards the narrower end; and yet the true ways of accounting for both these phænomena are extremely obvious and easy.

The simple mechanic powers are usually reckoned fix, the lever, axle and wheel, pully, wedge, inclined plane, and screw. The only method I have met with of explaining the nature of these machines on one principle, is that which I just now examined; and, as that appears to me unsatisfactory, I shall consider the

the nature of each machine separately in the order I have set them down.

The lever is said to be a right line, inflexible and void of weight. Its fundamental property is this; when any two forces act against each other on the arms of a lever, they will continue in æquilibrio, if their quantities are inversely as the distances between the points to which they are applied and the point round which the lever turns, which point is called the fulcrum or prop.

Several methods have been used, by different authors, to prove, that this property must necessarily belong to the lever. We find, in the works of Archimedes, a proof brought for this purpose, which has since been made use of by several writers of mechanics; who, I find, have somewhat altered the form of his argument, the substance of which is generally expressed as follows. — “ When a cylinder of any uniform matter is supported at its middle point, it will continue at rest; for all the parts on one side must balance those on the other, being exactly equal to them both in weight and situation, so that the whole weight of this cylinder may be looked upon as acting on the middle point on which it is supported.” From hence it is inferred, that the weight of such a cylinder will act upon whatever supports it, in the same manner as it would do if it was all contracted into the middle point of its axis. If therefore we suppose the cylinder to be distinguished into two unequal cylinders or segments, the distances between the middle points of those segments and the middle of the whole cylinder will be inversely as the lengths of the segments; that is, inversely as their weights: but, as it was said above, the weight of each cylinder, acts in the same manner as it would do if contracted

contracted into the middle point of its axis; and therefore, if the weights of these cylinders be contracted into these points, they will continue to support each as before. And thence it is concluded, that any two weights, acting against each other on a line sustained at a fixed point, will counterpoise one another, when they are inversely as the distances of the points on which they act, from the point on which the line rests. To this argument there seems to be a manifest objection; for, when the whole cylinder is distinguished into two segments, part of the weight of the greater segment acts on the same side of the fulcrum with the lesser segment; and therefore when the whole weight of the greater segment is contracted into its middle point on one side of the fulcrum, and acts all of it against the lesser segment, it requires at least some proof to shew, that this contracted weight will be ballanced by the weight of the lesser segment. Mr. Hugens, in his *Miscellaneous Observations on Mechanics*, takes notice of this objection to Archimedes's method, which, he says, several mathematicians had endeavoured to remove, but without success. He therefore, instead of this method, proposed one of his own, which depends on a postulatum that he uses in common with Archimedes, that I think ought not to be granted on this occasion; it is this: "When equal bodies are placed on the arms of a lever, the one which is furthest from the fulcrum will prevail and raise the other up". Now this is taking it for granted, in other words, that a small weight placed further from the fulcrum will support or raise a greater one. The cause and reason of which fact must be derived from the demonstration that follows, and therefore this demonstration.

stration ought not to be founded on the supposed self-evidence of what is partly the thing to be proved. But perhaps it may be said, that the postulatum may be granted merely on this account; the center of gravity of the two bodies (which in this case is the middle point between them) is not sustained; and therefore the body which is on the same side of the fulcrum with the center of gravity will descend.

In answer to this I must observe, that this property, which the center of gravity has of descending, when not placed directly above or below the point of suspension, cannot be proved to belong to it in any case; nor can we even shew that there is only one center of gravity between two bodies joined by a right line, until it is proved in general that the center of gravity of any two bodies is a point so placed between them that their distances from it are inversely as their weights: but this in effect includes the principal property of the lever, which therefore cannot be proved from any previous supposition, that the center of gravity will descend, even when the bodies are equal, and we know it is the middle point between them.

I must now proceed to consider what Sir Isaac Newton hath delivered on this subject in his Principia, after the 2d cor. to the 3d law of motion which Dr. Clarke (in his notes on Rohault) and all the subsequent writers, have quoted as an elegant proof of the property of the lever; and therefore what appears to me at present an objection to this proof I shall mention with great diffidence, and in hopes of being set right if I am wrong. Sir Isaac supposes two weights, as A and P. TAB. IV. Fig. 1. to hang by threads, from the points M and N, in a wheel or circular plane perpendicular to the horizon,

rizon and movable about its center O ; and then proposes to determine the forces which these weights have to turn the wheel round its center. In order to do this, he supposes that it is indifferent from what points in the perpendicular lines M A and N P the weights are hung, for that they will still have the same power to turn the wheel about its center. His words are :
 " Quoniam nil refert utrum filorum puncta K, L, D,
 " affixa sint vel non affixa ad planum rotæ; pon-
 " dera idem valebunt ac si suspenderentur a punctis
 " K et L, vel D et L". Now whether the points of the threads K, L, D, are fixed or not to the plane of the wheel is certainly of importance, as it must make a difference in the points of suspension of the weights, and consequently in the degrees of obliquity with which the weights act ; for the lowest point of the thread that is fixed to the plane must be considered as the point from which the weight hangs ; as the parts of the thread above that point are quite useless not being at all acted upon. And from thence I shall endeavour to shew that to suppose the weight A will have the same power to turn the wheel from whatever point in the line M A it hangs, is in effect presupposing what is intended to be proved. For it appears, from what he says immediately after, that, when the weight A hangs from the point D, if its whole force be expressed by the line A D, and be resolved into two forces, D C and A C, the former only will have any effect in turning the wheel, as it acts perpendicularly on the radius O D, while the latter is lost, its direction being parallel to O D. But it is evident, that, when the same weight hangs from the point K, as it acts perpendicularly on the radius O K, its whole force

is exerted to turn the wheel, and none of it lost by oblique action. Therefore the force which the weight A, exerts to oppose the weight P, and turn the wheel when it hangs from D, is, to the force it exerts when it hangs from K, as the line DC to AD, or as OK, to OD, (sim. triang. ADC, DOK) that is the force exerted by the weight A, hanging from the points D, and K, are inversely as the radii OD, and OK. And therefore to suppose, that these two forces will have the same effect in turning the wheel and opposing the weight P, is the same as supposing that two forces will have equal effects in moving the arms of a lever (on which they act perpendicularly) when they are inversely as the lengths of those arms. — But this is the very conclusion Sir Isaac draws from his premises, for he says: "Pondera igitur A & P, "quæ sunt reciproce ut radii in directum positi OK, "OL, idem pollebunt et sic consistent in æquilibrio, quæ est proprietas notissima libræ vectis et axis in peritrochio". This property of the lever, which is here expressed in general terms, includes two cases. For the arms of the lever may be either perpendicular or oblique to the directions of the weights. The first of these cases is the simplest, and should be first demonstrated: And I do not see how there can be any room for applying the resolution of forces in demonstrating this case, in which no part of either weight is lost by oblique action. But when this case is proved, we have from thence, by the resolution of forces, an easy way of shewing, in the second case, when the arms of the lever are oblique to the directions of the weights, that the weights will counterbalance each other, when they are reciprocally as the perpendicular distances of their lines

lines of direction from the center of motion. — From either of these cases, we may deduce an obvious reason why the weight A, should have the same power to turn the wheel, from whatever point it hangs in the line M A ; the truth of which, I am persuaded, cannot be proved independent of those cases, and therefore think it ought not to be used as a postulatum in demonstrating the general property of the lever.

Mr. Maclaurin, in his View of Newton's Philosophy, after giving us the methods which Archimedes and Newton have used for proving the fundamental property of the lever, proposes one of his own, which, he says, appears to be the most natural one for this purpose. However as to his method I shall only observe, that from equal bodies sustaining each other at equal distances from the fulcrum, he shews us how to infer that a body of one pound (for instance) will sustain another of two pounds at half its distance from the fulcrum, and from thence that it will sustain one of three pounds at a third of its distance from the fulcrum : and thus he goes on deducing, by a kind of induction, what the proportion is in general between two bodies that sustain each other on the arms of a lever. But this argument (which I do not think by any means satisfactory) he observes cannot be applied when the arms of the lever are incommensurable.

These are the methods of demonstrating the fundamental property of the lever, which are most worth taking notice of ; and, since they seem liable to objections, and the other methods I have met with are still more exceptionable, I shall propose a new proof of this property of the lever, which appears to me a very

a very simple one, and depends on a postulatum that, I believe, will be readily granted.

If a force be uniformly diffused over a right line, that is, if an equal part of the force acts upon every point of the line, and if the whole force acts according to one and the same plane; this force will be sustained, and the line kept in æquilibrio, by a single force applied to the middle point of the line equal to the diffused force, and acting in a contrary direction.

In order to shorten the following proof, I must premise by way of Lemma, that, if a right line be divided into two segments, the distances between the middle of the whole line, and the middle points of the segments, will be inversely as the segments. This is self evident when the segments are equal; and, when they are unequal, then, since half of the whole line is equal to half of the greater and half of the lesser segment, it is plain that the distance between the middle of the whole line and the middle of one segment must be equal to half of the other segment, so that these distances must be to each other inversely as the segments, all which appears evident from the inspection of TAB. VI. Fig. 2.

Let now the line G H, whose middle point is D, be divided into the unequal segments G L, and L H, whose middle points are C and F, and let two forces or weights, A and B, which are to each other as the segments G L and L H, be applied to their middle points C and F, and let them act perpendicularly on the line G H. Then (by the Lemma) the weights A and B will be to each other inversely as C D, and F D, (the distances of the points C and F, to which

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they

they are applied from the middle of the whole line) if then a third force or weight E, equal to the sum of the forces A and B, be applied to the point D, and acts on the line in an opposite direction; I say these three forces will sustain each other, and keep the line in æquilibrio. For let us suppose the force E to be removed, and instead of it another force, equal also to the sum of A and B, to be uniformly diffused over the whole line G H, and to act directly against the forces A and B, then the part of this force which acts on the segment G L, will be equal to the force A, and therefore will be sustained by it (postulatum); and the other part, which is diffused over the segment L H, will be equal to and sustained by the force B, so that the forces A and B will sustain this diffused force and keep the line in æquilibrio. — Let now two other forces act on this line in opposite directions, one of them the force E acting on the point D, as it was first supposed to do, and the other an uniformly diffused force equal to E (and consequently equal to the other diffused force), then these two additional forces will also ballance each other, and therefore the æquilibrium will still remain. So that the two forces A and B, and a diffused force acting on one side of the line sustain the force E, and a diffused force acting on the other side: but it is manifest, that, in this æquilibrium, the two diffused forces acting on opposite sides are perfectly equivalent, and therefore, if they are taken away from both sides, the æquilibrium must still remain. Hence it appears that the three weights or forces A, B and E, any two of which are (by the construction) to each other inversely as their distances from the third, will sustain each other, and keep the line on which they.

they act in æquilibrio: which is the first and most simple case of the property of the lever; for here the directions of the weights are supposed to be perpendicular to the line on which they act, and it is evident that, if one of the points C, D or F, be fixed or considered as a fulcrum, that the weights acting on the other two points will continue to support each other. I shall not trouble you with proving the second case of the property of the lever; it is most easily deduced from the first: for, when two weights act on the arms of a lever in oblique directions, and are to each other inversely as the perpendicular distances of their lines of direction from the center of motion, then, by the resolution of forces, it is easily proved, that the parts of those forces which act perpendicularly on the arms of the lever, and which only are exerted to turn the lever, are to each other inversely as the lengths of those arms; and therefore by the first case they must ballance each other.

I shall now mention some well known truths in mechanics, which, I think, cannot be proved otherwise than by deducing them from what hath been here demonstrated.

C O R. I.

It appears from hence, that the powers with which any two forces move or endeavour to move the arms of a lever, are as the rectangles, under lines proportional to the forces, and the perpendicular distances of their lines of direction from the fulcrum.

R 2

C O R.

C O R. II.

When therefore two bodies acting on the arms of a lever sustain each other, if one of them be removed farther from the fulcrum, it will preponderate; but if it be brought nearer to the fulcrum, the other weight will prevail: because the product to which its force is proportional will be increased in the first case, and diminished in the second.

C O R. III.

We learn from hence, to find out the center of gravity of any two bodies joined by an inflexible right line; and to prove that its definition will agree to one point only in the line. For if a point be taken in the line so that the distances of the bodies from it may be inversely as their weights, that point will be their center of gravity, because, when it is sustained, the bodies will be in æquilibrio. But if the line be sustained at any other point, then is the fulcrum removed farther from one body and brought nearer to the other than it was when the bodies ballanced each other; and therefore, by the preceding Cor. that body from which it is removed, or which is on the same side with the center of gravity, will descend. Consequently there is but one point in the line, which being sustained, the bodies will continue in æquilibrio, and therefore but one point only can be their center of gravity. Hence also it appears, that the center of gravity will always descend, when it is not directly above or below the point by which the body is sustained.

A C C

2 1

I shall

I shall now endeavour to be as concise as possible in what I have to say of the other mechanic powers ; having, I fear, been too tedious in my account of the lever, which however deserves to be particularly considered, since to it may be reduced the ballance, the axle and wheel, and (according to some writers) the pulley.

The ballance I do not consider as a distinct machine, because it is evidently no other than a lever fitted to the particular purpose of comparing weights together, and does not serve for raising weights, or overcoming resistances, as the other machines do.

When a weight is to be raised by means of an axle and wheel it is fastened to a chord that goes round the axle, and the power which, is to raise it is hung to a chord that goes round the wheel. If then the power be to the weight as the radius of the axle to the radius of the wheel, it will just support that weight ; as will easily appear from what was proved of the lever. For the axle and wheel may be considered as a lever whose fulcrum is a line passing through the center of the wheel and middle of the axle, and whose long and short arms are the radii of the wheel and axle which are parallel to the horizon, and from whose extremities the chords hang perpendicularly. And thus an axle and wheel may be looked upon as a kind of perpetual lever, on whose arms the power and weight always act perpendicularly, tho' the lever turns round its fulcrum. And in like manner when wheels and axles move each other by means of teeth on their peripheries, such a machine is, really, a perpetual compound lever ; and, by considering it as such, we may compute the proportion of any power to the weight it

it is able to sustain by the help of such an engine. And since the radii of two contiguous wheels, whose teeth are applied to each other, are as the number of teeth in each, or inversely as the number of revolutions, which they make in the same time; we may, in the computation, instead of the ratio of these radii, put the ratio of the number of teeth on each wheel; or the inverse ratio of the number of revolutions they make in the same time.

Some writers have thought the nature and effects of the pulley might be best explained by considering a fixed pulley as a lever of the first, and a moveable pulley as one of the second kind. But tho' the pulley may bear being considered in that light; yet, I think, the best and most natural method of explaining its effects (that is, of computing the proportion of any power to the weight it can sustain by means of any system of pulleys) is, by considering that every moveable pulley hangs by two ropes equally stretched, which must bear equal parts of the weight: and therefore when one and the same rope goes round several fixed and moveable pulleys, since all its parts on each side of the pulleys are equally stretched, the whole weight must be divided equally amongst all the ropes by which the moveable pulleys hang. And consequently if the power which acts on one rope be equal to the weight divided by the number of ropes, or double the number of moveable pulleys, that power must sustain the weight.

Upon this principle, the proportion of the power to the weight it sustains by means of any system of the pulleys, may be computed in a manner so easy and natural as must be obvious to every common capacity.

The

The proportion, which any power bears to the resisting force it is able to sustain by means of a wedge, has been laid down differently by different authors; some of whom therefore must have been mistaken: and none of them seem to have treated the matter so generally as they might have done. Without examining their several opinions, I shall proceed to consider what proportion a power acting on a wedge must have to the resistance it sustains in three different cases, to which I think all those relating to the wedge may be reduced.

First, When the resisting bodies act perpendicularly on the sides of the wedge, and recede also in lines perpendicular to the sides.

Secondly, When the resisting bodies act on the wedge in oblique directions equally inclined to the sides, and recede in lines perpendicular to the sides.

Thirdly, When the resisting bodies are confined, by planes put under them, or otherwise, to recede in particular directions oblique to the sides.

C A S E I.

Let the æquicrural triangle A, B, C, [TAB. VI. Fig. 3.] represent a wedge on whose sides the two equal resisting forces E and F act perpendicularly with lines of direction meeting at the point D, at which the power P acts perpendicularly on the base A C. Then, since these three forces are supposed to sustain each other and keep the wedge in æquilibrio, they must be to each other as the sides of a triangle to which their directions are parallel, and therefore they will be to each other as the sides of a triangle to which their directions.

directions are perpendicular; that is, the sum of the forces E and F will be to the power P, which sustains them, as the sum of the sides of the wedge to the base, or as one side to half of the base; that is, as the radius to the sine of half the vertical angle of the wedge. Hence, when in cleaving timber the wedge fills the cleft, in which case the resistance of the wood acts perpendicularly on the sides of the wedge, the power which drives the wedge must be to the cohesive force of the timber in a proportion somewhat greater than that above mentioned, in order that it may divide the timber, whose parts will then recede in lines perpendicular to the sides of the wedge.

C A S E II.

Let now the resisting forces of E and F be supposed to act obliquely on the sides of the wedge in the directions E K and F L; and let these forces be expressed by the lines E K and F L, and let each of them be resolved into two forces, expressed respectively by the lines E G, G H, and F H, H L, whereof the forces G H and H L, by acting parallel to the sides of the wedge, are lost: while the other forces, E G and F H, by acting perpendicularly on the sides of the wedge, keep the power P in æquilibrio; therefore by the first case these parts of the whole resisting force are to the power P, as radius to the sine of half the vertical angle of the wedge. But it is evident that the whole resisting force is to its parts expressed by E G, F H, as radius to the sine of the angle E K G, or F L H; and therefore (compounding these ratios) the whole resisting force will be to the power which sustains

sustains it, as the square of radius, to a rectangle under the sine of the angle which the directions of the resisting force make with the sides of the wedge, and the sine of half the vertical angle of the wedge. Now, since the force of the wedge is exerted in lines perpendicular to the surface of its sides, these resisting bodies will naturally recede in that direction; as we suppose them, in this case, free to move in any direction whatever.

C A S E III.

Let us suppose lastly, that the resisting bodies are confined, by planes put under them, to recede in the directions K E, L F, then the power which drives the wedge and the resisting force will be in æquilibrio, when the former is to the latter, as the sine of half the vertical angle of the wedge, to the sine of the angle E K G, F L H, that each side of the wedge makes with the direction in which the resisting force is confined to recede. For in the first case it was proved that the power P, which drives the wedge, is to the force with which it protrudes bodies in directions perpendicular to the sides, as the sine of half the vertical angle of the wedge to radius. Let then the line G E, which is perpendicular to the side A B, express the force with which the power P protrudes the resisting bodies in the directions G E, and H F, and let this force be resolved into two forces, expressed by the lines G O, and O E, one perpendicular and the other parallel to K E, the direction in which the resisting bodies are confined to move; then the force G O is lost, and only O E has effect in protruding the

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resisting

resisting bodies in the directions K E, and L F. This force therefore being to the force expressed by G E, as the sine of the angle (E G O, or) E K G, to the radius, and the Force G E being (as was said before) to the power P, as the radius to the sine of half the vertical angle of the wedge; it follows, that the force with which the resisting bodies are protruded in the directions K E, and L F, is to the power P, as the sine of the angle E K G, or F L H, which these directions make with the sides of the wedge, to the sine of half the vertical angle of the wedge: and consequently, if the resisting forces, which act on the wedge according to these directions, are to the power P in this proportion, there will be an æquilibrium between them.

Hence we may observe, that, if from D (the middle point in the back of the wedge) a line be drawn, as D A, meeting one of the sides; the resisting forces, which must recede in directions parallel to D A, will be to the power which sustains them, as D B, the height of the wedge, to the line D A; and this power, if at all increased, will remove these resisting bodies. When therefore the resisting bodies must recede in lines parallel to the back of the wedge, their resistance will be to the power which sustains it, as the height of the wedge, to half the breadth of it's back. This proportion of the power to the resistance in this last mentioned case, is confirmed by an experiment used by Gravesande and others, to shew the nature of the wedge. For, in this experiment, a wedge is drawn down between two cylinders, which roll on rulers parallel to the back of the wedge, and are kept together by weights. And probably it was from their attending to this experiment, without considering other cases, that

that they concluded the same proportion between the power and resistance would obtain in general.

I have already mentioned the proportion which the power that drives the wedge must have to the resistance in cleaving timber, when the wedge exactly fills the cleft; which case however seldom happens; for the wood generally splits to some distance before the wedge. And then, in order that there may be an equilibrium between the power driving the wedge and the resistance of wood, the former must be to the latter, as the sine of half the vertical angle of the wedge, to the cosine of the angle which the side of the cleft makes with the side of the wedge. The truth of which is easily understood from what was proved in the third case of the wedge; for the cosine of the angle, contained between the side of the cleft and the side of the wedge, is the sine of the angle which the side of the wedge contains with the direction in which the wood recedes; because, as the cleft opens, the wood must recede in lines perpendicular to the sides of the cleft; and in the direction of those lines doth the resistance of the wood act on the sides of the wedge.

The inclined plane is reckoned by some writers among the mechanic powers; and I think with reason, as it may be used with advantage in raising weights.

Let the Line A B [TAB. VI. Fig. 4.] represent the length of an inclined plane, A D its height, and the line B D we may call its base. Let the circular body G E F, be supposed to rest on the inclined plane, and to be kept from falling down it by a string C S tyed to its center C. Then the force with which this body

stretches the string will be to its whole weight, as the sine of ABD, the angle of elevation, to the sine of the angle which the string contains with a line perpendicular to AB the length of the plane. For let the radius CE be drawn perpendicular to the horizon, and CF perpendicular to AB: and from E draw EO parallel to the string and meeting CF in O. Then, as the body continues at rest and is urged by three forces, to wit, by its weight in the direction CE, by the reaction of the plane in the direction FC, and by the reaction of the string, or the force by which it is stretched, is to the weight of the body, as EO to CE: that is, as the sine of (the angle CEF, which is equal to) ABD, the angle of elevation, to the sine of the angle EOC, equal to SCO, the angle which the string contains with the line CF perpendicular to AB, the length of the plane.

When therefore the string is parallel to the length of the plane, the force with which it is stretched, or with which the body tends down the inclined plane, is to its whole weight, as the sine of the angle of elevation, to the radius, or as the height of the plane to the length. And in the same manner it may be shewn that, when the string is parallel to BD, the base of the plane, the force with which it is stretched is to the weight of the body, as AD to BD, that is, as the height of plane to its base. If we suppose the string, which supports the body GEF, to be fastened at S, and that a force, by acting on the line AD, the height of the plane, in a direction parallel to the base BD, drives the inclined plane under the body, and by that means makes it rise in a direction parallel to AD.

Then

Then, from what was proved in the third case of the wedge, it will appear, that this force must be to the weight of the body, as A D to B D, or rather in a proportion somewhat greater: if it makes the plane move on and the body rise.

From this last observation we may clearly shew the nature and force of the screw; a machine of great efficacy in raising weights or in pressing bodies closely together. For if the triangle A B D be turned round a cylinder whose periphery is equal to B D, then the length of the inclined plane B A will rise round the cylinder in a spiral manner; and form what is called the thread of the screw: and we may suppose it continued in the same manner round the cylinder from one end to the other; and A D the height of the inclined plane will be every where the distance between two contiguous threads of this screw, which is called a convex screw. And a concave screw may be formed to fit this exactly, if an inclined plane every way like the former be turned round the inside of a hollow cylinder, whose periphery is somewhat larger than that of the other. Let us now suppose the concave screw to be fixed, and the convex one to be fitted into it, and a weight to be laid on the top of the convex screw: Then, if a power be applied to the periphery of this convex screw to turn it round, at every revolution the weight will be raised up thro' a space equal to the distance between the two contiguous threads, that is to the line A D the height of the inclined plane B A; therefore since this power, applied to the periphery, acts in a direction parallel to B D, it must be to the weight it raises as A D to B D, or as the distance between two contiguous threads, to the periphery of the convex screw.

N. B.

N. B. The distance between two contiguous threads is to be measured by a line parallel to the axle; if we now suppose that a hand-spike or handle is inserted into the bottom of the convex screw, and that the power which turns the screw is applied to the extremity of this handle, which is generally the case; then as the power is removed farther from the axis of motion, its force will be so much increased (vide what was said of the lever, cor 1.) and therefore so much may the power itself be diminished. So that the power, which, acting on the end of a handle, sustains a weight by means of a screw, will be to that weight, as the distance between two contiguous threads of the screw, to the periphery described by the end of the handle. In this case we may consider the machine as composed of a screw and a lever, or, as Sir Isaac Newton expresseth it, *Cuneus a velle impulsus.*

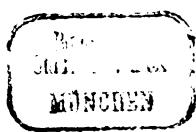
I have now given you my sentiments, as to the principles on which, I think, the efficacy of the mechanic powers may be most properly explained; and hope that, where I have presumed to differ from others, you will think I have some appearance of reason on my side. I find my paper has been drawn out much beyond what I at first expected, and I fear much beyond your patience; and therefore shall detain you no longer than to assure you that I am, Sir,

with the sincerest regard, your

most obedient humble servant,

Hugh Hamilton.

XXVI. An





The Origin made so.

XXVI. An Account of some subterraneous Apartments, with Etruscan Inscriptions and Paintings, discovered at Civita Turchino in Italy [TAB. VII. VIII. IX.] : Communicated from Joseph Wilcox, Esq; F. S. A. by Charles Morton, M. D. S. R. S.

Read March 17, 1763.

Civita Turchino, about three miles to the north of Corneto, is an hill of an oblong form, the summit of which is almost one continued plain. From the quantities of medals, intaglio's, fragments of inscriptions, &c. that are occasionally found here, this is believed to be the very spot, where the powerful and most ancient city of Tarquinii once stood: tho' at present it is only one continued field of corn. On the south-east side of it runs the ridge of an hill, which unites it to Corneto. This ridge is at least three or four miles in length, and almost entirely covered by several hundreds of artificial hillocks, which are called, by the inhabitants, Monti Roffi. About twelve of these hillocks have at different times been opened; and in every one of them have been found several subterranean apartments cut out of the solid rock. These apartments are of various forms and dimensions: some consist of a large outer room, and a small one within; others of a small room at the first entrance, and a larger one within: others are supported by a column of the solid rock, left in the centre, with openings on every part, from twenty to thirty feet. The entrance to them all is by a door of about five feet in height, by two feet and

and an half in breadth. Some of these have no other light but from the door, while others seem to have had a small light from above, through an hole of a pyramidal form. Many of these apartments have an elevated part that runs all round the wall, being a part of the rock left for that purpose. The moveables found in these apartments consist chiefly in Etruscan vases of various forms ; in some indeed have been found some plain sarcophagi of stone with bones in them. The whole of these apartments are stucco'd, and ornamented in various manners : some indeed are plain ; but others, particularly three, are richly adorned ; having a double row of Etruscan inscriptions running round the upper parts of the walls, and under it a kind of freize of figures in painting : some have an ornament under the figures, that seem to supply the place of an architrave. There have been no relievos in stucco hitherto discovered. The paintings seem to be in fresco, and are in general in the same stile as those which are usually seen on the Etruscan vases : though some of them are much superior perhaps to any thing as yet seen of the Etruscan art in painting. The paintings, though in general slight, are well conceived, and prove that the artist was capable of producing things more studied and more finished : though in such a subterranean situation, almost void of light, where the delicacy of a finished work would have been in a great measure thrown away ; these artists (as the Romans did, in their best ages, when employed in such sepulchral works) have in general contented themselves with slightly expressing their thoughts. But among the immense number of those subterranean apartments which

which are yet unopened, it is to all appearance very probable that many and many paintings and inscriptions may be discovered, sufficient to form a very entertaining, and perhaps a very useful, work: a work which would doubtless interest all the learned and curious world, not only as it may bring to light (if success attends this undertaking) many works of art, in times of such early and remote antiquity, but as perhaps it may also be the occasion of making some considerable discoveries in the history of a nation, in itself very great, though, to the regret of all the learned world, at present almost unknown. This great scene of antiquities is almost entirely unknown even in Rome. Mr. Jenkins, now resident at Rome, is the first and *only* Englishman who ever visited it.

XXVII. *An Account of a new Peruvian Plant, lately introduced into the English Gardens; the several Characters of which differ from all the Genera hitherto described; Presented to the Royal Society by George Dionysius Ehret, F. R. S.*

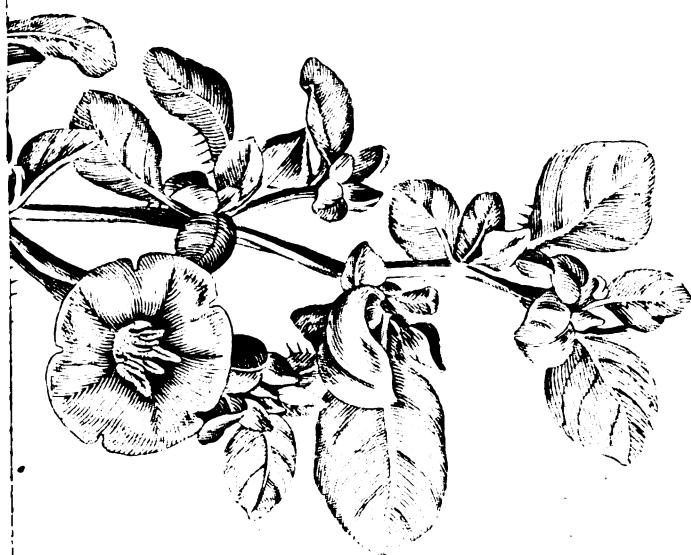
Read May 12, 1763. **T**HIS plant blowed in the Physick

Garden at Chelsea, and flourished there in great perfection in the year 1761. It produced abundance of branches, which spread themselves on the surface of the ground: these branches were greatly multiplied by side ones, which grow alternately; and are smooth towards the ground, and streaked towards the top, as the figure [TAB. X.] expresses. Each joint is furnished with many ovate-shaped leaves, having membranous ciliated footstalks.

This plant was also richly ornamented with abundance of buds and flowers: the flowers being of a sky-blew, with a dark embroidered purple bottom, made a beautiful appearance.

These flowers are monopetalous, or tube-shaped, having five obtuse laciniæ, which expand themselves exactly like unto the Alkekengi Indicum glabrum chenopodii folio, Dill. Hort. Elth. The difference in both these flowers is only in the insertion and situation of their filaments: the filaments of the alkekengi adhere at the base of the tube, but in this flower they are inserted in faux corolla at the swelling of the tube: in both of them the filaments are also hairy at their base, and their antheræ are distant from each other,

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other, whereas in the rest of the Akekengi their antheræ incline to each other. The most remarkable character in this plant is, the position of the five similar seeds, (each of these has its peculiar receptaculum) which lay in such a manner in the center of the calyx, that, at first sight, it appeared as if it belonged to that class of plants called *Herbæ verticillatae*; but, on a closer inspection, it appeared, that each of these similar seeds were separate seed vessels (or a trispermous fruit) and contained three seeds.

The ingenious and learned Dr. Albert Schlosser, of Amsterdam F. R. S. presented me with many curious dried specimens of plants, which he had collected in the Botanic Garden at Paris in the year 1755; amongst which was this plant, under the name of *Belladona Peruviana minor*. Jussieu. Hort. Reg. Paris.

Mr. Philip Miller proposed to honour this plant with the name of *WALKERIA*, in gratitude to RICHARD WALKER, D. D. Vice-master of Trinity College in Cambridge and Casuistical Professor, who, by his indefatigable pains, and at a large expence of his own, has lately founded a Physic Garden in that University, to incite and extend the study of Botany in that famous seat of the Muses.

Description of the Character, TAB. X.

FIG. a. Represents a side view of the calyx, whose leaves are open, and cover the tube of the flower: when these flowers drop off, the calyx closes instantly again (to protect the Embryo) and forms a pentagonal conical figure, see Fig. b.

T 2

FIG.

FIG. c. Is a side view of the corolla, separated from the periantheum: it has a small tube which swells into an open (monopetalous) bell-shaped figure: the limbus of the corolla, Fig. d, having small fissures, divides it into so many obtuse laciniae.

FIG. e. The inside of the corolla laid open, to expose to view the five stamina, whose filaments are inserted at the swelling of the tube: they are hairy at their base, and of equal length, and their apices are dispersed in the middle of the flower.

FIG. f. This figure represents the calyx laid open: it is monophylloous, divided into five laciniae: it also shews the situation of the five germina, which are surrounded with a yellowish nectariferous fleshy substance. From the center of these germina or embryos comes forth the style, which is of equal length with the stamina, having a globular or capitated stigma.

FIG. g. Five capsulae or seed-vessels, which are closely connected to each other, adhere together, and yet may each of them be separated, independent of its companion: they are punctated, rough, of a hard woody substance: each capsula contains three small ovate seeds: see the transverse section fig. h.

FIG. i. Represents the calyx and receptacle: the nectariferous part divides the receptacle into five semicircles: each of these vestigia had an oval-shaped round capsula, fig. k.

XXVIII. Ob-

XXVIII. Observations on two Ancient Roman Inscriptions discovered at Netherby in Cumberland: In a Letter to the Right Rev. Charles Lord Bishop of Carlisle, F. R. S. from the Reverend John Taylor, LL. D. Canon Residentiary of St. Paul's, and Chancellor of the Diocese of Lincoln.

To the Right Rev. the Lord Bishop of Carlisle.

Read May 12, 1763. **T**H E following observations I beg

leave to present to your Lordship, who was pleased to communicate those remains of antiquity, that gave birth to them. The Society of Antiquaries cannot but be greatly delighted to see your Lordship advanced to an Episcopacy, in a country Antiquitatum Romanarum feracissima; and succeeding, at a distance, a very consummate Antiquary, to whom this kingdom stands greatly indebted, the great bishop Nicholson.

Your Lordship's former situation in *another* remote part of England contributed greatly to the cultivation of this kind of letters, and brought us acquainted with what might otherwise have lain unknown or neglected. We begin already to experience the benefit of your Lordship's removal to *this*.

I am,

Amen-Corner,
April 28th,
1763.

my Lord,

etc. etc.

John Taylor.

THE inscriptions [TAB. XI.] marked N^o. I. N^o. II. were discovered at Netherby in Cumberland, the former in the year 1762, the other early in the present century: they both make mention of Marcus Aurelius Salvius, Tribune of the Cohors Prima Aelia Hispanorum Milliaria Equitata. The former moreover points out the particular emperor M. Aurelius Severus Alexander, in whose reign it was engraved: and almost directs us to the very year also: which must have been either the CCXXVIth or CCXXIXth of the Christian æra, for in those two years was that emperor consul: and one of those consulates this stone alludes to, in the last words of it; which I read thus:

IMPERATORE DOMINO NOSTRO
SEVERO ALEXANDRO, PIO,
FELICE, AVGVSTO, CONSULE.

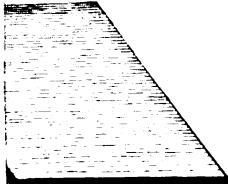
And here I take occasion to observe, that this appellation DOMINVS NOSTER was given to our emperor in the inscription before us, notwithstanding what is recorded of him by his historian, Lampridius, *Dominum se appellari vetuit*. And be it observed, that, whatever inclination Alexander Severus might have towards Christianity, as has been imagined, his forces in Britain, as appears from that pagan and frequent compliment, which occurs in the fourth line of this inscription, were not in the secret:

DEVOTA NVMINI MAIESTATIQVE EIVS.

And

*in the Parish
of May 1745.*

*of 36 Pillars
standing at E.E.*



I.

IMP CAES M AVRELIO
SEVERO ALEXANDRO PIO FEL AVG
PONT MAXIMO TRIB POT COS PP COH I AEL
HIS PANORVM & EQ DEVOTA NVMINI
MAIESTATIQVE EIVS BASELICAM
EQVESTREM EXERCITATORIAM
IAM PRIDEM A SOLO COEPTAM
AEDIFICAVIT CONSVMMAVITQVE
SVB CVRA MARI VALERIANI LEG
AVG PR PR INSTANTE M AVRELIO
SALVIO TRIB COH IMP D N
SEVERO ALEXANDRO PIO FEL



And farther still, a Cumberland inscription, marked LI. in Horsley, carries the pagan compliment to the same emperor something higher *:

DE A BVS MATRIBVS TRAMARINIS
ET NVMINI IMPERATORIS ALEXAN-
DRI AVG VSTI ET IVLIAE MAMMAEAE
Matri Avgvsti nostri et castro-
rvm totiq. domvi divinae
AETERNAEQ. VEXILLATIO. Posuit.

The passages, which seem to favour the opinion I mentioned, of this emperor's tendency to Christianity, are these of Lampridius, scil.

Judæis privilegia reservavit: Christianos esse pa-
fus est.

Matutinis horis, in larario suo (in quo et divos principes, sed optimos et electos, et animas sanctiores, in queis & Apollonium, et, quantum scriptor suorum temporum dicit, Christum, Abraham, et Orpheum, et hujuscemodi Deos habebat, ac majorum effigies) rem divinam faciebat.

Christo templum facere voluit, eumque inter Deos recipere.

Quum Christiani quendam locum, qui publicus fu-
erat, occupassent, contra Popinarii dicerent, sibi eum
deberi, rescripsit, Melius esse, ut quomodocunque il-
lic Deus colatur, quam Popinarius dedatur.

Clamabatque saepius, quod a quibusdam sive Ju-
dæis sive Christianis audierat, et tenebat: idque per
præconem, quum aliquem emendaret, dici jubebat,
QVOD TIBI FIERI NON VIS ID ALTERI NE FECERIS.

* This, as far as we know for certain, is the only inscription in Britain made under this emperor, except that we are now dis-
coursing of.

Quam

Quam sententiam usque adeo dilexit, ut et in palatio
et in publicis operibus præscribi juberet.

I mention this the rather, because I believe, that one of those inscriptions mentioned by Lampridius is come down to our times, but somewhat mutilated. It is to be found on the Via Appia, not far from the *Tres Tabernæ*; and is marked N° III. in the paper before you.

Netherby, whether the Castra Exploratorum of Antonine, with Horsley and Weseling, or the *Æsica* of Ravennas, with Camden and Gale, is the place, where the inscriptions marked N° I. and N° II. were lately discovered. N° I. served as a cover to a drain, which did not seem of any considerable age: the table part of which is five feet seven inches, by two feet four inches and a half: the margent two inches more. N° II. was found in a room or apartment belonging to a large building, lately discovered, but now pulled to pieces for the sake of the materials. My L. of Carlisle has a draught of it, where there appears to have been an hypocaust, and possibly thereabouts was the Basilica also, mentioned in our first inscription.

A Durham inscription, marked XI. in many instances explains ours, and is proper to be compared with it. It runs thus:

IMP. CAESAR M. ANT. GORDIA
NVS P. F. AVG. BALNEVM CVM
BASILICA A SOLO INSTRVXIT
PER GN. LVCILIANVM LEG. AVG.
PR PR CVRANTE M. AVR.
QVIRINO PRAE. COH. I. LEG. GOR.

The

The purport of the Inscription now under consideration is this, viz.

In the reign of Severus Alexander, Pius, Felix, &c. the Cohors Prima Ælia Hispanorum Milliaria Equitata put the finishing hand to a building, termed here Basilica Equestris Exercitatoria, the foundations of which had been laid some time before. This was conducted under the care and direction of Valerianus, the emperor's lieutenant and pro-prætor, at the instance of M. Aurel. Salvius, tribune of the aforesaid company.

Line 3. The Cohors I. Hispanorum is mentioned in many inscriptions found hereabouts, but in none of them called Ælia, as here in these two inscriptions. COH. I. AELIA DACORVM is very frequent. And in the Notitia we meet with *Cohors prima Ælia Clavigera*.

Line 4. I read HISPANORVM MILLIARIA EQVITATA; the Monogram standing for M. or MILLIARIA, and EQ. for EQVITATA, not EQVESTRIS. For the auxiliaries served on foot, some of the regiments being lined, or flanked, with horse, and called therefore Equitatæ: for that is the meaning of the word, not *promoted from the foot service to the horse*, which is the opinion of some, as Mr. Horsley, for instance, &c. I have spoken to this point more fully in my observations upon the Rutchester Inscription, which are printed in the Philosophical Transactions *.

Line 5. Basilica is a word of large extent, and commonly signifies what is built for *public use*, or by *public authority*. It is therefore frequently applied to a *bourse* or *exchange*. The *public roads* are termed

* A.D. 1747. N° 482. III.

Basilicæ : and the Christian writers took this word for their *churches*.

Though this be the common use of the word, it is not the primary. It signifies, I say, originally and principally, as it does in this inscription, a portico or colonnade, which being very large and considerable in places built for courts of justice, for public audiences and meetings of merchants, it came to pass, that the name of the *principal* was sunk in the *adjunct*; and all these places called alike *basilicæ*, from the colonnade, which attended, and perhaps sometimes encompassed them:

Basilicarum loca, *adjuncta foris*, quam calidissimis partibus oportet constitui, ut per hyemem sine molestia tempestatum se conferre in eas negotiatores possent. Vitruv. V. 1.

In the law-books I find them sometimes distinguished :

Sacram vel religiosam rem vel usibus publicis in perpetuum relictam, ut *forum*, aut *basilicam*, aut hominem liberum, inutiliter stipulor. L. 83. § 3. D. de V. O.

And so likewise Asconius upon Cic. Orat. pro Milone :

Quo igne & ipsa quoque curia flagravit, & item Porcia basilica, quæ erat ei juncta, ambusta est.

In Capitolinus I meet with *basilica centenaria*, *basilica pedum quingentorum*. And in the same light we must certainly view the words of Vopiscus in the life of Aurelian :

Miliarensem denique porticum in hortis Sallustiis ornavit, in qua quotidie et *equos* et se fatigabat.

Which passage will explain the words of Juvenal, Sat. IV. init. Quid

**Quid refert igitur, quantis jumenta fatiget
Porticibus —**

And both together, the use and destination of the building, which is the subject of our Inscription, **BASILICA** (*i.e.* porticus) **EQUESTRIS EXERCITATORIA.**

As the Roman affairs in Britain are little known under this emperor; one only Inscription besides, as I observed, either bearing his name, or referring to his age, these notices may possibly be more welcome. And what makes the first Inscription more so, is the mention of a new Legate, or lieutenant and pro-prætor, Valerianus, in this province, never taken notice of before. A copper Inscription lately discovered in the estate of the D. of Norfolk in Yorkshire, and now in his Grace's possession, affords us another, and that a very remarkable personage, under the emperor Hadrian, and one much known in the Roman history.

What was the prænomen of this Legate, l. 9. is a matter of farther enquiry.

**XXIX. A Method of lessening the Quantity
of Friction in Engines, by Keane Fitzgerald,
Esq; F. R. S.**

Read May 12,
1763. **M**ECHANICS, or that branch of
mathematics which considers mo-
tions and moving powers, their nature and laws, is
properly distinguished into rational, and practical.
U 2 **A know-**

A knowledge in rational mechanics, which comprehends the whole theory of motion, upon which natural philosophy so greatly depends, is chiefly confined to the learned; and the proper construction of engines and machines, which is the principal object of practical mechanics, altho' so very necessary to carry on the several branches of husbandry, manufacture, and commerce, upon which, the riches and power of a nation depend in a great measure, is seldom attended to, but by the meer handicraftsman; who is little acquainted with the principles he works on, and from whom no great improvements can well be expected; yet it has happened sometimes, that excellent contrivances have been invented, for raising heavy weights and overcoming their resistances, by persons who never took the trouble of examining into the cause of gravity.

As this branch is certainly most useful to mankind; and a knowledge in it, generally deemed one of the marks by which a civilized nation is distinguished from barbarians, one would imagine, it should have induced a greater attention to improvements in it, than has been generally found: But it often happens that mechanical powers, seemingly demonstrable in theory, are found very deficient in operation, from unexpected obstructions; which, with the expence and trouble that generally attend the reducing speculations of this nature into practice, have probably been the greatest obstacles to improvements in it.

One of the greatest obstructions to the mechanical powers of engines proceeds from the friction, or resistance of the parts rubbing on each other; which in general, is greater, or less, as the rubbing parts

parts bear the greater, or less pressure; and yet this obstruction is but little attended to. The theorist makes no allowance on account of friction; and the practical mechanician, who feels the effects, yet, as if unavoidable, seldom takes the trouble of searching for a remedy.

Amongst the few who have endeavoured to ascertain the quantity of friction proceeding from weight, some have deemed it equal to $\frac{1}{3}$, others to $\frac{1}{2}$, and others more, or less, according to their different methods, or accuracy in making experiments. Doctor Desaguillers gives an account of some experiments, which shew the quantity of friction in a cylinder, to be equal to $\frac{1}{2}$ of the power required to move it, when the surface of the cylinder moves as fast as the power.

In order to examine the quantities of friction proceeding from different weights, I had an exact balance made, which weighed 27 ounces; the pivets of the axis were $\frac{1}{2}$ inch diameter, and turned in brass sockets, fixed in a frame for the purpose.

Seven pound suspended on each arm, at 18 inches distance from the center, required $1\frac{1}{2}$ ounce, 2 penny weight, to be applied to either end, to overcome the resistance from friction in the slightest degree; and 3 ounces to carry it down 2 inches.

Fourteen pound, applied in the same manner, required $3\frac{1}{2}$ ounces to move the balance; and $6\frac{1}{4}$ ounces to sink either end 2 inches.

Twenty one pound required $4\frac{1}{4}$ ounces to give it the least motion; and $7\frac{3}{4}$ ounces to sink it about 2 inches.

Seven pound, suspended on each arm at 9 inches distance from the center, required 3 ounces and $\frac{1}{4}$ to move either end in the least degree.

Fourteen

Fourteen pound required $6\frac{3}{8}$ ounces ; and 21 pound required $9\frac{1}{4}$ ounces.

I placed another axis in the same ballance, the pevets of which were 1 inch diameter, and suspended 7 pound on each arm at 18 inches distance from the center, which required $3\frac{3}{4}$ ounces to be applied to either end, to overcome the resistance from friction ; and then that end sunk near 2 inches.

Fourteen pound, applied in the same manner, required $7\frac{1}{2}$ ounces, which carried that end down somewhat more than 2 inches.

Twenty one pound required $11\frac{3}{4}$ ounces, and sunk either end $2\frac{1}{2}$ inches.

Seven pound, suspended on each arm at nine inches distance from the center, required $7\frac{1}{2}$ ounces to move either end.—Fourteen pound required 14 ounces, and 21 pound required $20\frac{1}{4}$ ounces.

On repeating these experiments, there was little or no variation ; and altho the several powers, required to overcome the resistance from friction, do not correspond exactly in proportion to the several weights and distances ; yet it appears, that the least power required, was equal to $\frac{1}{4}$ the weight on the pevets ; and that it required a power nearly equal to the whole weight, to overcome the resistance from friction, with but a small degree of velocity. But it does not follow, that the extraordinary power, seemingly required to overcome the friction with this degree of velocity, is to be attributed entirely to that cause, as part of it is necessary to raise the opposite weight with the same degree of velocity, tho' some part of it certainly is. For when there is little or no obstruction from friction,

a power

a power of one ounce, more than what is just necessary to counterballance a weight of 7 pound, will raise it with as great a degree of velocity, as 2 ounces over and above what is just necessary to overcome the resistance from friction. So that it must require an additional power in proportion, to overcome the resistance from friction, with the same degree of velocity, that it may be necessary to raise the weight.

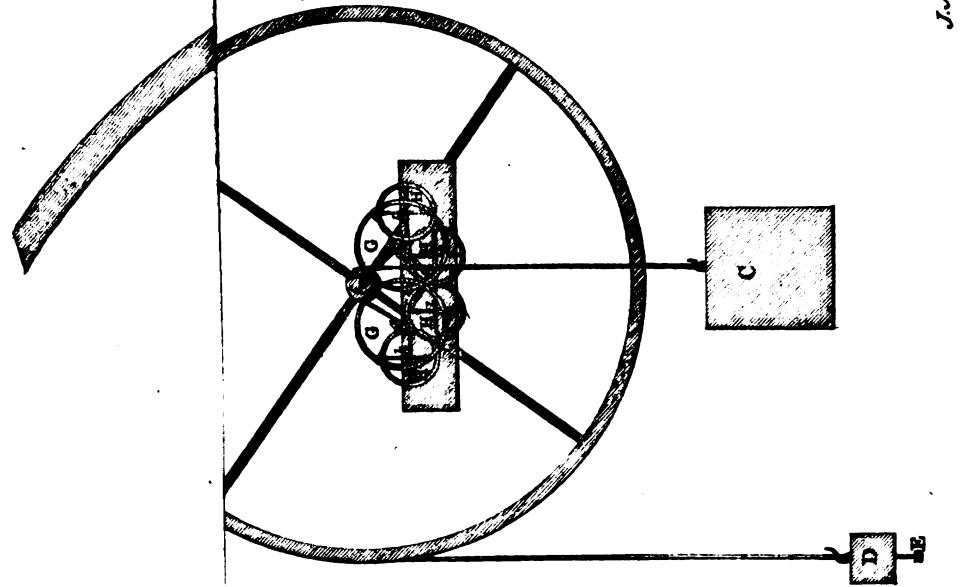
It is not imagined that these experiments should determine the exact quantity of friction proceeding generally from weight, or pressure ; which probably can never be ascertained by any experiments, however accurate ; for even in engines of equal dimensions, and loaded with equal weights, the quantities of friction may be very unequal, from circumstances differing, which are sometimes imperceptible ; such as the firmness, elasticity, roundness and smoothness of the parts rubbing on each other ; particularly the roundness, and smoothness of the gudgeons, or pevets, which, in large engines, are seldom turned true, or polished. But it appears from these experiments, that the quantity of friction in large engines may reasonably be estimated at $\frac{1}{2}$ the weight, or pressure, on the rubbing parts ; although in such as are small, and finished with exactness, the quantity may probably be about $\frac{1}{4}$.

It is evident that the quantity of friction in any engine, is equal in its opposition to a certain portion of weight, or pressure on the parts rubbing on a dead surface. And, altho' gravity is an active principle always tending to a center, and friction, a kind of vis inertiae in opposition to motion, yet it may be considered mechanically as so much weight which requires a power to overcome its resistance, in a ratio .

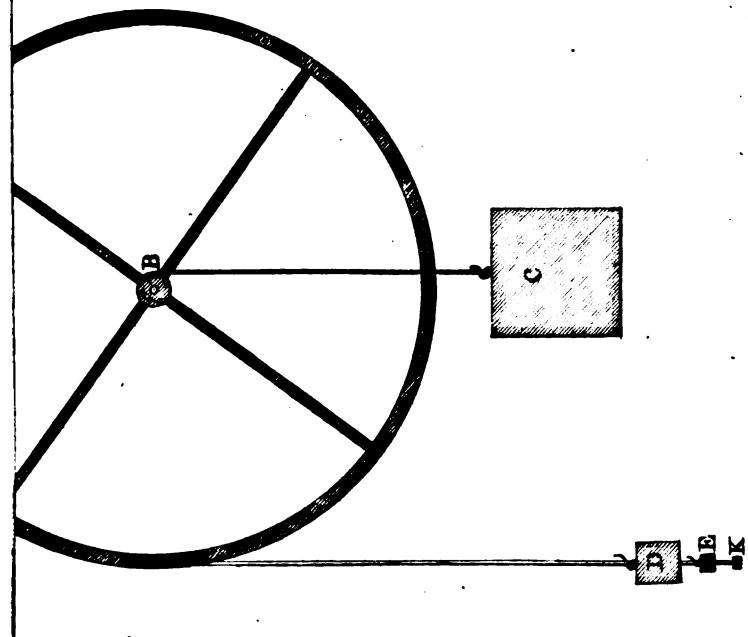
a ratio of the velocity of the power, to the velocity of the part rubbing on a dead surface; as in the axis in peritrochio, TAB. XII. Fig. 1. If the wheel A be 20 feet diameter, the axis B 1 foot diameter, the pevets f of the axis B 4 inches diameter, and the weight C to be raised by the axis B, 12 tons or 24,000 pounds.

The power D, in the wheel A, with respect to the weight C to be raised on the axis B, is required in a ratio of the semidiameter of the wheel A to the semidiameter of the axis B, which is $\frac{1}{2}$; therefore the power D = 1200 pound is sufficient to counterballance the weight C, and the least additional power would raise it, if there were no obstruction. But the quantity of friction in the pevets f , supposed equal to $\frac{1}{4}$ the weight or pressure on that part, requires an additional power in the wheel A to overcome its resistance, in a ratio of the semidiameter of the wheel A, to the semidiameter of the pevets f , or of the velocity of the power in the wheel A, to the velocity of the part rubbing on a dead surface in the pevets f , which are $\frac{1}{2}$. And as the weight of the wheel A, supposed 1500 pound, also the power D 1200 pound, required to counterballance the weight C 24,000 pound, in all 26,700 pound; center in the pevets f , the quantity of friction in the pevets f , being equal to $\frac{1}{4}$ the weight, or 13,350 pound hanging on them, will require a power in the wheel A somewhat more than $220 \frac{1}{2}$ pound to overcome its resistance. And as this additional power E $220 \frac{1}{2}$ pounds causes an additional friction = $10 \frac{1}{2}$ pounds, it also requires a further power K = $1 \frac{1}{2}$ pounds to overcome its resistance; but the quantity of friction proceeding from thence, need not be estimated in a calculation of this nature.

As



J. N. Gould Jr.



As the power E, in the wheel A, with respect to friction in the pevets f, is in a ratio of the semidiameter of the wheel A to the semidiameter of the pevet f, it is evident, that, by enlarging the diameter of the wheel A, or reducing the diameter of the pevets f, the power over friction will be increased in proportion; but whatever power is gained by enlarging the diameter of the wheel A, will be lost equally in time, or velocity, with respect to the weight C to be raised; and altho' there will be no loss in time, or velocity, by reducing the diameter of the pevets f; yet this cannot be done beyond the proper degree of strength required to sustain the weight C &c.

As it also appears that the power E with respect to friction in the pevets f, is in a ratio of its velocity to the velocity of the pevet f rubbing on a dead surface, it follows, that if the velocity of the part rubbing on a dead surface can be decreased, whilst the velocity of the power D continues in the same ratio, with respect to the weight C to be raised on the axis B; the power E over friction, will be increased in proportion, without any loss in time or velocity, as to the weight C to be raised; which may be effected in the following manner, and the quantity of friction reduced to any degree that may be required.

Fig. 2. Let the pevets f of the wheel A, turn on the peripheries of the wheels G. G. 3 feet diameter, whose pevets g, g, are 1 inch diameter, and the whole friction will be transferred from the pevets f, to the pevets g, which will then be the only parts rubbing on a dead surface, by which means the velocity of the power in the wheel A, to the velocity of the pevets g, will be in a ratio of $\frac{1}{16}$. For as the pevets f, 4

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inches

inches diameter, turn on the peripheries of the wheels G, 3 feet diameter, 9 revolutions of the pevets f, are equal to 1 revolution of the wheels G; and the circumference of the pevets f, being 4 times the circumference of the pevets g, the space the pevets f, would have rubbed on a dead surface in one revolution, is equal to the space the pevets g rub on a dead surface in 36 revolutions of the pevets f; therefore the velocity of the pevets f, being $\frac{3}{4}$ to velocity of the pevets g, and the velocity of the power D in the wheel A being $\frac{6}{7}$ to the velocity of the pevets f, the velocity of the power D, to the velocity of the pevets g, is $\frac{6}{7} \times \frac{3}{4} = \frac{18}{28} = \frac{9}{14}$. So that $\frac{1}{14}$ of 13250 pound, which was the weight equal to the quantity of friction in the pevets f; or a power somewhat more than 6 pound 2 ounces in the wheel A, will be sufficient to overcome the resistance from friction in the pevets g.

To reduce this quantity of friction to a less degree, let each of the pevets g, be placed on the peripheries of the wheels H, 2 feet diameter, whose pevets b are $\frac{1}{4}$ inch diameter; and the whole friction will then be transferred from the pevets g, to the pevets b; by which the velocity of the power in the wheel A, to the velocity of the part rubbing on a dead surface, in the pevets f, will be in a ratio of $207:36$. For the circumference of the pevet g, being $\frac{1}{4}$ of the circumference of the wheel H, on which it turns, makes 24 revolutions, for 1 of the pevet b. And the circumference of the pevet g, being 4 times the circumference of the pevet b, the space the pevet g would have rubbed on a dead surface in 1 revolution, is equal to the space the pevet b rubs in 96 revolutions; therefore

therefore the velocity of the pevet g , to the velocity of the pevet b , is $\frac{9}{16}$. And as it appears that the velocity of the power in the wheel A, is in a ratio of $\frac{11}{16}$ to the velocity of the pevet g ; consequently its velocity to that of the pevet b , is $\frac{11}{16} \times \frac{9}{16} = \frac{99}{256}$. So that $\frac{1}{207360}$ of 13,250 pound the quantity of weight deemed equal to the friction originally in the pevets f , or a power E somewhat more than 2 ounces, will be sufficient to overcome the friction in the pevets b .

Thus it is evident, that, by the application of additional wheels, or by enlarging the diameters of these, the resistance from friction may be reduced to less than the resistance of the medium the wheel passes through..

The whole weight which centers in the axis of the wheel A, being equally divided on the pevets f , and further subdivided on 32 pevets b , the weight on each of these pevets, being but $\frac{1}{16}$ of the weight on each of the pevets f , does not require more than $\frac{1}{16}$ of its strength. And as the quantity of friction in each of the pevets b is in proportion to the weight or pressure it bears, the sum of the several quantities of friction in the 32 pevets b , is equal to the quantity of friction that was originally in the 2 pevets f , in proportion to their velocities.

There is also some additional friction in the pevets b , on account of the weight of the wheels G and H; but, with respect to the power in the wheel A, it is not of consequence to require a calculation.

There is no engine for raising heavy weights, that has less friction than the axis in peritrochio. If the same weight were to be raised by 2 wheels, one mul-

tiplying the other ; the power in the first wheel, being in a ratio of $\frac{1}{4}$ to the weight to be raised, and $\frac{1}{2}$ to the friction in its pevets ; and the power of the second wheel in a ratio of $\frac{1}{4}$ to the weight, and $\frac{1}{4}$ to the friction in its pevets ; which powers are the same as in the wheel A, viz. $\frac{1}{4}$ with respect to the weight, and $\frac{1}{2}$ with respect to the friction ; although the powers required to counterbalance the weight on the axis, are equal in each ; yet it would require a power above 733 pound to overcome the resistance from friction in this engine, which is nearly treble the power required to overcome the friction in the wheel A, on account of four pevets rubbing on a dead surface in one, and but two pevets in the other.

By reducing the friction in the pevets of this engine, in the same manner as in the pevets of the wheel A, the power 733 pound, which is required merely on account of friction, may be applied to raise an additional weight of 14,650 pound, without any diminution in point of time, or velocity, with respect to the weight to be raised ; which at first view may seem contrary to the general principle, that whatever power is gained mechanically over weight, is lost equally in point of time, and velocity ; and is so in reality, with respect to practical mechanism ; For the saving a power, otherwise, hitherto, found necessary to overcome the resistance from friction, and applying it to the useful purpose of raising a greater quantity of weight, in equal time, is, in effect, equal to an acquisition of so much power.

If these wheels are made with tolerable exactness, and placed, as in the drawing, on a line opposite to the point of pressure of the pevets they support, the pressure

pressure will be equal on each wheel; and the greater the pressure, the more securely they are kept in their proper places. I have a double set of brass wheels, 8 inches diameter, with which I have made several experiments, and find the practice answer as near as possible to the theory. But as the expence of brass wheels, to large engines, would be very considerable, I had wheels made of wood, which I find to answer the purpose as well, if not better; as they are much lighter, and may be made strong enough to support a great weight, at a moderate expence.

The wooden wheels are fixed on an arbour, whose pevets have been turned true, and the edge of the wheel turned after it is fixed on the arbour. These wheels are placed in a wooden frame, with a small plate of brass fixed properly in the frame, for the pevets to turn in. They may be made with spokes, and fellies, capable of sustaining a considerable weight; and there is no danger of their wearing, as the pevet only rolls on the edge. I had wheels made of white deal, with several lamina glewed together, crossing each other in different directions of the grain of the wood, which hinders them from warping, or cracking; and which I found, upon trial, answered extremely well. By crossing the grain of the wood, the opposition to the pressure on the periphery is pretty equal in all parts; and the edge of the whcel, in a little time, becomes as smooth, and almost as hard as brass.

These wheels cannot be applied to wheel carriages; unless they were to move on very even ground, as sudden jerks, and turnings, would soon disorder them. But they may certainly be employed to ad-

vantage in all fixed engines, that are loaded with heavy weights ; especially when the power that operates is expensive, as men, horses, fire, &c. And in finer kind of engines, where it may be necessary to avoid any obstruction from friction as much as possible, the double, or treble wheels, where there is sufficient room, will reduce the quantity to any degree that can well be required.

Another advantage also arises from the application of these kind of wheels, that, if the motion is required to be extremely swift, though the pevets be as small as the weight they sustain can allow of, yet they scarce ever wear the holes they turn in ; for the last pevets in a treble set of wheels, which are the only ones that rub on a dead surface, will hardly make one revolution in two days.

There are several engines to which these wheels might be applied to advantage, even where the acting power costs nothing ; as watermills, where water is not always to be had plenty, which, by this means, would grind with much less water. Windmills, particularly, must receive great benefit from them ; the shaft being so large, the quantity of friction, which is in proportion to the part rubbing on a dead surface, must be greater in this, than most other engines ; besides, the rubbing part being wood, must still increase the quantity : I should therefore imagine, that, if the shaft were placed on wheels 5, or 6 feet diameter, it would not require above half the strength of wind, necessary at present. The frame in which these wheels might be placed, could easily be made in such a manner, as to be lowered, or raised ; so that if any inconvenience were found from too great velocity

velocity, when the wind increased, the shaft might then be let to turn in the usual manner. But there would be no danger of the shaft taking fire by any degree of velocity, whilst it turned on these wheels, as it would not then rub at all.

There have been many ingenious attempts, and some considerable improvements made, with respect to the saving of fuel necessary to work a fire engine, which is an article of great expence : but I do not find the diminution of friction has been considered as any ways material in this point, although it must necessarily reduce the quantity of fuel in proportion.

The power of a fire engine is estimated by the diameter of the cylinder and piston ; on which the atmosphere presses, when there is a vacuum made by the condensation of the steam with which the cylinder has been filled. This power, or pressure, is deemed equal to 15 pound per inch square on a medium : but I should imagine, that the steam, with which the cylinder is filled, being water expanded into 4000 times its bulk by the action of fire, when reduced to its original state by a strong injection of cold water dashing against the bottom of the piston, and mixing with it, must occupy such a space in the cylinder, as to hinder a perfect vacuum, which appears, in some measure, from the effects ; for the power of the atmosphere on a fire-engine is seldom found to raise 7 pound per inch, and it can hardly require 8 pound per inch to overcome the friction of the several parts of the engine, and also to give a proper degree of velocity to the leaver.

The friction of the piston moving up and down in the cylinder, and of the forcers or working rods, is in

in proportion to the diameter of the cylinders they work in. That of the plug frame, which is a piece of timber moved by the leaver through a wooden groove, by which the steam valve, and injection cock are opened and shut alternately, is pretty considerable; but the quantity proceeding from the several parts cannot be estimated with any tolerable degree of precision.

The whole weight to be raised, as also the superior power by which it is raised, center in the pevets of the axis of the great leaver, and the quantity of friction in the pevets, may be deemed equal to half so much weight hanging on them.

In order to form some estimate of the quantity of weight with which the axis of the leaver of a fire-engine is loaded, I took the dimensions of the several parts of that at the York-Buildings water-works; the leaver of which is 27 feet long, 2 feet 6 inches by 2 feet 2 inches in the middle, and 2 feet by 22 inches at the ends. The weight of which, with the archeads, chain, rods, and working frame hanging at one end, and the piston and chain at the other, may be computed at 6 tons, or 12,000 pound. The cylinder is 45 inches diameter, about 1591 square inches; which, at 15 pound per inch pressure of the atmosphere, is 22,274 pound. The pillar of water to be raised is 10,060 pound, which is not $6 \frac{1}{2}$ pound per inch; so that the remainder of the power is employed in overcoming the resistance from friction in the several parts of the engine, and giving the leaver a degree of velocity equal to 120 feet per minute, which it moved in common work.

The

The weight of the power, or pressure, of the atmosphere taken at 14 pound per inch square; 22,274 pound, with the pillar of water 10,060 pound, and also of the leaver, &c. 12,000 pound, amounting in the whole to about 22 tons, center in the axis of the leaver. The quantity of friction resulting from this weight, supposed equal to half, or 11 tons, hanging on the pevets 6 inches diameter, the leaver being 27 feet long, requires a power at either end = 425 pound to overcome its resistance in the least degree, and must still require a further power to overcome the friction of the other parts of the engine, and give the leaver a degree of velocity = 120 feet per minute.

Before I give an account of the method I took to reduce the quantity of friction in the pevets, it may be proper to mention a general error in the manner of placing the axis of the leaver under the beam. A balance, having its center of motion underneath, and equal weights at each end, being placed horizontally, will remain in that position; as both weights are equidistant from the center of gravity, which is perpendicular to the center of motion; but when it is made to incline to either side, it will continue to move on that side, untill it becomes parallel to the horizon, with the center of motion above the balance: for when either end is depressed in the least degree, as in fig. 3, it becomes more distant from the center of gravity; and the opposite end which is raised in proportion, is brought nearer to it, although both ends still continue equidistant from the center of motion.

Fig. 3. The lever A of this engine is 2 feet 9 inches from the upper part of the beam, to the center of its axis B placed underneath; and weighs, with its arch-heads, about 5 tons. When it was placed in a horizontal position, it required but $93\frac{1}{2}$ pound to overcome the resistance from friction in the pevets; but when either end was depressed 4 feet below the level, at which distance the springs are fixed, it required $53\frac{1}{4}$ pound to be applied to the opposite end to bring it back again: so that a power = $440\frac{1}{2}$ was required, on account of the center of gravity being so much changed by the position of the axis underneath.

Fig. 4. To avoid this general error; I had the axis B placed on the upper side of the leaver, and fixed by proper bolts and screws to a bar of iron equally strong, placed underneath: and, in order to reduce the quantity of friction, which is in proportion to the space rubbing on a dead surface in equal time, I had them made in the form b B, fig. 4, by which they are equally strong, though the rubbing part b, is but $1\frac{1}{2}$ diameter; so that by changing only the form of the pevets, the friction is reduced to $\frac{1}{4}$ of its original quantity. I applied two quadrants, D D, to each of these pevets, whose radii are 2 feet 6 inches, by which the whole friction of the pevets b of the axis of the leaver, are transferred to the pevets d of the quadrants, which are $1\frac{1}{2}$ inch diameter. These quadrants are equal in effect to wheels 5 feet diameter; the radius of which is 4° to the semidiameter of its pevet, and reduce the friction in the pevets of the quadrants to $\frac{1}{75}$ part of what it was in the pevets b of the axis; which x by $\frac{1}{4}$ the reduction made by changing the form of the pevets = $\frac{1}{15}$: by which means the friction

friction that was in the pevets B, fig. 3. of the great axis, which was = 425 pound, is reduced to $\frac{1}{16}$, or somewhat less than $2\frac{1}{4}$ pound.

Upon trial, the leaver, that before required a power of 95 pound to overcome the least resistance from friction, was as easily effected by the application of $\frac{3}{4}$ pound; and the resistance from friction occasioned by a weight of 6 tons is of so little consequence, that the leaver may be swung with a slight thread, and will continue in a state of vibration for several minutes after.

The original quantity of friction in the pevets B of the leaver A, fig. 3. which, when loaded with it's full weight 22 tons, required a power = 425 pound to overcome it's resistance, is by this method reduced to 2 pound 10 ounces; and, if there were any need of reducing it further, it might be done by applying two small quadrants to each pevet of the larger, which would reduce it to one ounce or less.

It is not easy to determine the quantity of friction that was in the plug frame, but that has also been reduced to $\frac{1}{16}$ by the application of several rollers 5 inches diameter, whose pevets are $\frac{1}{4}$ inch diameter, on which it now moves. But it is evident that a power = $440\frac{1}{4}$ has been saved by changing the position of the axis of the leaver; and a power of 421 pound 6 ounces by reducing the quantity of friction in the pevets.

The visible effect, with respect to the working of the engine, according to the most exact observations by different persons, both before, and after these several alterations were made, is, that it now makes 18 strokes at 8 feet per stroke, for 15

that it ever made, with the same, or rather a smaller quantity of fuel; and must therefore discharge $\frac{1}{2}$ more water in equal time; which consequently saves $\frac{1}{2}$ of the fuel. But the effect is found still greater, as to supplying the tenants with water; for the engine performs the same service better now in 5 hours, than ever it did before in six: which can only be accounted for, by the extraordinary regularity of its stroke, which does not abate of it's full length suddenly, as it used to do, when the strength of the fire abated: this I take to be occasioned in a great measure, from placing the axis above the leaver, by which the center of gravity becomes reversed to what it was before; so that it requires the same power to keep the end of the leaver depressed as low as the springs, that it required before to bring it back, when so much depressed; which is a particular benefit; for the stop, or sett, generally in large engines, when the ends of the leaver come to the springs, is a defect that has been endeavoured to be remedied in some degree, by the help of the springs. But when the axis is placed above the leaver, and the friction reduced, as in fig. 4, if one end is brought down to the springs, and let to return, it carries the other end down to the springs without any assistance, and will continue to do so several times, abating somewhat of the length of the stroke, each time.

This engine, from several improvements that have been made in the boyler, consumes but 4 bushels of coals in an hour; which is deemed $\frac{1}{2}$ less than others of equal bigness; and it performs the same work now in 20 hours, that it did before in 24 hours, it is a saving, in effect, of 16 bushels in 24 hours, amounting

mounting to 162 chaldrons in a year's constant work, which is a very considerable article, even where coals are to be had at a cheap price,

It may be proper to observe that the archeads C of the leaver, must be drawn from the center of the small part b of the pevet, which turns on the quadrants. The quadrants and frame must be made sufficiently strong, which I had made of cast iron. The pevets of the quadrants are made of tempered steel, and turned true. There are four pillars G in the back plate of the frame, with shoulders, and strong screws, which pass through the fore plate, and are screwed tight by a nut i, when the quadrants are placed in the frame.

The back plate E (fig. 4.) of the frame, is longer than the fore plate F, in order to admit the iron bolts G at each end; by which the frame is screwed to a wooden block. The edges of the frame rest on a broad plate of iron, laid on a level board; upon which the blocks and frames are placed, and bolted down in the usual manner. The holes that the pevets of the quadrants turn in, are made in square pieces of bras e, riveted for the purpose into the frame plates.

The round part b, of the axis B, fig. 4, is made of hardened steel, and the edges g of the quadrants are also of the same metal; otherwise the very great weight they sustain, would make a deep impression in that part. There are two springs, b b, to each quadrant, which keep them in their proper places, and yield easily to the motion of the quadrants.

There was great care taken to make the frame square, and place the quadrants upright and level; and

and also to place the leaver exactly in the center. By which means there has been no alteration required since they were first fixed ; and the engine continues to work as even, and true as it is possible.

I have applied wheels for reducing friction to some other engines with great advantage, which I shall take the liberty of laying before the Royal Society some other time ; and fear I have trespassed too much on their patience already by this long detail.

*XXIX. The Difference of Longitude between
the Royal Observatories of Greenwich and
Paris, determined by the Observations of
the Transits of Mercury over the Sun in the
Years 1723, 1736, 1743, and 1753 : By
James Short, M. A. F.R.S.*

Read June 2, 1763. **I**T will, no doubt, appear surprizing, that I should attempt to determine the difference of longitude between two of the most celebrated observatories in Europe ; and in which some of the greatest astronomers, that ever lived, have, for above eighty years, been constantly observing the motions of the heavenly bodies : yet it is most certain, that, to this day, we are ignorant of the said difference of longitude : the English astronomers reckoning it to be = 9' 20'', and the French setting it down at 9' 10'', which, they tell us, was found

found by M. Cassini, by observations of the eclipses of Jupiter's first satellite made by him, whilst in London in the year 1698: we are no where told, that I know of, by what observations the English astronomers have fixed this difference at $9' 20''$.

In the Memoirs of the Royal Academy of Sciences at Paris, for the year 1734, there is an account given of thirty-three corresponding observations of the eclipses of the first satellite of Jupiter, made at Greenwich and Paris, from the year 1677, to the year 1701: The mean of these thirty-three observations gives the difference of longitude between Paris and Greenwich = $9' 29''$.

I had lately the honor to deliver to this Society, a paper concerning the parallax of the Sun, determined by the observations of the late transit of Venus: In that paper I took notice that observations of the transits of Venus and Mercury over the Sun; have always been looked upon by astronomers, as very proper for determining the differences of longitudes between the places where such observations have been made. I have calculated, and it may be demonstrated, that, if we compare the observations of the late transit of Venus made at Greenwich, and by M. de la Lande at Paris, and suppose that the difference of longitude between these two places is = $9' 25'$, it will follow that the Sun and Venus are at an infinite distance, which is absurd. Again, if we suppose the difference to be greater, it will follow, that the Sun and Venus are more than infinitely distant, which is likewise absurd. We are therefore certain, if these observations are to be depended on, that the difference of longitude between Greenwich and Paris is less

less than $9' 25''$. If we compare the observations made at Savile-house with the same observation by M. de la Lande at Paris, and reason in the same manner, we shall find that the difference of longitude between Greenwich and Paris must be less than $9' 33''$. Thus far, therefore, a limit, one way, is fixed for the difference of longitude between these two places.

The late transit of Venus was the only one which had ever been observed at Greenwich and Paris, and by comparing the observation at Greenwich, with that made by M. de la Lande at Paris, the difference of longitude comes out = $9' 8''$, and if we compare the observations at Savile-house ($30''$ of time west of Greenwich) with that of M. de la Lande *, the said difference of longitude comes out = $9' 16''$. Since, therefore, we have only this one transit of Venus, by which we can determine this difference of longitude, we must have recourse to the transits of Mercury, of which there have been four since the year 1723, observed at London, at Greenwich and at

| | | | |
|--|---|---|-------------|
| * M. de la Lande saw the internal contact of Venus with the Sun's limb | — | — | at 8 28 25 |
| Père Clouet | — | — | at 8 28 26 |
| M. Meffier | — | — | at 8 28 27 |
| M. Ferner | — | — | at 8 28 29 |
| M. de la Caille | — | — | at 8 28 37½ |
| M. Maraldi | — | — | at 8 28 42 |

Since, therefore, the observations of messieurs Maraldi and de la Caille differ so much from the observations of the first four gentlemen (who agree very nearly together) it is plain that they ought to be rejected; and indeed M. de la Caille says, in a letter to Dr. Bevis, that the telescope he observed with was a bad one, and consequently his observation not to be depended on: M. de la Lande says the same in a letter to Mr. Maskelyne, read at the Royal Society.

Paris.

Paris. I have, therefore, extracted from the Philosophical Transactions, and the Memoirs of the Royal Academy at Paris, the several observations of the four transits of Mercury over the Sun in the years 1723, 1736, 1743, and 1753.

The observations in the year 1723, were made by Dr. Halley at Greenwich, by Dr. Bradley at Wansted, and by Mr. George Graham at London, by messieurs Cassini, Maraldi, and De L'isle at Paris. Those in the year 1736, were made by Dr. Bevis at Greenwich, and by messieurs Cassini and Maraldi at Paris. Those in the year 1743, were made by messieurs Cassini, Maraldi, Le Monnier and de la Caille at Paris, and by Dr. Bevis and myself at Mr. Graham's house in Fleet-street, London. Those in the year 1753 were made by messieurs Cassini, Bouguer, de L'isle, Merville, Libour, le Gentil, and de la Lande at Paris, and by Dr. Bevis and myself in Surrey-street, London.

By means of these observations, I have got no less than 63 determinations of the difference of longitude between the royal observatories of Greenwich and Paris, and having corrected them by parallax, they are as follows.

1723.

By the internal contact at ingress observed by Dr.
Halley.

| | | | | | | |
|------------|---|---|---|---|---|----|
| M. Cassini | — | — | = | 9 | ' | 23 |
| de L'isle | — | — | = | 9 | ' | 14 |
| de L'isle | — | — | = | 9 | ' | 14 |
| Maraldi | — | — | = | 9 | ' | 23 |

Dr. Bradley.

| | | | | | | |
|-----------|---|---|---|---|---|----|
| de L'isle | — | — | = | 9 | ' | 12 |
| Cassini | — | — | = | 9 | ' | 21 |
| Maraldi | — | — | = | 9 | ' | 21 |
| de L'isle | — | — | = | 9 | ' | 12 |

Mr. Graham.

| | | | | | | | |
|-----------|---|---|---|---|---|---|----|
| de L'isle | — | — | — | = | 8 | ' | 56 |
| Cassini | — | — | — | = | 9 | ' | 5 |
| Maraldi | — | — | — | = | 9 | ' | 5 |
| de L'isle | — | — | — | = | 8 | ' | 56 |

| | | | | | |
|----|-----|----|---|---|----|
| 12 | 110 | 22 | 9 | ' | 12 |
|----|-----|----|---|---|----|

1736.

By the external contact at egress observed by Dr.
Bevis.

| | | | | | | | |
|---------------|---|---|---|---|---|---|----|
| M. Maraldi | — | — | — | = | 9 | ' | 37 |
| Cassini, jun. | — | — | — | = | 9 | ' | 44 |
| Cassini, sen. | — | — | — | = | 9 | ' | 14 |

| | | | | | |
|---|----|----|---|---|----|
| 3 | 28 | 35 | 9 | ' | 37 |
|---|----|----|---|---|----|

1743. By

1743.

By the internal contact at egress observed by Dr.
Bevis.

| | | |
|-----------------|---------|---------|
| M. de la Caille | — — — = | 9' 4, 5 |
| Maraldi | — — — = | 9 18, 5 |
| Le Monnier | — — — = | 8 53, 5 |
| Cassini, sen. | — — — = | 9 33, 5 |
| Cassini, jun. | — — — = | 9 27, 5 |

$$\begin{array}{r} \hline 5 | 46 17, 5 | 9 15, 5 \\ \hline \end{array}$$

1743.

By the external contact at egress observed by Dr.
Bevis.

| | | |
|-----------------|---------|----------|
| M. de la Caille | — — — = | 9' 16, 5 |
| Maraldi | — — — = | 9 36, 5 |
| Le Monnier | — — — = | 9 23, 5 |
| Cassini, sen. | — — — = | 9 20, 5 |
| Cassini, jun. | — — — = | 9 42, 5 |

$$\begin{array}{r} \hline 5 | 47 19, 5 | 9 27, 9 \\ \hline \end{array}$$

1743.

By the internal contact at egress observed by my
self.

| | | |
|-----------------|---------|----------|
| M. de la Caille | — — — = | 8' 57, 5 |
| Maraldi | — — — = | 9 11, 5 |
| Le Monnier | — — — = | 8 46, 5 |
| Cassini, sen. | — — — = | 9 26, 5 |
| Cassini, jun. | — — — = | 9 20, 5 |

$$\begin{array}{r} \hline 5 | 45 42, 5 | 9 8, 5 \\ \hline \end{array}$$

Z 2

2743. By

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1743.

By the external contact at egress observed by my self.

| | | |
|-----------------|---------|---------|
| M. de la Caille | — — — = | 9 18, 5 |
| Maraldi | — — — = | 9 38, 5 |
| Le Monnier | — — — = | 9 25, 5 |
| Cassini, sen. | — — — = | 9 22, 5 |
| Cassini, jun. | — — — = | 9 44, 5 |

| | | |
|---|----------|---------|
| 5 | 47 29, 5 | 9 29, 9 |
|---|----------|---------|

1753.

By the internal contact at egress observed by Dr. Bevis.

| | | |
|-------------|---------|---------|
| M. Cassini | — — — = | 9 25, 5 |
| Bouguer | — — — = | 9 6, 5 |
| de L'isle | — — — = | 9 5, 5 |
| Merville | — — — = | 9 1, 5 |
| Libour | — — — = | 9 0, 5 |
| Le Gentil | — — — = | 9 9, 5 |
| de la Lande | — — — = | 9 3, 5 |

| | | |
|---|----------|--------|
| 7 | 63 52, 5 | 9 7, 5 |
|---|----------|--------|

1753.

By the external contact at egress observed by Dr. Bevis.

| | | |
|-------------|---------|-----------|
| M. Cassini | — — — = | 9 " 26, 5 |
| Bouguer | — — — = | 8 57, 5 |
| de Lisle | — — — = | 9 7, 5 |
| Merville | — — — = | 9 19, 5 |
| Libour | — — — = | 9 30, 5 |
| Le Gentil | — — — = | 9 26, 5 |
| de la Lande | — — — = | 9 25, 5 |

| | | |
|---|----------|--------|
| 7 | 65 13, 5 | 9 19 " |
|---|----------|--------|

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1753.

By the internal contact at egress observed by my self.

| | | | | | | |
|-------------|---|---|---|---|-----|---|
| M. Cassini | — | — | = | 9 | 18, | 5 |
| Bouguer | — | — | = | 8 | 59, | 5 |
| de L'isle | — | — | = | 8 | 58, | 5 |
| Merville | — | — | = | 8 | 54, | 5 |
| Libour | — | — | = | 8 | 53, | 5 |
| Le Gentil | — | — | = | 9 | 2, | 5 |
| de la Lande | — | — | = | 8 | 56, | 5 |

| | | | | | |
|---|----|----|----|----|---|
| 7 | 63 | 3, | 59 | 6, | 5 |
|---|----|----|----|----|---|

1753.

By the external contact at egress observed by my self.

| | | | | | | |
|-------------|---|---|---|---|-----|---|
| M. Cassini | — | — | = | 9 | 22, | 5 |
| Bouguer | — | — | = | 8 | 53, | 5 |
| de L'isle | — | — | = | 9 | 3, | 5 |
| Merville | — | — | = | 9 | 15, | 5 |
| Libour | — | — | = | 9 | 26, | 5 |
| Le Gentil | — | — | = | 9 | 22, | 5 |
| de la Lande | — | — | = | 9 | 21, | 5 |

| | | | | | |
|---|----|-----|----|-----|---|
| 7 | 64 | 45, | 59 | 15, | 1 |
|---|----|-----|----|-----|---|

The mean of the above 10 means is - = 9 16, 7

The mean of the above 63 results of
the difference of longitude between } = 9 15
Greenwich and Paris is _____

The

The mean of 43 results which differ
 not more than $15''$ from the mean } = $9' 16''$
 of the whole is _____ }

The mean of 19 results which differ
 less than $15''$, and more than $8''$ } = $9' 14, 2$
 from the mean of the whole, is — }

The mean of 24 results which differ
 less than $8''$ from the mean of the } = $9' 17, 5$
 whole is _____ }

The mean of the above 5 means is — = $9' 15, 8$

And even the mean of those 20 results which differ more than $15''$ from the mean of the whole, and which are rejected, gives the said difference = $9' 12\frac{1}{4}''$, which differing only $3\frac{1}{2}''$ from the 43 results, is a proof of the great accuracy in the determination of the differences of longitudes by observations of the transit of Mercury over the Sun.

Let us now examine the limit of the errors in these 10 several sets of determinations, and we shall find that the limit of the errors in the year —

- 1723 is = $27''$ by the internal contact at ingress.
- 1736 is = $30''$ by the external contact at egress.
- 1743 is = $40''$ * by the internal contact at egress.
- 1743 is = $26''$ by the external contact at egress.
- 1753 is = $25''$ by the internal contact at egress.
- 1753 is = $33''$ by the external contact at egress.

* If we reject the observations of M. le Monnier, in which there seems to be some mistake, because it differs considerably from the rest, the limit of the error will be = $29''$, agreeing nearly with the other limits.

From

From hence we may safely conclude that the difference of longitude between any two places may be determined by one single observation of the contact of Mercury with the Sun's limb, made at each place, so that the error in the determination will not exceed $30''$ of time from the truth: whereas in the above 33 observations of the eclipses of the first satellite of Jupiter we find the limit between the errors to amount to $3' 44''$ of time. If we take a mean of the said observations of the first satellite, the difference of longitude between Greenwich and Paris is $= 9' 29''$, and if we reject those which differ the most from the rest, the mean of the remaining 25 observations gives the said difference $= 9' 40''$, and the mean of those 8 observations, which are rejected, gives the said difference $= 8' 53''$, both which last determinations can be proved to be very far from the truth by the observations of the late transit of Venus; for by the said observations of Venus it appears that the difference of longitude between Greenwich and Paris cannot exceed $9' 33''$, as I said before; and if the said difference is $= 8' 53''$, then the parallax of the Sun, by the Savile-house observation compared with that of M. de la Lande at Paris, would amount to $20''$ which we are sure it cannot be.

Upon the whole therefore we may conclude, that the difference of longitude between the royal observatories of Greenwich and Paris (as determined by 63 observations of the contact of Mercury with the Sun's limb made at each place) is $= 9' 16''$. This determination would have been perhaps more decisive, if I could have had recourse to the books containing the observations of the late astronomer royal, Dr. Bradley.

Bradley. Observations! made by one of the greatest astronomers, and by the best and most accurate observer, assisted by the best and most accurate instruments, which are in any observatory: But alas! the public are hitherto deprived of the use of these most excellent observations*.

In a former paper which I had the honor to give into the Royal Society, concerning the parallax of the Sun, I therein assumed the difference of longitude between Greenwich and Paris to be = $9' 10''$; and as the determination of this difference is now more certain by the transits of Mercury above mentioned, being found = $9' 16''$; and as this difference of longitude will make some small difference in the result of the said parallax from the observations made at all those places †, which are to the east of Greenwich, where the late transit of Venus was observed: I have therefore computed them again, and they are as in the following synoptic table.

* On Thursday following, being the 9th of June, a motion was made, at the meeting of the Royal Society, by the Rev. Nevil Maskelyne, F. R. S. and unanimously agreed to, recommending it to their Council, as visitors of the Royal Observatory, to take proper measures for obtaining and securing the astronomical observations, that have been made there in times past, for the benefit of the publick: It was also agreed on to publish them, when obtained, at the expence of the Society; and for the future, to publish the observations made at the Royal Observatory annually, in the Philosophical Transactions.

† Because the longitudes of all those places were taken from the *Connoissance des Temps*, and the *Swedish Act*s, in which their differences of longitude from Paris are marked down.

The time of the internal contact of Venus with
the Sun's limb observed at the Cape of Good Hope
compared with that at

Sun's Parallax.

| | | | | | | |
|-----------------|---|---|----|----|----|----|
| Greenwich | — | = | 8, | 42 | — | 1 |
| Shirburn-Castle | — | = | 8, | 15 | — | 2 |
| Savile-House | — | = | 8, | 57 | — | 3 |
| Leskeard | — | = | 8, | 69 | — | 4 |
| Paris | — | — | = | 8, | 54 | 5 |
| Bologna | — | — | = | 8, | 54 | 6 |
| Rome | — | — | = | 8, | 74 | 7 |
| Drontheim | — | — | = | 8, | 33 | 8 |
| Upsal | — | — | = | 8, | 60 | 9 |
| Stockholm | — | — | = | 8, | 59 | 10 |
| Hernosand | — | — | = | 8, | 78 | 11 |
| Calmar | — | — | = | 8, | 97 | 12 |
| Abo | — | — | = | 8, | 68 | 13 |
| Tornea | — | — | = | 8, | 09 | 14 |
| Cajaneburg | — | — | = | 8, | 43 | 15 |

By the mean of these 15 results, the Sun's } parallax on the day of the transit } = 8, 54
 And if we reject the 2d, 11th, 12th, } and 14th, which differ the most from } the rest, the mean of the remaining } eleven gives the Sun's parallax } = 8, 56
 Therefore the mean horizontal parallax } of the Sun is } = 8, 69

**XXX. An Account of a remarkable Fish,
taken in King-Road, near Bristol: In a
Letter from Mr. James Ferguson, to
Thomas Birch, D. D. Secret. R. S.**

Reverend Sir,

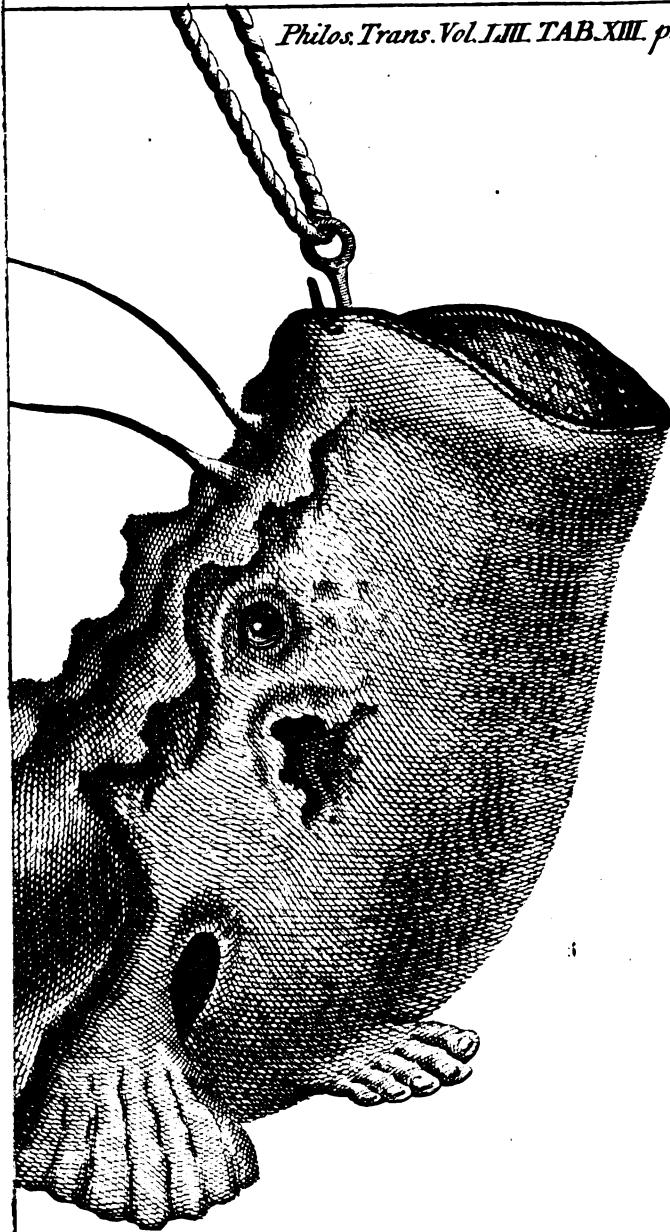
Bristol, May 5th 1763.

Read June 2d. 1763. — I Herewith take the liberty of sending

you a drawing of a very uncommon kind of fish [TAB. XIII.] which was lately caught in King-Road, a few miles from this city; and is now shewn at the Hot-Wells. It fought violently against the fisher-man's boat, after they got it in their net, and was killed with very great difficulty. No body here can tell what fish it is, only some say it is a Sea Lion; but, to the best of my remembrance, it answers not to the description or figure of the Sea Lion, that is given in Lord Anson's voyage. I took the drawing on the spot, and do wish I had had my Indian Ink and Pencils, by which it might have been much better shaded; but I hope you'll excuse the roughness of the draught, as it is the first I ever made with a pen.

The length of the fish is four feet nine inches, and the thickness in proportion as in the figure. The mouth is a foot in width, and of a squarish form: it has three rows of sharp small teeth, very irregularly set, and at some distance from each other: it has no tongue, nor narrow gullet, but is all the way down, as far as one can see, like a great hollow tube: in the back of the mouth within, there are two openings like

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J. Monro Sc.

like nostrils; and about nine inches below the jaw, and under these openings, are two large knobs, from which proceed several short teeth; a little below which, on the breast side, is another knob with such teeth.— On each side within, and about a foot below the jaws, there are three cross ribs, somewhat resembling the straight bars of a chimney-grate, about an inch distant from each other; through which we see into a great cavity within the skin, towards the breast; and under the skin, these cavities are kept distended by longitudinal ribs, plain to the touch on the outside. I put my arm down through the mouth, quite to my shoulder, but could feel nothing in the way; so that its heart, stomach, and bowels must lie in a very little compass near its tail, the body thereabout being very small.

From the neck proceed two long horns, hard and very elastic, not jointed by rings as in lobsters: and on each side of the back there are two considerable sharp edged risings, of a black and long substance. Between each eye and the breast, there is a cavity somewhat like the inside of a human ear; but it doth not penetrate to the inside. From each shoulder proceeds a strong muscular fin, close by which, towards the breast, is an opening, through which one may thrust his hand and arm quite up through the mouth: and between these fins proceed from the breast two short paws, somewhat like the fore half of a human foot, with five toes joined together, having the appearance of nails. Near the tail are two large fins, one on the back, the other under the belly. The skin is of a dark brown colour, but darker spotted in several places, and entirely without scales.

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If you think this any way deserves the notice of
the Royal Society, I shall be very glad of your com-
municating it; and am, with the greatest esteem,

Reverend Sir,

Your most obliged humble Servant,

James Ferguson.

XXXI. Rules

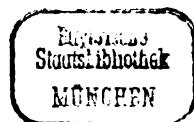


Fig. 7

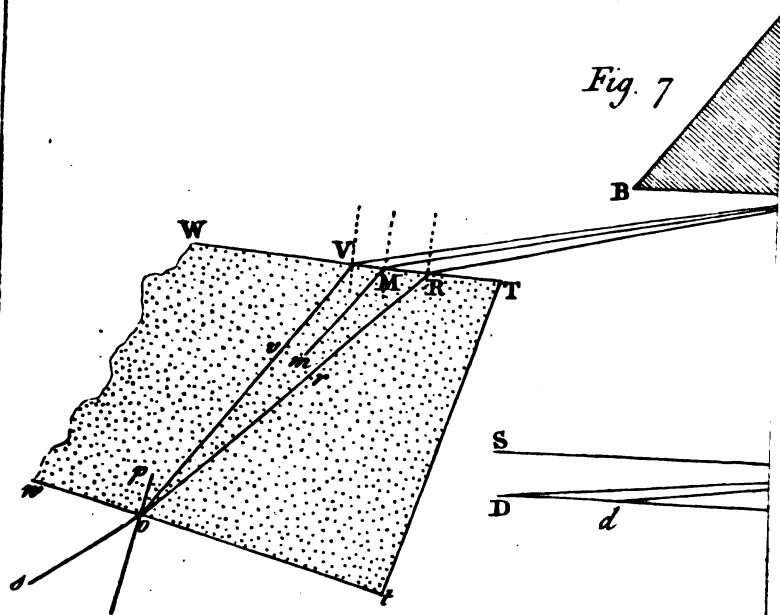
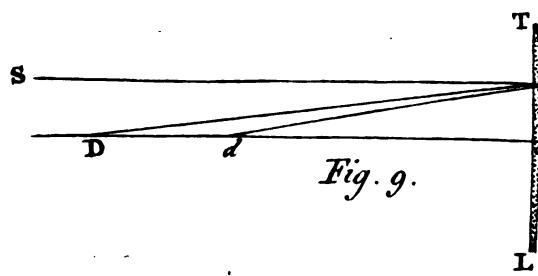


Fig. 9.



XXXI. Rules and Examples for limiting the Cases in which the Rays of refracted Light may be reunited into a colourless Pencil: In a Letter from P. Murdoch, M.A. and F.R.S. to Robert Symmer, Esq; F.R.S.
Jan. 3, 1763.

Read June 2, } 1. ^{1763.} LET SO, a small pencil of the solar light, pass through the refracting medium ABCD [TAB. XIV. Fig. 1.] whose opposite surfaces, represented by AB, CD, are parallel planes: then the violet rays OV, will, in the second refraction into the air, emerge parallel to the red; for both will be parallel to the incident ray SO, and consequently to each other: that is, Vv will be parallel to Rr, as is plain from the common principles of Optics.

2. If the light after its emergence is received on a screen placed any where beyond RV, it will be tinged with violet on the side Vv, and with red towards Rr: and if the incident pencil SO is exceeding small, all the intermediate colours will be seen in the same order as when light is refracted by a prism.

But if the incident pencil is not very small; or if the luminous body from which the rays are transmitted through a small opening at O, has a considerable breadth, like that of the Sun's disk; then so many rays of every kind will mix towards the middle of the spectrum as to produce a pure white; but at the extremities Vv and Rr, it will still be tinged

ed with violet and red: for a violet ray from the uppermost point of the Sun's disk will be more refracted than the other can be; and a red ray from the lowest part of his disk, will be less refracted than any other.

3 If BC , the distance of the refracting surfaces, is increased or diminished; RV , the distance of the extreme rays, will be increased or diminished in the same proportion: and if RV approaches very near to the aperture O , the colours will become imperceptible.

4. To reunite these rays, we may place another medium of the same refractive power, and of the same thickness ($bc = BC$) as in the figure; so as the rays Vv , Rr , &c. may enter its surface cd at the same angle as they emerged at from CD , or as SO entered AB ; and after refraction at the point o of the surface ab , to which they converge, they will be reunited into os the continuation of SO , in a pencil every way like the incident pencil SO , excepting that the light will have been somewhat weakened in its passage through the media.

5. Other things remaining, let the thickness of the second medium be cp , less than cb or CB , the surface parallel to cd being pe ; and the emergent rays or will be indeed parallel to the incident as formerly, but the spectrum will fall below the place of the screen where SO or os would fall. It will likewise be coloured, as the rays were not yet united at the point o . If the thickness be greater than cb , the spectrum will fall above the line SO or os , and the violet and red, after their intersection in o , will have changed sides.

6 Other

6. Other things remaining, suppose the refractive power of the medium ac to be increased, making the extreme rays to intersect before they reach the surface ab ; in that case, let the medium be turned round upon an axis perpendicular to the plane of refraction (represented by the plane of the figure) in the order of the letters a, b, c , so that the angle of incidence of the rays Vv , Rr , the line vr , and the angle vor may be continually decreasing till the intersection o falls into the side ab ; and the rays will emerge colourless and parallel to the incident pencil SO ; above, or below, or in the line $SOas$, according to the assumed place of the axis of revolution.

If, on the contrary, the refractive power of the medium ac be diminished, and, with it, the angle of convergence of the extreme rays; the point where they would intersect falling beyond the surface ab ; the medium must then revolve the contrary way, in the order c, b, a ; to bring the point of intersection to the surface ab . But if the refractive power be so small that even when cd becomes almost coincident with Vv , the point of intersection falls still beyond ba , in that case the rays cannot be made to emerge colourless, otherwise than by increasing the depth of the medium till its surface passes through the point of intersection. And in like manner, when the refractive power of the second medium ac is greater than that of AC , making the rays to meet within the medium, as at q a point in the line pe ; we may, instead of turning the medium round on an axis, cut off the part pa , leaving the surface pe parallel to cd ; and the emergent light will be colourless.

From:

From these few principles we may determine the phænomena of light transmitted through parallelepipeds that are contiguous to the air, their position and refractive powers being given. Or we may dispose them so that the emergent light shall, or shall not, be tinged with colours.

And we already see (what shall be more distinctly explained below) that if light be transmitted through whatever number of media (A, B, C, &c.) all the refractions may be corrected by the equal and contrary refractions of the same number of the same media (c, b, a ,) similar and similarly situated to the former; provided there is a medium Z interposed between the two series, thus; A, B, C, Z, c, b, a ; and that the rays in their passage through Z, are parallel to one another.

7. But to give the rays this parallelism in their passage through Z, and to explain the several phænomena of refracted light, we shall need the following

L E M M A, a PROBLEM.

Given (in Fig. 2.) DCB the difference of two angles ACD, ACB, and the ratio of DI the sine of the greater to BH the sine of the lesser being likewise given, to find the angles.

For DF, the sine of the given difference, write s , and for its cosine CF write c ; for the lesser sine BH, the letter z , and let the given ratio of DI to BH, be that of m to n , the radius CB being unity.

Then, having drawn FG perpendicular to DI; from the similar triangles in this figure, we shall have
CB

$CB : CH :: DF : DG$, or $1 : \sqrt{1-z^2} :: s : DG = s \sqrt{1-z^2}$; and $CB : BH :: CF : GI$, or $1 : z :: c : GI = cz$. But (by Hypoth.) $DI : BH :: m : n$; that is $DG + GI$, or $s \sqrt{1-z^2} + cz : z :: m : n$; which gives $\sqrt{1-z^2} : z$, or $CH : BH$, or $1 : \text{tang}$.

$ACB :: m - nc : ns$; that is, $\text{tang. } ACB = \frac{ns}{m - nc}$.

In words—multiply the sine of the given difference by the least term of the given ratio for a dividend: from the greater term subtract the product of the cosine of the difference and the lesser term for a divisor; and the quotient shall be the tangent of the lesser angle ACB .

Or, if you prefer a geometrical construction; In the semidiameter CB produced take CM to CB as DI to BH ; and in the tangent to the circle at B , make BL to BC , as DF to FM , and BCL shall be the lesser angle sought.

Or you need only join DM and draw the semidiameter CA parallel to it.

8. But before we apply this solution, it may be proper to give a table of the refractive powers of glass, water and spirit of wine, whether contiguous to the air, or perhaps the fluids contiguous to glass: these being the substances in which experiments may be most conveniently made: and it is also necessary to know the limitations that arise from those several powers.

I.

When light passes from air into glass, and the angle of incidence is next to 90° , whose sine is unity;

The sine of the refraction of the red rays $\frac{5}{7}$ is $.6493508 = \sin.$ } $40^\circ 29' 33", 6$
 And of the violet $\frac{5}{7}$ $= .6410256 = \sin.$ $39^\circ 52' 6"$

Whose difference $0^\circ 37' 27", 6$

is the greatest angle at which the violet and red rays can diverge in the refraction from air into glass, wanting very little of $37\frac{1}{2}'$.

And when an unrefracted pencil passes from glass into air, as soon as the angle of incidence exceeds $39^\circ 52' 6"$, the violet rays will begin to be reflected; and when the incidence exceeds $40^\circ 29' 33", 6$ the rays will be totally reflected.

II.

From Air into Water.

The sine of refraction of the red is } $48^\circ 44' 44''$
 $.7517905 = s.$

Of the violet $.7454080 = s.$ $48^\circ 11' 39"$

And the greatest divergence ————— $0^\circ 33' 5"$
 the angle of beginning reflection from water into air being $48^\circ 11' 39''$.

III. From

III.

From Water into Glass.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} \\ {}' \\ {}'' \end{array} \right. \begin{array}{l} 59 \\ 44 \\ 20 \end{array} \\ \text{Or } 0,863739 = s. \end{array}$$

$$\text{Of the violet :: 1 : } 0,859966 = s. \quad \underline{59 \ 18 \ 45}$$

The difference of which ————— ${}^{\circ} 25 \ 35 \frac{1}{2}$
is the greatest divergence.

IV.

From Air into Spirit of Wine.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} \\ {}' \\ {}'' \end{array} \right. \begin{array}{l} 47 \\ 10 \\ 20,2 \end{array} \\ \text{Or } 0,7334001 = s. \end{array}$$

$$\text{Of the violet :: 1 : } 0,7266366 = s. \quad \underline{46 \ 36 \ 18,6}$$

The difference of which ————— ${}^{\circ} 34 \ 1,6$
is the greatest divergence.

V.

From Spirit of Wine into Glass.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} \\ {}' \\ {}'' \end{array} \right. \begin{array}{l} 62 \\ 18 \\ 0,1 \end{array} \\ \text{Or } 0,8853964 = s. \end{array}$$

$$\text{Of the violet :: 1 : } 0,8821802 = s. \quad \underline{61 \ 54 \ 24}$$

And their difference ————— ${}^{\circ} 23 \ 36$
is the greatest divergence.

B b 2

These

1723.

By the internal contact at ingress observed by Dr.
Halley.

| | | | | |
|------------|---|---|---|--------|
| M. Caffini | — | — | = | 9' 23" |
| de L'isle | — | — | = | 9 14 |
| de L'isle | — | — | = | 9 14 |
| Maraldi | — | — | = | 9 23 |

Dr. Bradley.

| | | | | |
|-----------|---|---|---|------|
| de L'isle | — | — | = | 9 12 |
| Caffini | — | — | = | 9 21 |
| Maraldi | — | — | = | 9 21 |
| de L'isle | — | — | = | 9 12 |

Mr. Graham.

| | | | | | |
|-----------|---|---|---|---|------|
| de L'isle | — | — | — | = | 8 56 |
| Caffini | — | — | — | = | 9 5 |
| Maraldi | — | — | — | = | 9 5 |
| de L'isle | — | — | — | = | 8 56 |

| | | | | |
|----|-----|----|---|----|
| 12 | 110 | 22 | 9 | 12 |
|----|-----|----|---|----|

1736.

By the external contact at egress observed by Dr.
Bevis.

| | | | | | |
|---------------|---|---|---|---|--------|
| M. Maraldi | — | — | — | = | 9' 37" |
| Caffini, jun. | — | — | — | = | 9 44 |
| Caffini, sen. | — | — | — | = | 9 14 |

| | | | | | |
|---|----|----|---|---|---|
| 3 | 28 | 35 | 9 | 3 | " |
|---|----|----|---|---|---|

1743. By

1743.

By the internal contact at egress observed by Dr.
Bevis.

| | | |
|-----------------|---------|---------|
| M. de la Caille | — — — = | 9' 4, 5 |
| Maraldi | — — — = | 9 18, 5 |
| Le Monnier | — — — = | 8 53, 5 |
| Cassini, sen. | — — — = | 9 33, 5 |
| Cassini, jun. | — — — = | 9 27, 5 |

$$\begin{array}{r} 5 | 46 \ 17, \ 5 \\ 9 \ 15, \ 5 \end{array}$$

1743.

By the external contact at egress observed by Dr.
Bevis.

| | | |
|-----------------|---------|----------|
| M. de la Caille | — — — = | 9' 16, 5 |
| Maraldi | — — — = | 9 36, 5 |
| Le Monnier | — — — = | 9 23, 5 |
| Cassini, sen. | — — — = | 9 20, 5 |
| Cassini, jun. | — — — = | 9 42, 5 |

$$\begin{array}{r} 5 | 47 \ 19, \ 5 \\ 9 \ 27, \ 9 \end{array}$$

1743.

By the internal contact at egress observed by my
self.

| | | |
|-----------------|---------|----------|
| M. de la Caille | — — — = | 8' 57, 5 |
| Maraldi | — — — = | 9 11, 5 |
| Le Monnier | — — — = | 8 46, 5 |
| Cassini, sen. | — — — = | 9 26, 5 |
| Cassini, jun. | — — — = | 9 20, 5 |

$$\begin{array}{r} 5 | 45 \ 42, \ 5 \\ 9 \ 8, \ 5 \end{array}$$

Z 2

2743. By

1743.

By the external contact at egress observed by my self.

| | | |
|-----------------|---------|---------|
| M. de la Caille | — — — = | 9 18, 5 |
| Maraldi | — — — = | 9 38, 5 |
| Le Monnier | — — — = | 9 25, 5 |
| Cassini, sen. | — — — = | 9 22, 5 |
| Cassini, jun. | — — — = | 9 44, 5 |

| | | |
|---|----------|---------|
| 5 | 47 29, 5 | 9 29, 9 |
|---|----------|---------|

1753.

By the internal contact at egress observed by Dr. Bevis.

| | | |
|-------------|---------|--------|
| M. Cassini | — — — = | 9 " 5 |
| Bouguer | — — — = | 9 6, 5 |
| de L'isle | — — — = | 9 5, 5 |
| Merville | — — — = | 9 1, 5 |
| Libour | — — — = | 9 0, 5 |
| Le Gentil | — — — = | 9 9, 5 |
| de la Lande | — — — = | 9 3, 5 |

| | | |
|---|----------|--------|
| 7 | 63 52, 5 | 9 7, 5 |
|---|----------|--------|

1753.

By the external contact at egress observed by Dr. Bevis.

| | | |
|-------------|---------|---------|
| M. Cassini | — — — = | 9 " 5 |
| Bouguer | — — — = | 8 57, 5 |
| de L'isle | — — — = | 9 7, 5 |
| Merville | — — — = | 9 19, 5 |
| Libour | — — — = | 9 30, 5 |
| Le Gentil | — — — = | 9 26, 5 |
| de la Lande | — — — = | 9 25, 5 |

| | | |
|---|----------|------|
| 7 | 65 13, 5 | 9 19 |
|---|----------|------|

1753.

By the internal contact at egress observed by my self.

| | | | | | | |
|-------------|---|---|---|---|-----|---|
| M. Cassini | — | — | = | 9 | 18, | 5 |
| Bouguer | — | — | = | 8 | 59, | 5 |
| de L'isle | — | — | = | 8 | 58, | 5 |
| Merville | — | — | = | 8 | 54, | 5 |
| Libour | — | — | = | 8 | 53, | 5 |
| Le Gentil | — | — | = | 9 | 2, | 5 |
| de la Lande | — | — | = | 8 | 56, | 5 |

| | | | | | | |
|---|----|----|----|---|----|---|
| 7 | 63 | 3, | 59 | ' | 0, | 5 |
|---|----|----|----|---|----|---|

1753.

By the external contact at egress observed by my self.

| | | | | | | |
|-------------|---|---|---|---|-----|---|
| M. Cassini | — | — | = | 9 | 22, | 5 |
| Bouguer | — | — | = | 8 | 53, | 5 |
| de L'isle | — | — | = | 9 | 3, | 5 |
| Merville | — | — | = | 9 | 15, | 5 |
| Libour | — | — | = | 9 | 26, | 5 |
| Le Gentil | — | — | = | 9 | 22, | 5 |
| de la Lande | — | — | = | 9 | 21, | 5 |

| | | | | | | |
|---|----|-----|----|---|-----|---|
| 7 | 64 | 45, | 59 | ' | 15, | 1 |
|---|----|-----|----|---|-----|---|

The mean of the above 10 means is - = 9 16, 7

The mean of the above 63 results of
the difference of longitude between } = 9 15
Greenwich and Paris is ————— }

The

The mean of 43 results which differ
 not more than 15" from the mean
 of the whole is _____ } = 9' 16"
 The mean of 19 results which differ
 less than 15", and more than 8"
 from the mean of the whole, is _____ } = 9' 14, 2
 The mean of 24 results which differ
 less than 8" from the mean of the whole is _____ } = 9' 17, 5
 The mean of the above 5 means is _____ = 9' 15, 8

And even the mean of those 20 results which differ more than 15" from the mean of the whole, and which are rejected, gives the said difference = 9' 12 $\frac{1}{2}$ ", which differing only 3 $\frac{1}{2}$ " from the 43 results, is a proof of the great accuracy in the determination of the differences of longitudes by observations of the transit of Mercury over the Sun.

Let us now examine the limit of the errors in these 10 several sets of determinations, and we shall find that the limit of the errors in the year

1723 is = 27" by the internal contact at ingress.
 1736 is = 30" by the external contact at egress.
 1743 is = 40 * by the internal contact at egress.
 1743 is = 26 by the external contact at egress.
 1753 is = 25 by the internal contact at egress.
 1753 is = 33 by the external contact at egress.

* If we reject the observations of M. le Monnier, in which there seems to be some mistake, because it differs considerably from the rest, the limit of the error will be = 29", agreeing nearly with the other limits.

From

From hence we may safely conclude that the difference of longitude between any two places may be determined by one single observation of the contact of Mercury with the Sun's limb, made at each place, so that the error in the determination will not exceed $30''$ of time from the truth: whereas in the above 33 observations of the eclipses of the first satellite of Jupiter we find the limit between the errors to amount to $3' 44''$ of time. If we take a mean of the said observations of the first satellite, the difference of longitude between Greenwich and Paris is $= 9' 29''$, and if we reject those which differ the most from the rest, the mean of the remaining 25 observations gives the said difference $= 9' 40''$, and the mean of those 8 observations, which are rejected, gives the said difference $= 8' 53''$, both which last determinations can be proved to be very far from the truth by the observations of the late transit of Venus; for by the said observations of Venus it appears that the difference of longitude between Greenwich and Paris cannot exceed $9' 33''$, as I said before; and if the said difference is $= 8' 53''$, then the parallax of the Sun, by the Savile-house observation compared with that of M. de la Lande at Paris, would amount to $20''$ which we are sure it cannot be.

Upon the whole therefore we may conclude, that the difference of longitude between the royal observatories of Greenwich and Paris (as determined by 63 observations of the contact of Mercury with the Sun's limb made at each place) is $= 9' 16''$. This determination would have been perhaps more decisive, if I could have had recourse to the books containing the observations of the late astronomer royal, Dr. Bradley.

Bradley. Observations! made by one of the greatest astronomers, and by the best and most accurate observer, assisted by the best and most accurate instruments, which are in any observatory: But alas! the public are hitherto deprived of the use of these most excellent observations*.

In a former paper which I had the honor to give into the Royal Society, concerning the parallax of the Sun, I therein assumed the difference of longitude between Greenwich and Paris to be = $9' 10''$; and as the determination of this difference is now more certain by the transits of Mercury above mentioned, being found = $9' 16''$; and as this difference of longitude will make some small difference in the result of the said parallax from the observations made at all those places †, which are to the east of Greenwich, where the late transit of Venus was observed: I have therefore computed them again, and they are as in the following synoptic table.

* On Thursday following, being the 9th of June, a motion was made, at the meeting of the Royal Society, by the Rev. Nevil Maskelyne, F. R. S. and unanimously agreed to, recommending it to their Council, as visitors of the Royal Observatory, to take proper measures for obtaining and securing the astronomical observations, that have been made there in times past, for the benefit of the publick: It was also agreed on to publish them, when obtained, at the expence of the Society; and for the future, to publish the observations made at the Royal Observatory annually, in the Philosophical Transactions.

† Because the longitudes of all those places were taken from the *Connoissance des Temps*, and the *Swedish Acts*, in which their differences of longitude from Paris are marked down.

The time of the internal contact of Venus with
the Sun's limb observed at the Cape of Good Hope
compared with that at

Sun's Parallax.

| | | | | | |
|-----------------|---|---|-------|---|----|
| Greenwich | — | = | 8, 42 | — | 1 |
| Shirburn-Castle | — | = | 8, 15 | — | 2 |
| Savile-House | — | = | 8, 57 | — | 3 |
| Leskeard | — | = | 8, 69 | — | 4 |
| Paris | — | = | 8, 54 | — | 5 |
| Bologna | — | = | 8, 54 | — | 6 |
| Rome | — | = | 8, 74 | — | 7 |
| Drontheim | — | = | 8, 33 | — | 8 |
| Upsal | — | = | 8, 60 | — | 9 |
| Stockholm | — | = | 8, 59 | — | 10 |
| Hernosand | — | = | 8, 78 | — | 11 |
| Calmar | — | = | 8, 97 | — | 12 |
| Abo | — | = | 8, 68 | — | 13 |
| Tornea | — | = | 8, 09 | — | 14 |
| Cajaneburg | — | = | 8, 43 | — | 15 |

By the mean of these 15 results, the Sun's } = 8, 54
parallax on the day of the transit }

And if we reject the 2d, 11th, 12th,
and 14th, which differ the most from } = 8, 56
the rest, the mean of the remaining }

eleven gives the Sun's parallax } = 8, 69
Therefore the mean horizontal parallax } = 8, 69
of the Sun is — — — — —

**XXX. An Account of a remarkable Fish,
taken in King-Road, near Bristol: In a
Letter from Mr. James Ferguson, to
Thomas Birch, D. D. Secret. R. S.**

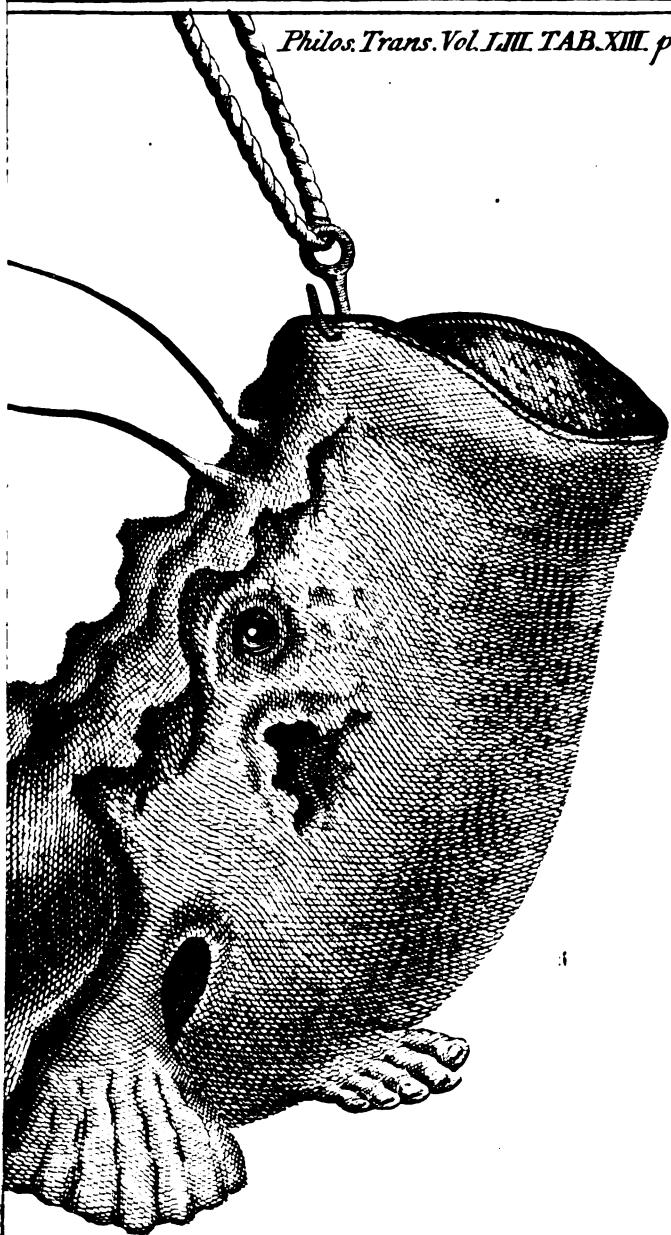
Reverend Sir,

Bristol, May 5th 1763.

Read June 2d. 1763. I Herewith take the liberty of sending you a drawing of a very uncommon kind of fish [TAB. XIII.] which was lately caught in King-Road, a few miles from this city; and is now shewn at the Hot-Wells. It fought violently against the fisher-man's boat, after they got it in their net, and was killed with very great difficulty. No body here can tell what fish it is, only some say it is a Sea Lion; but, to the best of my remembrance, it answers not to the description or figure of the Sea Lion, that is given in Lord Anson's voyage. I took the drawing on the spot, and do wish I had had my Indian Ink and Pencils, by which it might have been much better shaded; but I hope you'll excuse the roughness of the draught, as it is the first I ever made with a pen.

The length of the fish is four feet nine inches, and the thickness in proportion as in the figure. The mouth is a foot in width, and of a squarish form: it has three rows of sharp small teeth, very irregularly set, and at some distance from each other: it has no tongue, nor narrow gullet, but is all the way down, as far as one can see, like a great hollow tube: in the back of the mouth within, there are two openings like

Philos. Trans. Vol. IJII. TAB. XIII. p. 170.



J. Monet Sc.

like nostrils; and about nine inches below the jaw, and under these openings, are two large knobs, from which proceed several short teeth; a little below which, on the breast side, is another knob with such teeth.— On each side within, and about a foot below the jaws, there are three cross ribs, somewhat resembling the straight bars of a chimney-grate, about an inch distant from each other; through which we see into a great cavity within the skin, towards the breast; and under the skin, these cavities are kept distended by longitudinal ribs, plain to the touch on the outside. I put my arm down through the mouth, quite to my shoulder, but could feel nothing in the way; so that its heart, stomach, and bowels must lie in a very little compass near its tail, the body thereabout being very small.

From the neck proceed two long horns, hard and very elastic, not jointed by rings as in lobsters: and on each side of the back there are two considerable sharp edged risings, of a black and long substance. Between each eye and the breast, there is a cavity somewhat like the inside of a human ear; but it doth not penetrate to the inside. From each shoulder proceeds a strong muscular fin, close by which, towards the breast, is an opening, through which one may thrust his hand and arm quite up through the mouth: and between these fins proceed from the breast two short paws, somewhat like the fore half of a human foot, with five toes joined together, having the appearance of nails. Near the tail are two large fins, one on the back, the other under the belly. The skin is of a dark brown colour, but darker spotted in several places, and entirely without scales.

[172]

If you think this any way deserves the notice of
the Royal Society, I shall be very glad of your com-
municating it; and am, with the greatest esteem,

Reverend Sir,

Your most obliged humble Servant,

James Ferguson.

XXXI. Rules



Fig. 7

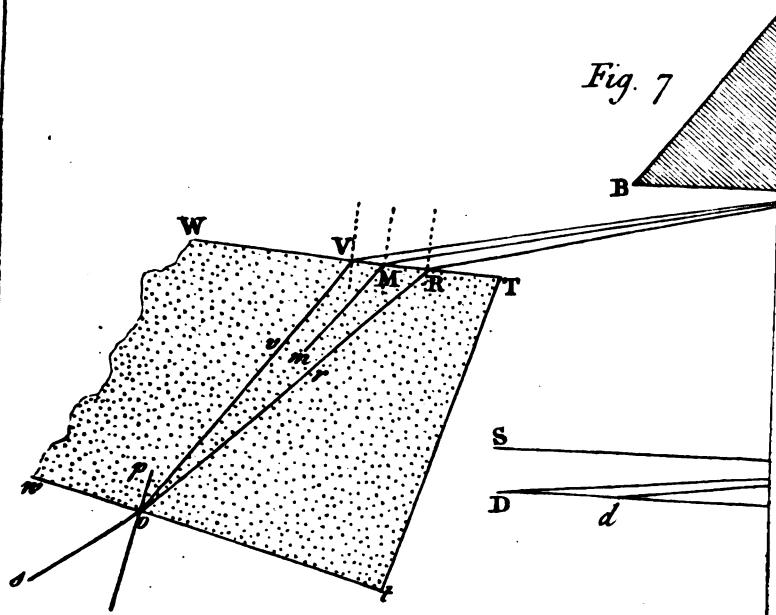
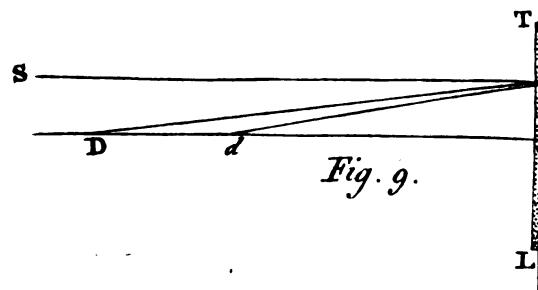


Fig. 9.

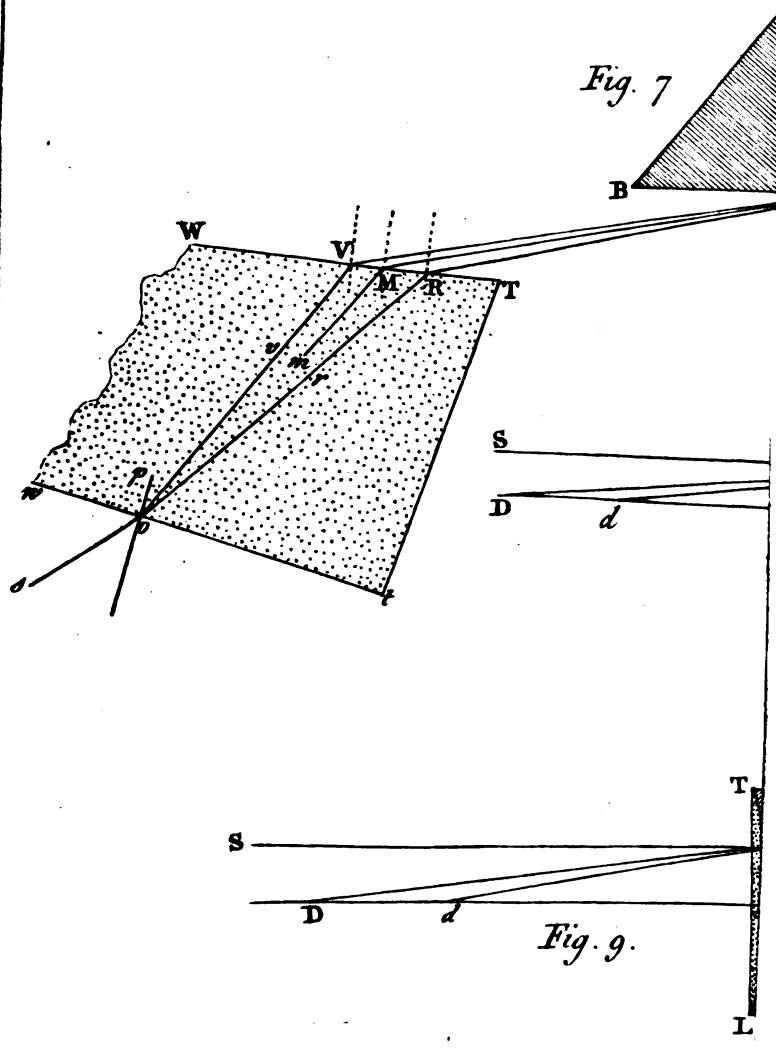


XXXI. Rules and Examples for limiting the Cases in which the Rays of refracted Light may be reunited into a colourless Pencil: In a Letter from P. Murdoch, M. A. and F. R. S. to Robert Symmer, Esq; F. R. S.
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2. If the light after its emergence is received on a screen placed any where beyond RV, it will be tinged with violet on the side Vv, and with red towards Rr: and if the incident pencil SO is exceeding small, all the intermediate colours will be seen in the same order as when light is refracted by a prism.

But if the incident pencil is not very small; or if the luminous body from which the rays are transmitted through a small opening at O, has a considerable breadth, like that of the Sun's disk; then so many rays of every kind will mix towards the middle of the spectrum as to produce a pure white; but at the extremities Vv and Rr, it will still be tinged



XXXI. Rules and Examples for limiting the Cases in which the Rays of refracted Light may be reunited into a colourless Pencil: In a Letter from P. Murdoch, M. A. and F. R. S. to Robert Symmer, Esq; F. R. S.
Jan. 3, 1763.

Read June 2, } 1. ^{1763.} LET SO, a small pencil of the solar light, pass through the refracting medium ABCD [TAB. XIV. Fig. 1.] whose opposite surfaces, represented by AB, CD, are parallel planes: then the violet rays OV, will, in the second refraction into the air, emerge parallel to the red; for both will be parallel to the incident ray SO, and consequently to each other: that is, Vv will be parallel to Rr, as is plain from the common principles of Optics.

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ed with violet and red: for a violet ray from the uppermost point of the Sun's disk will be more refracted than the other can be; and a red ray from the lowest part of his disk, will be less refracted than any other.

3 If BC, the distance of the refracting surfaces, is increased or diminished; RV, the distance of the extreme rays, will be increased or diminished in the same proportion: and if RV approaches very near to the aperture O, the colours will become imperceptible.

4. To reunite these rays, we may place another medium of the same refractive power, and of the same thickness ($bc = BC$) as in the figure; so as the rays Vv, Rr, &c. may enter its surface cd at the same angle as they emerged at from CD, or as SO entered AB; and after refraction at the point o of the surface ab, to which they converge, they will be reunited into os the continuation of SO, in a pencil every way like the incident pencil SO, excepting that the light will have been somewhat weakened in its passage through the media.

5. Other things remaining, let the thickness of the second medium be cp, less than cb or CB, the surface parallel to cd being pe; and the emergent rays wo will be indeed parallel to the incident as formerly, but the spectrum will fall below the place of the screen where SO or os would fall. It will likewise be coloured, as the rays were not yet united at the point o. If the thickness be greater than cb, the spectrum will fall above the line SO os, and the violet and red, after their intersection in o, will have changed sides.

6 Other

6. Other things remaining, suppose the refractive power of the medium ac to be increased, making the extreme rays to intersect before they reach the surface ab ; in that case, let the medium be turned round upon an axis perpendicular to the plane of refraction (represented by the plane of the figure) in the order of the letters a, b, c , so that the angle of incidence of the rays Vv , Rr , the line vr , and the angle vor may be continually decreasing till the intersection o falls into the side ab ; and the rays will emerge colourless and parallel to the incident pencil SO ; above, or below, or in the line $SOas$, according to the assumed place of the axis of revolution.

If, on the contrary, the refractive power of the medium ac be diminished, and, with it, the angle of convergence of the extreme rays; the point where they would intersect falling beyond the surface ab ; the medium must then revolve the contrary way, in the order c, b, a ; to bring the point of intersection to the surface ab . But if the refractive power be so small that even when cd becomes almost coincident with Vv , the point of intersection falls still beyond ba , in that case the rays cannot be made to emerge colourless, otherwise than by increasing the depth of the medium till its surface passes through the point of intersection. And in like manner, when the refractive power of the second medium ac is greater than that of AC , making the rays to meet within the medium, as at q a point in the line pe ; we may, instead of turning the medium round on an axis, cut off the part pa , leaving the surface pe parallel to cd ; and the emergent light will be colourless.

From:

From these few principles we may determine the phænomena of light transmitted through parallelepipeds that are contiguous to the air, their position and refractive powers being given. Or we may dispose them so that the emergent light shall, or shall not, be tinged with colours.

And we already see (what shall be more distinctly explained below) that if light be transmitted through whatever number of media (A, B, C, &c.) all the refractions may be corrected by the equal and contrary refractions of the same number of the same media ($c, b, a,$) similar and similarly situated to the former; provided there is a medium Z interposed between the two series, thus ; A, B, C, Z, c, b, a ; and that the rays in their passage through Z, are parallel to one another.

7. But to give the rays this parallelism in their passage through Z, and to explain the several phænomena of refracted light, we shall need the following

L E M M A, a PROBLEM.

Given (in Fig. 2.) DCB the difference of two angles ACD, ACB, and the ratio of DI the sine of the greater to BH the sine of the lesser being likewise given, to find the angles.

For DF, the sine of the given difference, write s , and for its cosine CF write c ; for the lesser sine BH, the letter z , and let the given ratio of DI to BH, be that of m to n , the radius CB being unity.

Then, having drawn FG perpendicular to DI; from the similar triangles in this figure, we shall have

CB

$CB : CH :: DF : DG$, or $1 : \sqrt{1-z^2} :: s : DG = s \sqrt{1-z^2}$; and $CB : BH :: CF : GI$, or $1 : z :: c : GI = cz$. But (by Hypoth.) $DI : BH :: m : n$; that is $DG + GI$, or $s \sqrt{1-z^2} + cz : z :: m : n$; which gives $\sqrt{1-z^2} : z$, or $CH : BH$, or $1 : \text{tang}$.

$ACB :: m - nc : ns$; that is, $\text{tang. } ACB = \frac{ns}{m - nc}$.

In words—multiply the sine of the given difference by the least term of the given ratio for a dividend: from the greater term subtract the product of the cosine of the difference and the lesser term for a divisor; and the quotient shall be the tangent of the lesser angle ACB .

Or, if you prefer a geometrical construction: In the semidiameter CB produced take CM to CB as DI to BH ; and in the tangent to the circle at B , make BL to BC , as DF to FM , and BCL shall be the lesser angle sought.

Or you need only join DM and draw the semidiameter CA parallel to it.

8. But before we apply this solution, it may be proper to give a table of the refractive powers of glass, water and spirit of wine, whether contiguous to the air, or perhaps the fluids contiguous to glass: these being the substances in which experiments may be most conveniently made: and it is also necessary to know the limitations that arise from those several powers.

I.

When light passes from air into glass, and the angle of incidence is next to 90° , whose sine is unity;

The sine of the refraction of the red rays $\frac{s}{r} = \frac{.6493508}{.77}$ is $.6493508 = \sin. -$ } $40^\circ 29' 33", 6$
 And of the violet $\frac{s}{r} = \frac{.6410256}{.77} = \sin. 39^\circ 52' 6"$

Whose difference $0^\circ 37' 27", 6$

is the greatest angle at which the violet and red rays can diverge in the refraction from air into glass, wanting very little of $37\frac{1}{2}'$.

And when an unrefracted pencil passes from glass into air, as soon as the angle of incidence exceeds $39^\circ 52' 6''$, the violet rays will begin to be reflected; and when the incidence exceeds $40^\circ 29' 33", 6$ the rays will be totally reflected.

II.

From Air into Water.

The sine of refraction of the red is $\frac{s}{r} = \frac{.7517905}{.87}$ } $48^\circ 44' 44", 4$

Of the violet $\frac{s}{r} = \frac{.7454080}{.87} = s. 48^\circ 11' 39"$

And the greatest divergence ————— $0^\circ 33' 5"$

the angle of beginning reflection from water into air being $48^\circ 11' 39"$.

III. From

III.

From Water into Glass.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} 59' 44'' \\ 0,863739 = s. \end{array} \right. \frac{20}{2}$$

$$\text{Of the violet :: 1 : } 0,859966 = s. \quad \underline{59' 18' 45}$$

The difference of which ————— ${}^{\circ} 25' 35\frac{1}{2}$
is the greatest divergence.

IV.

From Air into Spirit of Wine.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} 47' 10'' \\ 0,7334001 = s. \end{array} \right. \frac{20,2}{2}$$

$$\text{Of the violet :: 1 : } 0,7266366 = s. \quad \underline{46' 36' 18,6}$$

The difference of which ————— ${}^{\circ} 34' 1,6$
is the greatest divergence.

V.

From Spirit of Wine into Glass.

$$\text{Sin. incid. : s. refr. of the red :: 1 : } \left\{ \begin{array}{l} {}^{\circ} 62' 18'' \\ 0,8853964 = s. \end{array} \right. \frac{0,1}{1}$$

$$\text{Of the violet :: 1 : } 0,8821802 = s. \quad \underline{61' 54' 24}$$

And their difference ————— ————— ${}^{\circ} 23' 36$
is the greatest divergence.

B b 2

These

These numbers are partly transcribed from Sir Isaac Newton, and partly computed by a rule of Mr. Euler in the Philosophical Transactions.

They are indeed carried on to more decimal places than the experiments hitherto made can well bear: but it is hoped that hereafter methods may be devised to measure the refractions of light to a very great degree of precision.

9. When a slender pencil SO, is refracted by the surface of a denser medium OT (Fig. 3.) the extreme rays being OV, the violet, and OR the red; we have seen that the surface RVT, at which the rays pass again into the rarer medium, being parallel to the first surface OT, the extreme, and all the intermediate, rays will emerge parallel to each other, and to the pencil SO.

But if the last surface RVT cuts the former in a line perpendicular to the plane of refraction at the point T, on the side of the radiant point S, then the extreme rays being refracted at the points V, R, will converge to some point F in the rarer medium: and if the light be received on a screen at F, it will be colourless; if nearer to the refracting medium, or farther from it, it will be tinged, but on different sides.

Thus if the denser medium is water, and the surrounding medium is air; the angle of incidence LOS being 20° , the angle of divergence VOR will be $7' 46''$. And OVP the angle of incidence at the second refraction for the violet rays being taken of 30° , the angle of convergence RFY will be $14' 26''$.

On the contrary, if the plane VRt, (Fig. 4.) which terminates the denser medium cuts the first refracting plane

plane on the other side of the perpendicular OL, the rays will diverge from some point f on the other side of the second surface : the violet ray OV being more refracted from the perpendicular VP, than the red is from the perpendicular Rp.

And it is evident, that if the distance (OT or O_t) of the point of incidence from the edge of a prism, the angle of incidence LOS, and the angle of the prism (OTV or $O_t V$) are given, together with the refractive powers of the media, the lines OV, OR, will be given in magnitude and position. And thence the distance VR being given, with the angles of refraction at the second surface, the points, F or f , to which the rays converge, or from which they diverge, will be given. And their locus, or the Curve in which all these points are found, may be assigned ; whether the angle of the prism is constant, and the angle of incidence is variable, or the contrary ; and whether the rays are refracted, or, at a certain obliquity, come to be reflected by the second plane.

10. If it is further required that the extreme, and all the intermediate, rays which meet at F (in Fig. 3.) should thenceforth remain united in a colourless pencil : through the point of convergence F draw (by the lemma) the line ZX, making the angles ZFR, ZFV, such that their difference RFV being the given angle of convergence, their sines may be as the sines of refraction of the red and violet rays, when they pass from a given denser medium GKH into the air, at a common angle of incidence : and HFG perpendicular to ZX will be the line in which the surface of that medium must cut the plane of refraction, when the rays RF, VF, are refracted into the same

same line FN. And if the medium be terminated on the other side by any plane KN to which FN is perpendicular, the pencil NY, continued in the air, will remain colourless.

For instance, if the medium GK is glass, and the angle RFV is $14' 26''$, ZFR the angle of incidence of the red rays will be found of $17^\circ 54' 14''$; and the angle of refraction XFN, common to all the rays, will be $12^\circ 6' 34'' \frac{1}{4}$.

But if the plane HG, to which ZX is perpendicular, passes not thro' the point of reunion F, but on this or the other side of it; the rays in their passage thro' the medium, though parallel to each other, will be laterally separated.

11. Let a ray SOL (Fig. 5.) of a mean degree of refrangibility be refracted by AB the side of a glass prism ABC, so that the refracted ray OM may be perpendicular to the side of the prism AC; it is required to apply to this another prism of a differently refracting substance, as of water, so that the ray Mo being refracted at o, by the side DC, the refracted ray so may be parallel to OS.

The angle of incidence SOP, and the refractive power of the glass being given, the angle SOM, and its supplement LOM, are given produce Mo to n; and because os is to be parallel to LO take for the difference of the angles in the lemma, the given angle nos ($=LOM$), and through the point o draw rop, so that the sine of pon may be the sine of pos, as the sine of incidence to that of refraction, when a meanly-refrangible ray passes from water into air; and DoC, perpendicular to rp, will be the position of the side required.

¶

We



h u g e f a f t
b a g n i n g
t o p k u d
f h y g

A Plate of Sig: Abate Venuti's copy of the

We have here supposed the ray SO to be homogeneous, of a mean refrangibility; but if it is a ray from the Sun the image at s will be very much tinged. The colours will have been separated at O; a small matter more at M, but they will diverge very considerably at o; for setting aside the refractions at O and M; that is, supposing a pencil Mo to pass unrefracted in water till it falls upon a surface of air at an angle of incidence of about $47^{\circ} 32' \frac{1}{4}$, the divergence of the extreme rays will be about $2^{\circ} 51' \frac{1}{2}$: a small difference of sines answering to a considerable difference of the angles when they approach to 90° : the ultimate difference to which they converge, being (from water into air) $7^{\circ} 26' \frac{1}{4}$.

12. Let a pencil of the solar light SO (Fig. 6.) fall upon the surface of water BC, the extreme rays being refracted into OV, OR; it is required to assign the glass prism PN π (whose section PN π is an isosceles triangle) such, that the base N π being parallel to SO, and the surface of the water AC being inclined to the base N π in the same angle as the surface BC; the extreme rays, in their passage through the glass prism, shall be parallel; and all the rays shall emerge colourless in the line SO os; that is, in the incident ray produced thro' both the media.

The angle SOB, and the refractions from air into water, being given, the angles VON, RON, and their difference VOR, are given. Draw therefore, by the lemma, the line OG, making the sine of ROG to that of VOG, as the sine of refraction of a red ray, in passing from glass into water, is to the sine of refraction of a violet ray, their angles of incidence being equal, and PN perpendicular to OG will be the intersection

of

of the plane of refraction with the side of the prism that is required.

Thus the angle $\angle SOB$ being 30° , $\angle VOR$ will be $18' 12'' \frac{1}{2}$, $\angle VOG = 50^\circ 38' 4'' \frac{1}{2}$, $\angle ROG = 50^\circ 19' 52'' \frac{1}{2}$, whose sines are as the sines of refraction of the violet and the red, in passing from glass into water at a common angle of incidence. And therefore, the angles of the emergence of the rays OV , OR , in passing from water into glass will be equal, that is Vv will in its passage through the glass prism, be parallel to Rr , and the rays meeting with equal and contrary refractions at the points v , r , o , as they suffered at V , R , O , will emerge colourless at o .

Yet we must not be surprised if the pencil os is not absolutely pure light (even supposing, the matter, the figure, and the disposition, of the media to be faultless) because (1°) perhaps the refractive powers have not been determined with sufficient exactness (2°). If the glass plate which contains the water be not very thin, the light will have received a slight tincture in passing through it at O : This however may be remedied by confining the water between two glass prisms. And (3°) it is scarce possible to make experiments of this kind with a pencil of light so slender as the theory prescribes (see § 2.)

But proper allowances being made on these accounts, and the refracting planes adjusted as the lemma directs, the light will emerge sufficiently pure to justify the theory. And the refractions of either medium being given, it will appear from the experiments whether those of the other medium have been determined with sufficient accuracy.

Observe

Observe likewise, that as, in practice, we must fit the water to the glass, not the glass to the water, we are to begin by assuming VR of a convenient magnitude; and supposing the rays Vv , Rr , &c. to be parallel within the glass, find the point O to which they converge in the water, through which a plane may be drawn which shall send them out into the air, in a colourless pencil OS.

R E M A R K S.

I.

The 8th experiment in Sir Isaac Newton's optics (Book I. Part 2.) seems to have been made under the conditions which are limited by the foregoing problem; though he does not specify these conditions. For, it is to be presumed, he did not combine his prism and water at random, but adjusted them so as to produce the expected effect. It is observed likewise, that he does not give us a description of his experiment so particular as, in most instances, he was wont to do. He thought perhaps that the consequences he deduces from it might sufficiently explain his meaning; especially as he had, in the foregoing propositions, fully established the principles of his theory.

However this be, several persons of skill and address in optical matters, have produced experiments in contradiction to that of Sir Isaac, and have affixed meanings to his conclusions which he never could intend, without being grossly inconsistent with himself: an

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imputation from which common candor and decency ought to have protected so great a name *.

For instance, when he says that "light as often as by contrary refractions it is so corrected that it emergeth in lines parallel to those in which it was incident, continues ever after to be white"; can this assertion possibly bear the meaning they would obtrude upon us? Had Sir Isaac so entirely forgot his own doctrine as not to know, That if the glass prism PN_n, in the last scheme, is, any where above Vv, terminated by a plane to which the pencil SO is perpendicular, the rays Vv, Rr, &c. though emerging parallel to SO, will exhibit their several colours? The sense therefore which the experiments affix to Sir Isaac Newton's words being so absurd, had not they done better to look out for one that was consistent with his theory? and such a one they would have found by only drawing a figure like the foregoing; where the rays of the pencil, reunited in o_s, as well as when separated within the glass prism, are parallel to each other and to the incident pencil. But, if the water is terminated by a plane different from AC, passing through the point o, and making the rays (no longer parallel to SO) to diverge, then the light will, by degrees, in passing on from o, become coloured: which is Sir Isaac's other position.

To this meaning his own words ought to have led the objectors. It was light, not separate rays, which

* The reader ought to be told, that it is not here intended to detract from the merit of the late Mr. Dollond's improvement of refracting telescopes; but only to correct a mistake of his concerning that difference of dispersion of rays, which he has so happily applied to use.

emerged

emerged in his experiment; and which (being parallel to the incident light) continued to be colourless.

He adds farther, "the permanent whiteness argues, that in like incidence of the rays, there is no separation of the emerging rays": as much as to say, that in his experiment (as in our 6th Figure) the pencil, in passing or repassing, is supposed to meet with surfaces of equal refractive powers, similarly situated.

The other cases in which refracted light may recover its whiteness, although it emerges not parallel to the incident, or may be tinged though parallel to it, Sir Isaac does not treat of: the experiment he had made, being sufficient for the purposes to which he applies it. But he assures his readers, that if they will argue truly upon his theory, trying all things with good instruments, and sufficient circumspection, the expected event will not be wanting. And the fact is, that in all the experiments which have been made, if none of the necessary data are wanting, the appearance of the emerging light may be certainly predicted.

II.

When a slender pencil of light is refracted at the surface of any medium, the extreme rays, the violet and red, and the several intermediate rays, each of its particular degree of refrangibility, will all diverge from, or converge to, the same physical point: or when that point, by altering the position of the plane, is thrown to an infinite distance, will all of them become parallel. And it appears from the foregoing solution, that such parallelism may always be effected,

C c 2

whatever

whatever be the refracting power of the medium PNz , provided that, in a given medium, the quantities m , n , &c. of the lemma, which represent the sines of refraction of the several sorts of rays, to a common sine of incidence, continue to be in constant ratios to one another.

Conversely, if, from experiments such as that which Sir Isaac Newton made, it follows that, whatever be the refractive powers of the media, and the angle of incidence of the light, the pencils SO , sa , may be made to reciprocate with each other, while all the sorts of rays, in passing or repassing through the prism PNz , become parallel; if, I say, this is confirmed by experiments, it is a proof that, for any given medium, the ratios of those quantities m , n , &c. are invariable.

III.

And hence Sir Isaac deduces the two theorems subjoined to his 8th experiment; by the first of which he contrives to make the ratios of the sines of refraction belonging to the several sorts of rays, to a common sine of incidence, when they pass from glass into air, to serve for finding the like ratios for the rays passing from water into air, without the trouble of new experiments.

His first theorem may be deduced in this manner:

Let all the sorts of rays, whether united in a pencil of light, or separated parallelwise by refraction, have the same angle of incidence whose sine is I , when they pass from a denser into a rarer medium; and let V and R stand for the sines of refraction of the

the extreme (or any two sorts of) rays. Then seeing by the experiments, the ratio of V to I is given, as also that of R to I ; the ratio of $V-I$ to I , as also (invert.) that of I to $R-I$, and (exæquo) that of $V-I$ to $R-I$, are given: for this last write the ratio of I to p .

In like manner, let the refractive power of the medium from which the rays emerge into the same medium as before, be increased or diminished, as also the common angle of incidence; and we need only write other marks v and r for the fines, and i for the common fine of incidence; for we shall have as before $v-i$ to $r-i$ in a given ratio; which call that of I to q . And, from these two, we have $\frac{V-I}{v-i} = \frac{R-I}{r-i} \times \frac{q}{p}$. But p is always nearly equal to q ; in the refractions from glass, and from water into air, their difference is less than $\frac{1}{400}$ part of either; we may therefore put the ratio $\frac{V-I}{v-i}$ equal to $\frac{R-I}{r-i}$; which is the first theorem.

And thence, if one difference $R-I$ becomes equal to $r-i$, the other differences $V-I$, &c. will be respectively equal to $v-i$, &c. and the same set of differences may be made to serve for several media, provided the fines of incidence are taken in their due proportion.

Thus when red rays pass from glass into the air, we have $I : R :: 50 : 77$ and $R-I : I :: 27 : 50$, and when they pass from water into air $i : r-i :: 3 : 1$, and therefore, as we are to make $R-I$ every where equal to $r-i$, we get, ex æquo, $i : I :: 81 : 50$, as Sir Isaac Newton finds it.

IV. But

VI.

But to explain this matter a little farther, and obviate some difficulties concerning it, I shall add the following

E X A M P L E S.

The refractive powers being as marked above, let red rays fall from glass into air at the angle of incidence 20° , the angle of refraction will be $31^\circ 47'$.

Again, let them fall from water into air at an angle of $34^\circ 1'$, making their angle of refraction $48^\circ 5'$.

And the difference of the sines of $31^\circ 47'$, and 20° will be precisely equal to the difference of the sines of $48^\circ 5'$, and $34^\circ 1'$.

At the same angles of incidence 20° and $34^\circ 1'$, let the violet rays fall from glass and water into air; and the angle of refraction from the glass, will be $32^\circ 14\frac{1}{2}'$ nearly, and that from the water will be $48^\circ 38'$ nearly. And the difference of the sines of $32^\circ 14\frac{1}{2}'$ and 20° will equal the difference of the sines of $48^\circ 38'$ and $34^\circ 1'$, within .000488, or less than $\frac{1}{2000}$ th part.

We see likewise that the red and violet rays diverged from the glass medium at an angle of $27\frac{1}{2}'$; but from the water at an angle of $33'$; making the difference of divergence in this example $5\frac{1}{2}'$; that is $\frac{1}{3}$ of the whole divergence of the red and violet rays when refracted from glass into air, at incid. 20° .

Whence

Whence it appears, that although the differences of fines above specified, or the excesses in Sir Isaac's theorem, may, in refractions from different media into the same rarer medium, be made equal, it does by no means follow, that the divergences of the several sorts of rays (or if you chuse to call it their dispersion) will be the same in the two refractions; for Sir Isaac's excesses 27 , $27\frac{1}{2}$, &c. are the excesses of fines; not of angles, as some opticians seem to have misapprehended.

Again, let an unrefracted pencil of light fall from common glass into the air (Fig. 7.) at the incidence 39° , and the angles of refraction will be,

| | | |
|---------------|---------|-----------------------|
| Of the violet | - - - - | $79^{\circ} 2' 2''$ |
| Of the red | - - - - | $75^{\circ} 43' 55''$ |

And their difference - $3^{\circ} 18' 7''$ is the divergence of the extreme rays.

And the angle of refraction of the mean ray is $77^\circ 16' 19''$.—By *mean ray* is understood the ray whose fine of refraction is a geometrical mean between the fines of refraction of the extreme rays, the common radius being unity.

Let now the same rays be refracted the contrary way by a surface of water WT, then, to make the mean emergent ray parallel to the incident pencil, its angle of incidence must be $86^\circ 37'\frac{1}{2}$: and the extreme rays will now converge at an angle of $20\frac{1}{8}$ minutes, nearly.

Through the point of convergence o, draw (by the Lemma) a plane wt, to terminate the water, and unite all the rays into a colourless pencil os: and this emergent.

emergent pencil will be found to make with a perpendicular to the terminating surface an angle of $49^{\circ} 6' \frac{1}{4}$, and will be inclined to the first incident pencil in an angle of 14 degrees, 28 minutes, 20 seconds. Nor is there any other plane besides this which will thus unite the rays. If planes parallel to it cut the rays any where but in their point of convergence, they will be parallel to each other, but exhibiting their several colours. And planes not parallel to it, will every where give a coloured image, excepting only when they pass through the point of convergence; but then the rays having cross'd at that point, will thenceforth diverge from one another, and give a coloured spectrum.

From all which it appears that light refracted thro' different media may emerge colourless, although its first direction be considerably altered. And that its mean direction may remain the same, though its extremities be sensibly tinged with colours. Positions which, I know not by what mishap, have been deemed paradoxes in Sir Isaac Newton's theory of light, but which are really the necessary consequences of it.

Of Telescopical Object-Glasses giving an Image free from Colours, Fig. 8 and 9.

If the extreme rays, the red and violet, after one or more refractions, diverge from points D and d, the distance of the point of divergence of the least refrangible from the lens, being greater than that of the most refrangible, such a semidiameter of the last spherical surface, from which they are to pass into the

the air, may be assigned, as shall unite the extreme, and all the intermediate, rays in the same focus F; neglecting the aberration from the figure.

The R U L E is this;

For the distances of the points of divergence from the lens, write D the greater, and d the least; the semidiameter of any of the given surfaces being assumed for unity: And $\frac{M}{r}$, $\frac{m}{r}$ expressing the ratios of the sines of incidence and refraction of the violet and red rays out of air into the last medium whose surface is required: the semidiameter of that surface will be $\frac{M-m \times Dd}{MD-md}$; as may be easily demonstrated from a theorem of Dr. Smith, in the remarks subjoined to his Optics.

Thus if the last medium is glass, the semidiameter of the surface from which the rays pass into the air, must be $\frac{Dd}{78D-77d}$, it being, in this case, $\frac{M}{r} = \frac{7}{5}$, $\frac{m}{r} = \frac{77}{50}$.

EXAMPLE I.

Let M_pCN_cM (Fig. 8.) be a double convex lens of water confined between the plano-concave $MTLN$, and the meniscus MKN_cM , both of glass, and having the radii of their surfaces contiguous to the water, equal to each other, or to unity: and if a ray S_p , parallel to the common axis of the lenses, after being refracted by the aqueous lens, have its

VOL. LIII. D d extreme

extreme rays, the red and violet, divergent from the points D and d ; the distance of F, the focus where all the rays can meet, will be 8.898 : and when this happens, the exterior surface of the meniscus, that is, the surface represented by MPKN, will have its radius to that of the inner surface McN, as 139 to 154.

EXAMPLE II.

When a double concave of glass (the radii of whose surfaces are unity) is inclosed in water, as in Fig. 9, the water being confined on one side by a thin glass plate TL, and on the other by a concentric spherical shell MPKN; the semidiameter of this shell must be to unity as 471 to 547: and the focal distance CF, at which the colourless image is formed, will be $4.77\frac{2}{3}$. In these examples the thickness of the lenses is neglected; but it may easily be taken into the account, if it is thought necessary.

The same thing may be effected by means of any media of different refractive powers: for the semidiameter of the last refracting surface being determined according to the foregoing rule, the nearer distance of the points of divergence (d) of the more refrangible rays will be so compensated by their greater refrangibility, that all the rays will converge to the same focus F. And this without introducing any new principle into the science of optics, or any dispersion of light different from the refractions discovered by Sir Isaac Newton near a hundred years ago.

XXXII. An Account of the Success of the Bark of the Willow in the Cure of Agues. In a Letter to the Right Honourable George Earl of Macclesfield, President of R. S. from the Rev. Mr. Edmund Stone, of Chipping-Norton in Oxfordshire.

My Lord,

Read June 2d,
1763.

AMONG the many useful discoveries, which this age hath made, there are very few which, better deserve the attention of the public than what I am going to lay before your Lordship.

There is a bark of an English tree, which I have found by experience to be a powerful astringent, and very efficacious in curing aguish and intermitting disorders.

About fix years ago, I accidentally tasted it, and was surprised at its extraordinary bitterness; which immediately raised me a suspicion of its having the properties of the Peruvian bark. As this tree delights in a moist or wet soil, where agues chiefly abound, the general maxim, that many natural maladies carry their cures along with them, or that their remedies lie not far from their causes, was so very apposite to this particular cafe, that I could not help applying it; and that this might be the intention of Providence here, I must own had some little weight with me.

The excessive plenty of this bark furnished me, in my speculative disquisitions upon it, with an

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argument

argument both for and against these imaginary qualities of it ; for, on one hand, as intermittents are very common, it was reasonable to suppose, that what was designed for their cure, should be as common and as easy to be procured. But then, on the other hand, it seemed probable, that, if there was any considerable virtue in this bark, it must have been discovered from its plenty. My curiosity prompted me to look into the dispensaries and books of botany, and examine what they said concerning it ; but there it existed only by name. I could not find, that it hath, or ever had, any place in pharmacy, or any such qualities, as I suspected ascribed to it by the botanists.

However, I determined to make some experiments with it ; and, for this purpose, I gathered that summer near a pound weight of it, which I dried in a bag, upon the outside of a baker's oven, for more than three months, at which time it was to be reduced to a powder, by pounding and sifting after the manner that other barks are pulverized.

It was not long before I had an opportunity of making a trial of it ; but, being an entire stranger to its nature, I gave it in very small quantities, I think it was about twenty grains of the powder at a dose, and repeated it every four hours between the fits ; but with great caution and the strictest attention to its effects : the fits were considerably abated, but did not entirely cease. Not perceiving the least ill consequences, I grew bolder with it, and in a few days increased the dose to two scruples, and the ague was soon removed.

It was then given to several others with the same success ; but I found it better answered the intention, when a dram of it was taken every four hours in the intervals of the paroxysms.

I have

I have continued to use it as a remedy for agues and intermitting disorders for five years successively and successfully. It hath been given I believe to fifty persons, and never failed in the cure, except in a few autumnal and quartan agues, with which the patients had been long and severely afflicted; these it reduced in a great degree, but did not wholly take them off; the patient, at the usual time for the return of his fit, felt some smattering of his distemper, which the incessant repetition of these powders could not conquer: it seemed as if their power could reach thus far and no farther, and I did suppose that it would not have long continued to reach so far, and that the distemper would have soon returned with its pristine violence; but I did not stay to see the issue: I added one fifth part of the Peruvian bark to it, and with this small auxiliary it totally routed its adversary. It was found necessary likewise, in one or two obstinate cases, at other times of the year, to mix the same quantity of that bark with it; but these were cases where the patient went abroad imprudently, and caught cold, as a post-chaise boy did, who, being almost recovered from an inveterate tertian ague, would follow his business, by which means he not only neglected his powders, but, meeting with bad weather, renewed his distemper.

One fifth part was the largest and indeed the only proportion of the quinqua made use of in this composition, and this only upon extraordinary occasions: the patient was never prepared, either by vomiting, bleeding, purging, or any medicines of a similar intention, for the reception of this bark, but he entered upon it abruptly and immediately, and it was

was always given in powders, with any common vehicle, as water, tea, small beer and such like. This was done purely to ascertain its effects; and that I might be assured the changes wrought in the patient could not be attributed to any other thing: though, had there been a due preparation, the most obstinate intermittents would probably have yielded to this bark without any foreign assistance: And, by all I can judge from five years experience of it upon a number of persons, it appears to be a powerful absorbent, astringent, and febrifuge in intermitting cases, of the same nature and kind with the Peruvian bark, and to have all its properties, though perhaps not always in the same degree. It seems likewise to have this additional quality, viz. to be a safe medicine; for I never could perceive the least ill effect from it, though it had been always given without any preparation of the patient.

The tree, from which this bark is taken, is styled by Ray, in his *Synopsis*, *Salix, alba, vulgaris*, the common white Willow. *Hæc omnium nobis cognitum maxima est, et in fatis crastam et proceram Arborem adolefecit.*

It is called in these parts, by the common people, the willow, and sometimes the Dutch willow; but, if it be of a foreign extraction, it hath been so long naturalized to this climate, that it thrives as well in it as if it was in its original soil. It is easily distinguished by the notable bitterness and the free running of its bark, which may be readily separated from it all the summer months whilst the sap is up. I took it from the shoots of three or four years growth, that sprung from Pollard trees, the diameters of which shoots,

shoots, at their biggest end, were from one to four or five inches: it is possible, and indeed not improbable, that this cortex, taken from larger or older shoots, or from the trunk of the tree itself, may be stronger; but I have not had time nor opportunities to make the experiments, which ought to be made upon it. The bark, I had, was gathered in the northern parts of Oxfordshire, which are chiefly of dry and gravelly nature, affording few moist or moory places for this tree to grow in; and therefore, I suspect that its bark is not so good here as in some other parts of the kingdom. Few vegetables are equal in every place; all have their peculiar soils, where they arrive to a greater perfection than in any other place: the best and strongest Mustard-seed is gathered in the county of Durham; the finest Saffron-Flowers are produced in some particular spots of Essex and Cambridgeshire; the best Cyder-apples grow in Herefordshire, Devonshire and the adjacent counties; the roots of Valerian are esteemed most medicinal, which are dug up in Oxfordshire and Gloucestershire: And therefore why may not the Cortex Salignus, or Cortex Anglicanus, have its favourite soil, where it may flourish most, and attain to its highest perfection? It is very probable that it hath; and perhaps it may be in the fens of Lincolnshire, Cambridgeshire, Essex, Kent, or some such like situations; and, though the bark, which grew in the county of Oxford, may seem in some particular cases to be a little inferior to the quinquina, yet, in other places, it may equal, if not exceed it.

The powders made from this bark are at first of a light brown, tinged with a dusky yellow, and the longer they are kept, the more they incline to a cinnamon

cinnamon or lateritious colour, which I believe is the case with the Peruvian bark and powders.

I have no other motives for publishing this valuable specific, than that it may have a fair and full trial in all its variety of circumstances and situations, and that the world may reap the benefits accruing from it. For these purposes I have given this long and minute account of it, and which I would not have troubled your Lordship with, was I not fully persuaded of the wonderful efficacy of this Cortex Salignus in agues and intermitting cases, and did I not think, that this persuasion was sufficiently supported by the manifold experience, which I have had of it.

I am, my Lord,

with the profoundest submission and respect,

Chipping-Norton, your Lordship's most obedient
Oxfordshire,
April 25, 1763. humble Servant

Edward Stone.

XXXIII. *An*

**XXXIII. An Account of an Earthquake in
Siberia: In a Letter from Mons. Wey-
marn to Dr. Mounsey, Principal Phy-
sician of the Emperor of Russia, F. R. S.
Translated from the French. Communi-
cated by Mr. Henry Baker, F. R. S.**

Read June 16, 1763. I Cannot express the excess of joy and compleat satisfaction, with which I heard, by our friend Dr. Erasmus, and the Reverend Mr. Minau, and soon after by the Petersburgh Gazette, the pleasing and long expected news, that his Imperial Majesty, our most gracious Sovereign and Master, has been pleased to confer on your Excellency the Office of Archiater, and Supreme Head of the Medical Faculty, throughout the whole Empire, with the rank and dignity of a Privy Counsellor.

As I suppose your Excellency has received my last letter, as also the skin of a monstrous lamb; and not doubting but you will be glad to collect other curiosities of this country, I shall not fail to send you, by the first opportunity, several pieces, with proper remarks, on different subjects relating to the natural history and geography of these regions. In the mean time, I have the honour to send you inclosed, an account of an earthquake we felt, on our frontier lines, in the month of November last year; and, tho' these accidents are no uncommon thing here, yet I think it deserves our attention, considering the circumstances it has been attended with, which has

induced me to subjoin my own reflexions, which I take the liberty to submit to your Excellency's judgment, and humbly beg your opinion of them, if your important avocations will allow you time.

It is a great concern to me, that the immense labours of my very burdensome and fatiguing post will not allow me to follow my inclination for the study of nature, and for curious and useful enquiries into the physical sciences, which would enable me to satisfy the desire I know your Excellency has to acquire a particular knowledge of the properties and produce of this country, which well deserve the attention of the learned, and would require an abler hand and more leisure than I am master of.

The more I examine this country, the more I find it worthy of the closest attention. The air and most of the waters are excellent, the soil is fruitful, and produces all that can be imagined. With a little more application and industry, and if the inhabitants would divest themselves of their old prejudices, it might easily be made a most delightful and wealthy country.

Your Excellency's time is too well employed to be wasted in reading voluminous epistles, wherefore I shall put an end to this letter, that has already taken up too many of your moments; but cannot conclude without renewing the protestations of the sincere and inviolable respect and attachment, with which I have the honour to subscribe myself,

Sir, Your Excellency's

From Fort Omsk,
March 26th,
1762.

Most obedient humble Servant,

W. W. Weymarn.

The

THE weekly papers are filled with all the remarkable events, which happen in all the known and inhabited parts of our globe, altho' they are neither extraordinary nor uncommon, either with regard to the productions and effects of nature, or the places where they happen. These laudable endeavours to impart whatever may be unknown, or but little known as yet, to the generality of the world, are useful helps towards getting an insight into the various works of nature, and the promoting of arts and science in general, as they put ingenious and learned men, and lovers of sciences, upon searching into the causes and effects of natural events: in order to improve such as may prove beneficial to mankind, and likewise to find out the means of preventing or removing such as may be hurtful: And should those communications be productive of neither of these advantages, they would at least serve to make us more acquainted with the countries and places where those things happen. Hitherto it does not appear that any thing of this kind has been published relating to Siberia, a vast and rich tract, abounding in all kinds of natural productions, and well worthy the notice of the learned and curious. But this scarcity of news from Siberia seems to be rather owing to the inattention of the inhabitants than the neglect of the news writers. The times of indolence and inattention seem however to be now at an end even in Siberia, from whence we have the following account of an Earthquake, which was felt there on the 28th of November last (old stile) in the evening, towards the frontier lines on the side of Zengoria. The shocks were felt

E e 2

at

at the same instant to the extent of above a thousand versts *. The inaccuracy of this account, and the omission of minute circumstances, must be imputed to these people's being unaccustomed to make or describe any observations. However we shall relate it such as it is.

From Fortin Nowikowski, the last but one on the line of Kusnetsk, to the Eastward. Oct. 24, 1761.

The day before yesterday, Oct. 22d, at one in the afternoon, a noise was heard under ground, which, tho' of a short duration, was pretty distinctly observed by the whole garrison, and particularly by those whose houses stand without the walls of the Fort. This subterranean noise, whilst it lasted, was attended with a trembling of the earth, which only shook the timber-houses. The next day, at four in the morning, it lightned as in summer, but this did not last long.

Fort of the Mines of Koliwan, situated on the Line of the same name, adjoining to that of Kusnetsk, Nov. 30, 1761.

The 28th instant, between 7 and 8 in the evening, we felt an Earthquake, which begun by a subterranean noise. Its course was from East to West. The shocks were not so violent as to do any damage,

* N. B. A Russian Verst is. 1166 $\frac{2}{3}$ English yards.

and

and but slightly shook the houses. This Earthquake lasted but three minutes. On the same day, at the same hour, and with the same circumstances, this Earthquake was felt at Fort Czagirsk, and at the Redoubt of Inesk, both on the Line of Koliwan, but with this difference, that not only the houses, but also the bastions, and even the timber tower at Czagirsk, were shaken, but no damage ensued,

Fort Ust Kamenogorski, situated at the Southern extremity of the Line of Irtisch, and on the Eastern bank of that river. Nov. 30. 1761.

The day before yesterday, between 7 and 8 in the evening, was heard a subterranean roaring noise, like that of a very violent storm: and soon after were felt such violent shocks of an Earthquake, for the space of about 20 minutes, that several wooden houses were removed from their places: and the green turfs, that the roofs are covered with, were cracked and dropped off. Water, that stood in pails and other wooden vessels, was spilt on the ground. The rumbling noise was distinctly observed to come from the East, and to extend toward the North. The same thing was likewise observed in all the fortines and redoubts dependent on Fort Ust Kamenogorski, situated lower down the banks of the Irtisch.

From.

From Fortin Schoulbinsk, situated on the banks
of the Irtisch, 125 versts from Ust Kamenogoriski, Nov. 30. 1761.

It was the day before yesterday, between 7 and 8
in the evening, that, without hearing any noise under
ground, we felt the Earthquake here, which lasted
but about two or three minutes, and did no other
mischief than shaking the houses a little. Its direc-
tion seemed to be from South to North.

Fort Sempalat, near the Irtisch, 206 versts from
Ust Kamenogoriski, Dec. 1. 1761.

On Wednesday last, Nov. 28, some officers hav-
ing met at my house to spend the evening, between
7 and 8 we felt the bench on which we were sitting
shake several times pretty violently; and, thinking
at first that some of the company did it in sport, we
began to chide one another; but, being at length con-
vinced that the motion proceeded from an Earthquake
that shook the whole house, and made the beams
and doors crack, every one hasted to the door, to
escape the danger they apprehended from the falling
of the house. We were scarce got out, but we heard
the centry, who was upon duty on the top of
the timber tower, call out, that the whole tower was
shaking, as well as all the other works of the for-
tification. However, we were soon delivered from
our

our fears, the Earthquake having lasted but about 12 minutes, without doing any other damage than throwing down and breaking some earthen ware here and there. Upon my return home, I found my books tumbled off the shelf and lying on the ground, as did likewise my ink-bottle that stood upon the table. As the shaking of the houses was observed to be from East to West, it is to be conjectured that the direction of the Earthquake, or rather the kindling of the subterranean combustible matter, was from South to North, as some pretend to have expressly observed. Just as the post is going out, we have an account that this Earthquake was felt at Fortin Glouchowiskoi, as also at the Redoubt of Pjanojarsk, with the same circumstances, and at the same time, as here.

Fort Jamischeff, on the banks of the Irtisch, 460 versts from Ust Kamenogorski, Dec. 3. 1761.

The Post from Sempolat, and other places higher up the Irtisch, as likewise that which is come in at the same time from the Line of Koliwan, having brought us an account of a violent Earthquake that was felt on the 28th of last month, not only on the line of Irtisch from Ust Kamenogorski, but likewise on those of Kusnets'k and Koliwan, we must credit the observations made here, by numbers of people, of an Earthquake on the 28th of November between 7 and 8 in the Evening, which, tho' it lasted near 12 minutes, was so slight as not to occasion the least damage, but only a gentle motion hardly to be felt.

Extract

Extract of a Letter from the Foundery at Bar-naoul, Feb. 9. 1762.

I here send you an account of the Earthquake, which was felt here on the 28th of November last year. At above half an hour after 7 that evening, the air being dense, calm and quite still, an undulating motion was felt, like that of large and high waves, which continued for some minutes, and was immediately succeeded by the Earthquake, with such violent shocks, that the beds, chairs, tables, and other household goods, were removed from their places and thrown about the rooms. The shaking of the houses was very strong. Its direction was from South-West to North-East. Some persons passing, at that instant, over the great dyke, before which are the melting furnaces, have reported that they heard a loud noise, like that of the great hammers when they are all employed in the works.

I beg leave to add, to these several accounts, some reflexions, relating to the origin, progress, and effects of this common and well known phænomenon, which all parts of the world are liable to.

i. The ridge of mountains, called Altaiskoi Chrebet, or Chain of Altai, from Lake Teletskoi to the Eastern bank of the Irtisch, covers all that part of the frontiers of Siberia towards the South, which lies between the said Lake and the river Irtisch, and extends from East to West, and

So goes on beyond the Irtisch, in the same direction, thro' the country of Zengoria.

2. These mountains abound with all sorts of minerals; particularly that part which borders on the river Dgelo, which runs from the West into the river Katunja, is all full of a kind of Saltpetre, which is found in form of a cement, in great plenty, in the clefts and between the beds of rock; with this the Tartars and Kalmucks make very strong and good Gunpowder, by an industrious, simple and expeditious method.

3. This place is situated almost Eastward of Fort Ust Kamenogorski, from whence they seem to have given the most exact account of the Earthquake. The inhabitants, being accustomed to these events, which happen there almost every year, must be better able to trace its origin, progress, and effects than those of other places.

4. If the combustible matter took fire at first in the places mentioned in the second article, and if it may be conjectured that in this ridge of mountains, infinitely more combustible matter may be contained than in the flat country, without any interruption; the direction of the Earthquake must undoubtedly have followed the course of the ridge of mountains, that is to say, from East to West, till it was interrupted by invincible obstacles.

5. According to advices just received from the Kirgiss Kaisacks, who inhabit the parts beyond the Irtisch, they have had no Earthquake, neither on the 28th of November, nor for a long while before or after; and, as it came in a direct line from the East

to Ust Kamenogorski, and did not pursue its first direction from East to West, but turned off to the North, as appears from the accounts from the Forts Schoulbinsk and Semipalatnaja and others, its course must have been interrupted in its way, by some unsurmountable obstacle, towards Ust Kamenogorski. This obstacle seems to have been no other than the river Irtisch, which runs from South to North, whence it follows too, that the inflamed matter did not lie so deep as the bottom of the river, as it would otherwise have followed the direction of the ridge of mountains that extends towards the West.

6. The account from Barnaoul seems to confirm this opinion, and shews that the deviation of the Earthquake happened near Fort Ust Kamenogorski, which lies directly to the South West of Barnaoul.

XXXIV.

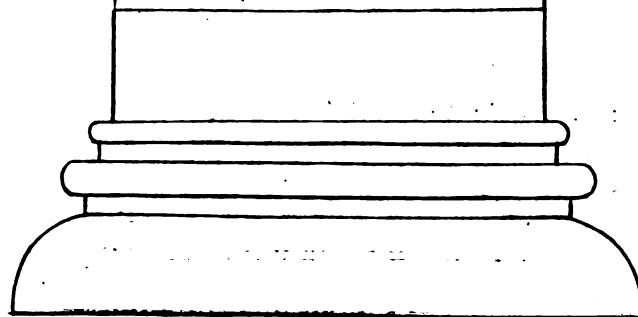
*Roman Inscriptions at Tunis in Africa, copied about the Year 1730. by
Dr. Carlos, a native of Madrid, then Physician to the Bey of Tunis,
communicated by John Locke, Esq. F.R.S.*

*Memoriae Antiquitatum qui in hoc
Tunetano Regno Inveniuntur.*

Tunetis hodie hec reponiuntur

*In duabus columnis, qua in taberna Cara Mamet
a Turcis, a Christianis vero & Leonardis vocataatur
hec memoria Adriani Imperatoris reperitur*

IMP. CAESAR
DIVI NERVE NEPOS
DIVI TRAIANI PARTHICI. E.
TRAIANVS HADRIANVS
AVG. PONT. MAX. TRIB.
POT. VII. COS. III.
VIAM A CARTHAGINE
THEVESTEM STRAVIT.
PER LEG. III. AVG.
P. METILIO SECVNDO
LEG. AVG. PR. PR.



*In duobus epistylis Domus Dei, quæ porticibus
Substant, hæc sepulchralia epitaphia
Leguntur.*

D. M. S.
ANARIA
FRVTA
CONIVX
CARISSIMA
PIAVIXIT
ANNIS XXXI
ESTO BOTI.

D. M. S.
APRONIA
VICTORIA PIA
VIXIT ANNIS XLIX

IMP. CAE S. C. VIBIUS
TREBONIANVS GALLVS
PIVS FELIX AVG. PONT.
MAX TRIB. POTEST.
COS. II JE SIC. PP.
ET IMP CAE S. C.
AFINTVS.....
VOLVSLANVS.....
PIVS FELIX. AVG
PONTIF. MAX. TRIB.
POTEST. II COS
DESIC.
PP. PROCONS
RESTITVERVNT.
XX.

*Inscriptio hæc legitur in quadam taberna, que ad
Trinitatis vinariam præstat in marmorâ columnâ
Casu ibidem deportata.*

J. Munde Sc.

In antiquæ Thuggæ ruinis adhuc hæc perstant, hodie
ab Arabibus corrupte Tucca.

| D. M. S. | | D. M. S. |
|-------------|----------------------------|-------------|
| C. MATTIVS | TIRINIVS FORTV | MAODIVS |
| PVLIALENVS | NATVS VIR ARMIS | VENIATERIA |
| BELLICVS P. | INGENIO ET ANIMO | NI FILIVS |
| V. A. L. V. | MAXIMO QVI CVM | P. V. M. XX |
| H. S. E. | ... NIS ET GRAECIS | H. Q. S. E. |
| | ... TIMIS H. F. T. P. | |
| | VIXITQVE LAETOS DVOS | |
| | ZOZIMOS IOVIS P.V.XXXIIII. | |

In Marmoreo sepulchro hoc carmen legitur.

Detrahe fæta comis et amorum apta tuorum,
Tristis inops nulla veste, Thalia, veni.
Non manus id alia improba virga,
Nec fiat ante tuos lucida palla pedes.
Iulius hoc feci mellitus qui vocor olim,
Cupito Patri, Matri venustæ meæ.
Me posui conjugem meam mihi Iuncia regatam,
Ut sit in æternum condita fama loci.

Viximus ad satiam, pietatem implevimus ambo,
Præstítimus sobolem fæmineam duplícem.
Vos quoque qui legitis versus, et facta probatis,
Discite sic vestros merito fancire parentes.
Vi te Gafriane, excolem, titulosque relinquam
Vivos, vi hoc facerem, fata dedere mihi.
Iulius hoc peto nunc à te, Dominator Averni,
Cum moriar manibus jaceant fossa quieta mihi.

Suffetula.

In quodam templo.

DIVI MARCI SACRVM

In lapide vero.
 M.C. LINARIO PROCONS
 REIPUBLICAE..ROMAE OB
 PIETATEM ET OBEDIENTIAM
 D D. P P.

In quodam lapide.

IMP CAES AVG - - - -
 - - - - -
 - - - - -
 SVFFETVLENTIVM HANC--
 -- AEDIFICAVERVNT.
 ET DD. PP. . . .

In Civitate Sicca Veneria, hodie *Chef.*

MERCVLIS SACRVM M. TITACVS PROCVLVS PROCV-
 RATOR AVGVSTI SVA PECVNIA FECIT.

In lapide hæc inscriptio.

VICTORI
 CFNTVRIONI
 LEGIONARIO
 EX EQVITE
 ROMANO
 OB MVNIFI
 CENTIAM ORDO
 SICCENSIVM
 - - CIVI ET
 ET CONDECVRIONI
 DD. P P.

In alio lapide.

IOVI OPT. MAX.
 CONSECRAVIT VI
 - - - - -
 SANTISSIMO
 PRINCIPI CAES.

In alio lapide;

SEXTO IVLIO GIMNAS
 TRIARCHO EIS VB - -
 PROFICII MESVI - -
 OPVLENTIAE ET ME
 LIVIO ORICVLONI. -

In Civitate Musta, quæ hodie Prædium Musti vocatur.

INVICTISSIMO FELICISSIMO QVE IMPERATORE IVLIO
 AVGUSTO CAESARI ORBIS PACATORI MVSTICENSIVM DD:

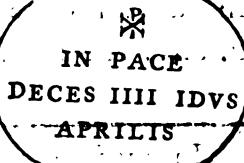
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|-------------------------|------------|-----------------------|
| DIIS MANIB. SAC. | D. M. S. | D. M. S. |
| ANONIVS FELIX PRON. | PATVLCIVS | C. MEMMIVS |
| ANIONIS ET PIVS VIXIT | PRIMVS | FELIX VIXIT |
| ANNIS XXIIII H. S. EST. | PIVS VIXIT | ANNIS |
| OT. EQ. II. L. 8. | ANNIS LXXX | LXXXII MENSIBUS. |
| | VHS BOTI | QVINQUE DIEBVS SEPTEM |
| | | H. S. E. |



| | |
|---------------------|-----------------|
| D. M. S. | D. M. S. |
| MARCVS POTITIVS AV | TVRIVS GEME |
| RELIANVS PIVS VIXIT | LIVS VXOR FILIO |
| ANNIS LXXXV | PIISSIMO FECIT. |
| O. T. B. Q. | CLODIVS VXORI |
| | DVLCISSIMAE |

In lapide rotundo



| | |
|--------------------------|------------|
| D. M. S. | D. M. S. |
| NVPTIALIS | COC. T. N. |
| VIXIT ANNIS XXI | IVS SOROR |
| H. S. B. Q. T. BOTI SIT. | EX P. VA. |

In magnis lapidibus hæc fragmenta inscriptionum
leguntur in diversis locis.

PATRAEEI CIVIVM SVORVM MERCVRIO
ATVIS MARMOREIS N. SEX SETO IRM SARMITI DIVI COMMODI FR
ET OMOMEMIO RVFO FORT ET ADNIPOTIS AVRELIV ANTONINI
RVNDATORVM REMVM - - - - - SEPTIMIVM.
TRI EORVM ET CAECII FAE. - - - - -

IMP C. S. T. S. M. AVRELIO DIVI MAR. ANTONINI PI
PONT MAX TRIB POT. XXIIII. M. THICI ET DIVI NERVAE.

In

In Civitate municipali Thibursica, hodie corrupte
nomine Taberzoc, haec reperiuntur monumenta.

In lapide in arcis antiquæ collabentibus muriis affixo
exterius haec inscriptio visitur.

SALVIS DOMINIS NOSTRIS CHRISTIANISSIMIS ET
INVICTISSIMIS IMPERATORIBVS IVSTINO ET SOFIA
AVGVSTIS HANC MVNITIONEM THOMAS EXCELEN
TISSIMVS PREFECTVS FELICITER AEDIFICAVIT.

In interioris lateris arcis lapide sic.

VRBI ROMAE ETERNAE AVG --
RESP MVNICIPI SEVERIANI ANTO
NINI LIBERI THIBVRSCENSIVM
BVRE.

In quodam fonte alibi.

NEPTVNO AVG. SAC. PRO SALVTE IMP. CAESARVM
L. SHTIMIS.

In quodam muro.

AEDEM NOVAM L. PALACTVS HONORATVS ET BONI
TATE AE. VXORIS SVAE -S-S- XX
MIL. N. EX.
MVLTIPLICATA PECVNIA EXCOLVIT ET OMNI RE
PERFECIT,

Alibi.

QVAM IN HOC TEMPLO OB
VAP SVA PECVNIA RESTITVIT OPERI
PAVET.

Ja

In alio lapide hoc fragmentum legitur.

Q. ACILIO C PAPIER ---
 INONIAE AVGG. L. NN
 SICENSIVM PROCON
 RES THEATRI PONR
 IB. C. ADVOCATO CODICI
 ADMINISTRATIONIS HEREDI
 IN ME ET COHERENTIVM CV
 LAVRENTIVM VICO AVGVSTINORVM
 SAÇERDOTEM LAVRENTIVM
 CAC - - - APIVM - - - RESP
 MVNICIPI SEVERIANI
 ANTONINIANI LIB. THIB. BVRE
 - - - - - PATRONO

In loco qui dicitur
 Bervic.
 D. M. S.
 Q. POMPONIVS
 ROGATVS
 FRVGALISSIMVS
 VIXIT ANNIS LXXV
 CERFICIA SIGGESS
 MARITO
 DVLCISSIMO S. P. F.

In loco Telel
 vocato.
 PETRONIA
 DONATA PIA
 VIXIT ANNIS
 LXXXV. M. I.
 PRO MERITIS
 EIVS MEMORIAE
 BENIGNISSIMAE
 OPPIA CELSINA
 FILIA FECIT.

In loco qui appell-
 latur Bujobsa.
 IMP. CAESAR
 M. AVRELIVS
 ANTONINVS
 AVG. PON. MAX.
 TRIBVNICIA POTES.
 XIII COSIII PP
 RESTITVTI
 LVIII.

In prædio ab Arabibus vocato Manfu, quod antiqua
Maramana esse videtur.

| D. M. S. | D. M. S. | D. M. S. |
|----------------------|---------------------------|----------------------|
| ANTONINVS | CARI ROMANI ET AVG | M. ANTONIVS |
| DOMITIVS | PARENTVM MEMORIA | DONATIANVS |
| VIXIT IN PACE | | NEPOS PIVS |
| ANNIS LVII. | | V. ANNIS XXII |
| L. I. S. | | M. VI. |
| | | S. P. |

| D. M. S. | D. M. S. |
|--------------------|----------------------|
| CAMILIVS | M. ANTONIVS |
| DONATIANVS | DONATIANVS |
| VIXIT ANNIS | NEPOS PIVS |
| ----- | V. ANNIS XXII |
| | M. VI. |
| | S. P. |

In Susa civitate hodie hujus regni præcipuâ.

MARCELI ALFONDI EPISCOPI.

In pago hodie Augen.

IVLIVS SA
BINIVS V.
ANNIS LVI
PM. ME
H. S. E.

In

In turre Manaéra dictâ.

| |
|-----------|
| AVRELIO |
| QVARTO |
| PATRI -- |
| - - - - - |
| - - - - - |

| |
|------------|
| C. SVELLIO |
| PONTIANO |
| PATRVELI |
| - - - - - |
| - - - - - |

| |
|-----------|
| CLAVDIA |
| CASTA -- |
| XEI - - - |
| - - - - - |
| - - - - - |

| |
|------------|
| L. AEMILIO |
| AFRICANO |
| AVVNCVLO |
| - - - - - |
| - - - - - |

In vico ab Arabibus hodie Taztor appellatur.

In unius columnæ scapo.

FORTISSIMO
IMP. ET.
PACATORI
ORBIS M. CLA
VDIO TACI
TO PIO FEL
AVG.

In alterius scapo.

DD. NN FLAVII F.
VALENTINIANO ET
VALENTI PII FELICES VIC
SEMPER AVGG.
MVNI MIZADO
TERENI - - -

In alia columnæ.

IMP. CAES. C.
VALERIVS
DIOCLETIANVS
PIVS FELIX
AVG

In

In lapide quadrato.

D. N. IMP. VALERIO LICINIA
NO LICINIO AVG MAX.
SARMATICO MAX, GERMA
NICO MAX. TRIBVNICIA POTES
TATE X CONS V IMP. X PATER PATRIAE PRO
CONS. COL. BISICA LVCANA DEVOTA NVMINIBVS.
MAJESTATIQVE EIVS.

In alio simili lapide.

POLLENTE^S IN PINE IMPERIO
DD. NN. HONORII ET THEODOSII PPS IMP. AVG.
ADMINISTRANTE FELICE INNODIO.

In alio lapide, qui Bovis caput insculptum habet, haec
superscriptio notatur.

SATVRNO AVG..

SAC.
MAFRINIVS FE
LLE SAC.
V. S. L. A.

In alio lapide:

ANTONIVS VICTOR
V. S. L. A.

In

In lapidibus quadratis hæc epitaphia Christianorum
sepulchralia sunt.

SANCTAE TRES
MAXIMA
ET DONATILLA
SECVNDA
BONA PVELLA.

ENATIANVS . . .
DIACONVS IN PAC
IT ANNO LXIII
SIKTO PRIDIE KA
LEND AVG.

IGNICIA D. VICIS
IN PACE.

VOLVSINA
CASTVLA PLA
VIXIT ANNIS L
H.S.P.O.T.P.Q. TIIS.

Hæc vero epitaphia in lapidibus cubicis Gentilium
Romanorum sunt.

D. M. S.

M. HORTENSIVS

FAVSTINVS ER. V.

CARISSIMVS

PIVS VIXIT

ANNIS LX.

H. S. E.

S. T. B. O.

T. T. I. S.

D. M. S.

L. VALERIVS

VICTOR

LVCANIANVS

VIXIT ANNIS

XXIII. M. III.

D. XXV.

D. M. S.

Q. SENTIVS

MARTIANVS

PIVS VIXIT ANNIS

XLVIII. MEN. IIII.

H. S. B. Q. T. B. Q.

Quinque

Quinque leucis à Tunete pagus est, qui ab Arabibus appellatur *Taborba*; amphitheatri vestigia prōstant adhuc, quod Mahamet Bey omnino diruit, in epistylio autem portæ sic legitur.

PRONEP. T. AELIO HADRIAN - - -
RVM GENTI QVE MVNICIPIVM AELIVM AVIT.
PROCOIS ET Q EGRILIO PLARIANO LEG. PR.

In lapide quadrilatero.

SEXT CAEL FILIO. Q. F.
QVESS CRECEN - - -
VOLVSIANO PRAEFEC - - -
FABIO SACERD CVRION - -
SACRIS FACIEND. ADVO
CATO FISCI ROMAE PROC
X HER AB EPISTVI
VIC ANTONINI AB EP
ISTVI AVGVSTORVM PA
TRONO MVNICIPII DD. PP.

In lapide marmoreo cominus in loco, qui ab Arabibus nuncupatur Sidi Tabet, a memoria illis cujusdam sancti veneranda, sic legitur.

MEMORIAE SANTISSI
MAE FEMINAE
DONATAE QVAE VIXIT ANNIS
XLI. MENSIBVS VIII.

In

Inter vestigia Civitatis Thugæ, hodie Arabum mapalia, ab aliisque nomine corrupto Tucca appellatae, templi ruinæ visuntur, in cuius porticu legitur.

L MARCIVS SIMPLEX ET L MAR
CELVS SIMPLEX REGILIANVS S. P. F.

In hujus templi latere sic.

CLAVDIO CAESARI AVG.
MAXIMO TRIBVNICIA POT.
R. CRASSVS AEDIL ORNAM. T.R.M.
TÌ VIR AVGVR IIVIR QVINQVE
C. FAR PERPETVVS SACERI
VS PAGI THVGGENSIS NOM
ET PERPETVI ARCV.

In alio lapide abhinc separato.

IMP CAES-DIVI
NERVAE NEPOTI
TRAIANI DACICI
PARTHICI FIL. L TRA
IANO HADRIANO AVG.
PONTIF MAX. TRIBVN
POTEST COS II PP.
CIVITAS THVGGAE DD. PF.

In alio lapide quadrato.

IMP CAES DIVI ANTONINI MAC - - -
MARCO AVRELIO SEVERO ALEXANDRO
PONTIFICI MAXIMO TRIBVNICIA POTES.
ET CASTRORVM ET SENATVS ET PA
LIVM LIBERVM THVGGA.

In prædio Caserim appellato prostant adhuc duo magnifica monumenta.

In Turri.

SIBI ET CLAVDIAE LEG III AVG LEG XVI LEG IIII LEG.
III APOLLINARIS LEG II ADIVTRICIS CONSECVTVS OB
VIRTVTEM IN EXPEDITIONEM PARTHICAM CORO
NAM MVRALEM VALLAREM TORQVES ET PHALERAS
AGIT IN DIEM OPERIS PERFECTI ANNOS LXXX.

Ex opposito ejusdam Turris.

SIBI ET CLAVDIAE MARTIAE CAPITOLINAE CONIVGI CARISSIMAE QVAE AGIT IN DIEM OPERIS PERFECTI ANNOS LXIV. ET MARCO PETRONIO FORTVNATO FILIO MILITAVIT ANNOS VI LEG XVII. PRIMOGENITO LEG II AVG VIXIT ANNOS XXXV CVI FORTVNATVS ET MARTIA PARENTES CARISSIMO MEMORIAM FECERVNT.
----- COLONIA SIMPLICIBVS QVONIAM FELIX CIVIBVS SPARTAM DIRIPVERE ROMANORVM HAEC POSSESSA FVERE.

In alia Turri.

FLAVIVS SECUNDVS FILIUS EPAMINONDAS FLAVIAE VXORI REGINAE SPARTAE SORORI EMILIANI FILIAE POSVIT HIC PRIMO FLAMINI PRAESIDENTIS IVSSV RECONDITAE KALEND. ----- IDVS IX. MENSE AVGSTO. FLAVIVS FLAVIVS EPAMINONDAS SECUNDVS IVRISCONSVLTVS AD PERPETVAM REI MEMORIAM ANNO LX. REGINA VERIA BIENNIO XXXV. AGESILAO SECUNDO COLOCATA FVIT IN MATRIMONIO ABSOLVTE LIBERAM POSVIT HIC TER STATVAM VXOR PIA. VIXIT ANNOS XL. M. V. D. III MAXIMIANO II ET HEREDIBVS HIC FVBRE.

Postea

Postea Distichis vitæ fragilitatem et miseriam deplorat, ut constat ex his fragmentis.]

CVIVS SI MEMBRIS VOCEM NATVRA DEDISSET
COGERAT HIC OMNES SVRGERE MANE DEOS
OPTO SECVNDE GERAS MVLTOS FELICITER ANNO^S
ET QVAE ELEGISTI HAEC MONVMENTA LEGAS.

Omnia carmina exscripsit Dnus Hernestus Aberstreit,
quæ hic deficiunt: primum Distichon alludit ad
lapideum Gallum, qui in cacumine turris erat.

Inter Civitatis Tignicæ vestigia, hodie *Tanica*, hæc
reperiuntur.

| |
|--|
| C. MEMMIO FELICI FLAMINI AVG PERP VTRIVSQVE PARTIS CIVITATIS TIGNICEN SIS C MEMMIUS FORTVNATVS FLAM AVG PERP VTRIVS QVE PARTIS CIVI TATIS THIGNICENSIS PROPTER EXIMIAM PIETATEM ET AFECTIO NEM FRATERNAM QVAM C. CAES ET TIBERIO I X HIBIT. POSVIT. |
|--|

| |
|---|
| In Magnæ Porticus fragmentis. ALTISSIMO SAECVLO DDDNNN -- ORI HOLITORI INDVLTA PAC -- CIPII THIGNICENSIS PROCON -- |
|---|

| |
|--|
| In alio vicino. CONSTANTINI MAXIMI V. - - - NIA A FUNDAMENTIS ET S - - - VLATV DO. DOMITICENO FILIO - |
|--|

| |
|---|
| Aibi in alio fragm. ANTONINI PII CASTRORVM THIGNICA DEVOTVM. |
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In Fornicibus et domibus epitaphia sepulchralia
reperiuntur.

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|-------------|
| D. M. S. |
| FABIUS FAVS |
| TVS PIVS |
| VIXIT ANNIS |
| LXXVI. |
| H. S. E. |

| |
|-------------|
| D. M. S. |
| ABIDIUS |
| FAVSTVS |
| LVCILIANVS |
| PIVS VIXIT. |
| H. S. E. |

| |
|------------|
| D. M. S. |
| F. MELLVS |
| AVSVMELLVS |
| PIVS VIXIT |
| ANNIS LV. |
| H. S. E. |

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| D. M. S. |
| C. HERCVLEIVS |
| IANVARIVS |
| P. VIXIT |
| ANNIS LXV |
| H. S. E. |

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| D. M. S. |
| T. VRANIA |
| IANVARIA |
| P. V. A. |
| LXV. |

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|---------------|
| D. M. S. |
| MARCVS HER |
| CVLEIVS IANVA |
| RIVS |
| P. V. A. LV. |
| H. S. E. |

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|---------------|
| D. M. S. |
| C. HERCVLEIVS |
| ABIBIANVS |
| P. V. A. XII. |
| H. S. E. |

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|------------|
| D. M. S. |
| HERCVLEIA |
| MARCIANA |
| PIA |
| V. A. XII. |

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|---------------|
| D. M. S. |
| C. HERCVLEIVS |
| VICTOR |
| PIVS V. A |
| XIII. |
| H. S. E. |

In ruinis Civitatis Beiso, hodie pagus Beissones vocatur.

In Arcis dirutæ frontispicio legitur.

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|--|
| MAGNIS ET INVICTISSIMIS DDDD. NNNN |
| DIOCLETIANO MAXIMIANO PERPETVIS |
| AVGG. ET CONSTANTINO MAXIMIANO NOBIS CAESARIBVS |
| RESPVBlica - - - - BEINSIVM DEDICAVIT. |
| MARCO IVLIO PROCONS PA. MAIESTATIQVE EORVM DICATO. |

Supra Arcis fenestram.

DIOCLETIANI ET MAXIMIANI ET - -

In alia.

PRO SALVTE IMP ANTONINI AVG PII ET
LIBERORVM SVORVM.
CINCIVS ET VICTOR AD LAVDANDAM - - -

D. M. S.
MAGNIA
NVS IVLIVS
P. V. A. XII.
H. S. E.

D. M. S.
CECILIA
FORTVNATA
PIAV.A.LXXXII
H. S. E.

D. M. S.
ANIANVS
P. V. A. L.
H. S. E.
VXOR MARITO
AMANTISSIMO

In templi vestigiis.

D. M. S.
VIIA VICTORIA
V. A. LXXXIIII
H. S. E.

AVG. SACRVM
SISTEMPLVM
CVM SVA PECVNIA
DICAVIT.

D. M. S.
Q. MORASSINA
FELICIA P.
V. A XXX
BAISAM.

In fragmento.

MARTIAE CHAR. N VICTORIAE PIAE VXORI
ET MARCITILIO VALERIANO NEPOTI
IBVS SVIS FECIT.

In

In Seluquia.

In columna.

IMP. CAESAR
MARCO AVRELIO
PROBO
PIO
FELICE
AVG.

IN FRAGMENTO.

PRO SALVTE IMP.C.
Q. MARTIVS FELIX
DE LIBERI PATRIS

In Lepide.

T. FLVIAN
CONSTAN
NOBILIB.
CAESARIB. NVMI
NI FORVM
VCATISSIMA
SVA PECVNIA
MVNICIPIC HIDIBB

In Templi ruinis.
H. DIANA AVG. SAC.

IMP CAES DIVI M...
ANTONINI PII GE..
NEP. DIVI HADRIANI
PRONEPOTI DIVI
TRAIANI PAR. AB
NEPOTIS DIVI NERVAE
SEPTIMIO SEVERO
PERTINACI AVG. ARA
N. PP. PONT MAX TRIB.
POTEST. IMP. VII. COS. II
HIDIBELENSIS.

In Templo.

IOVI OPTIMO
MAXIMO AVG. SAC

D. M. S.
MEMMIVS
IANVARIVS
PIVS VIXIT
ANNIS XXXV.
H. S. E.

In Columna.

SOLI INVICTO
CAES. M. AVRELII PROBI PII
DOMVS EIVS MVNICIPIVM CIII.

In fragmento.

PRO SALVTE IMP. CAES.
M. NVMISIVS DONATVS FL PP CONTIC.

Epitaphia.

LVRIA C. F
POSILLA
VICTORIS PIA
V. AN. XX. H. S. E

D. M. S.
SATVRNINVS FELICIVS
Q. SISENNE FIL. PIVS
VIXIT ANNIS LXXIII.

XXXV. A Letter from Mr. George Edwards, F. R. S. to Thomas Birch D. D. Secret. R. S. concerning An Observation made by him in Opticks.

Sir,

Read June 16, 1763. Having lately accidentally discovered, that the shadows of things floating in water, a little below its surface, are reflected from the air above the water more strongly (to my apprehension) than objects above the surface of the water are reflected from the water; and consequently, that fishes playing beneath the surface of a still water, may see their images distinctly playing in the air, with this advantage over men, who view their faces in the water; for things in air, that are reflected from the water, must have, when placed over the water, have their dark or shadowed sides reflected from it, which renders the images obscure. On the contrary, the inhabitants of the waters have almost a hemisphere of light falling on their upper sides, which are the sides, that are reflected from the air, which consequently renders such images lighter, and more striking to the eye, than reflections of obscured things in air, when reflected from the water. As I have never heard of, or read, any account of this discovery, I imagine it may be new: but you, Sir, in far more extensive reading, may be acquainted with such a discovery. If so, I acknowledge my ignorance of it; and ask pardon for giving

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giving you this trouble, and desire it may be layed aside ; but, if it be thought worthy communicating to the Royal Society, I will be ready, in a very simple and easy manner, to demonstrate the truth of the above discovery. I do not see any use of this discovery at present, more than an amufing speculation; tho' perhaps, when it is reconsidered by persons superior to me in penetrating into the secrets of optics, some real use may be made of it.

I am Sir, with great

respect, your very

June 15, 1763.

humble servant

George Edwards.

XXXVI. *An*

**XXXVI. Two remarkable Cases in Surgery,
by Mr. Francis Geach, Surgeon in Ply-
mouth. Communicated by John Huxham,
M. D. F. R. S.**

*Extract of a Letter from John Huxham, M. D.
F. R. S. to W. Watson, M. D. F. R. S. dated at
Plymouth, the 10th of May, 1763.*

I Have herewith sent you two extraordinary cases, which Mr. Francis Geach, one of our surgeons, put into my hands some time ago. I think there are some things remarkable in them. I have sent also three of the concretions found in the gall-bladder of the icteric person. The three others I reserve for making some experiments on them. They are all nearly of the same shape and size.

The person, wounded in the eye, is now considerably stronger and better. The observation, that wounds of the brain often cause a paralysis on the opposite side of the body, is as old as Hippocrates, and is taken notice of also by Aretæus.

I am very well assured that the facts are exactly related; and I choose to send them in the words, nay even in the hand writing of Mr. Geach.

[From

C A S E I.

S I R,

Read June 23,
1763.

A Man aged forty-two years, not much addicted to spirituous liquors, nor too rigidly abstemious, seven months ago received a violent blow on the right hypochondrium: soon after he was seized with the colic, and had a yellow suffusion over his skin: himself dated the æra of his misfortune from the blow. At first he had a diarrhoea, but at last became so costive as to have no regular intestinal discharge, but by the help of lenitives. He was much emaciated towards the end of life, his skin was astonishingly yellow, and dry as parchment, or leather shrivelled by the fire. Many medical processes were employed ineffectually. He had no considerable pain any where. A week before his death the left arm turned quite black. He had frequent hæmorrhages from his nose. On dissection, the liver was found considerably enlarged, externally of a pale lead colour, harder and more solid than in a sound state, but not schirrous; internally, more porous and spungy. The inner substance not deviating from its natural colour, seemed to be made up of distinct fibres intersecting one another, with vacuities between them equal in size to the small cells of honeycombs. The ductus cysticus, and hepaticus, as well as the pori bilarii were perfectly ligamentous. The gall-bladder had changed its pyriform figure, and affected that of a cylinder, the fibres of which were

were hard, white, and compacted. The pylorus, and the duodenum were in a similar state. The circular fibres of the pylorus were rigid beyond conjecture. The concretions, six in number, each weighing half a drachm, and specifically heavier than water, a circumstance unusual, were all ranged in a parallel line, and tallying pretty exactly with one another, so completely filled up the tube (for it might be called with more propriety so than bladder) as to allow but little intermediate space. The passage into the duodenum was almost closed up. Scarce any sincere gall issued forth on incision; but a small quantity of a turbid, saponaceous fluid, not unlike chocolate in colour, came out, or rather was expressed out, gradually.—The bile, not finding a ready exit through the ductus choledocus, stagnated probably in its repository, became diseased, and, acquiring the consistency of soapy dregs, proved the constituents of those concretions, which on experiment are found combustible as wax, and as no fermentation arises from pouring acids upon them, it may be concluded the bile is no alkali. The omentum was almost destroyed, the little that remained of it, was hard and black, and afforded no ill emblem of sea weed, when dried. The glands of the mesentery were in some parts schirrous; in others, they represented small and distinct steatomas. It may be needless to observe what is common to other dead bodies, that the distension of the stomach and intestines was in the greatest extremity.

C A S E II.

Read June 23,
1763. M R. James L——d, midshipman
of his majesty's ship Liverpool,
in a riot, December 10, 1762, was wounded in the
left eye: a small sword entered in at the external an-
gle, and passing quite through the eye, towards the
basis, struck against the inner part of the orbit. He
fell down instantaneously senseless, with loss of speech,
and an hemiplegia of the *opposite* side: blood was im-
mediately drawn, the texture of which was not strong-
ly cohering: the next morning he was found lying
upon his back, with the right eye widely opened,
and the pupil (though in a light room) considerably
dilated. This eye was incapable of discerning ob-
jects, never winking at the waving of the hand, or
the close application of the finger; though sometimes
it was convulsed. The left eye was extruded from its
orbit, and enlarged to the size of a pullet's egg, though
destitute of all its humours: his pulse beat at long in-
tervals, with a lazy motion, and stopped upon gentle
pressure: the body was not feverish, but preserved a na-
tural heat, the paralytic side, arm, and thigh excepted,
which were livid, cold, and rigid; the lancet was
employed without exciting any sensation, and blisters
lay on several days without raising any vesications;
these benumbed parts were constantly bedewed with
clammy sweat. He was devoid of anxiety, or in-
quietude, the powers of nature seemed to be almost
suspended, and life to be carried on, only through
the large organs and vessels. The functions of the
lower

lower belly were debilitated, lenient and strong purgatives producing no irritation in the stomach and intestines ; and clysters, though repeatedly injected, were never repelled. The urine was emitted by drops only, and sometimes it would run off suddenly in a deluge : his hearing, though not quite lost, was considerably impaired ; he lay lethargic and dead almost to every thing, though by pulling the arms and shaking the body, by loud and frequent calling, by desiring him to extend his tongue, he would gape widely ; and forgetting seemingly what had been said to him, keep his mouth wide open, when the tongue might be seen quivering and retracted. Five weeks elapsed in this state of insensibility, every thing he took was with voracity, but without relish and without distinction. About this time a new and dreadful symptom began to threaten, the jaw seemed to be moved with difficulty, and liquids only could be poured down ; the hypocondria were hard and distended, and every effort to procure an intestinal discharge proved ineffectual, when very large eruptions of the miliary kind were suddenly diffused over the sound parts. From that critical moment he perspired freely, and had an easy motion of the jaw ; his urine was rendered in a due quantity, and purgatives of the lenient kind easily operated, the hypochondria were soft, and equal ; the discharge from the eye, which hitherto had been acrid, was now copious and laudable, the sound eye had its motion, he could see distinctly, and seemed in other respects sensible, when roused from his stupefaction : soon after he could bear to be moved from the bed to a chair without fatigue,

the paralytic parts were rubbed with vinegar and mustard, and he took the following medicines.

Pulv valerian 3ss

— Cast. Rus. gr. 4.

Spec. Diambrae gr. iij.

Syrup. Croci q. s. m. f. Bolus ter die sumend.
ex haustu seri finapini.

A cataplasm of bread and milk had been daily applied to asswage the inflammation and swelling of the eye, and a decoction of thyme and mustard was employed as a gargarism to help the suppression of voice. Soon as he began visibly to mend, he had sometimes loud and sudden bursts of laughter, and sometimes only a long continued silent simpering, a species of convulsion not unlike that called by the Greek physicians *Kυνικός αναστομός*, save only that this was not attended with a fever. When he attempted to walk, he had such gestures as accompany the St. Vitus's dance; and seemed a perfect idiot, throwing eagerly forward one leg, and dragging the other trembling after. His appetite is now naturally moderate, his sleep sound and refreshing, his hearing acute, he speaks, but drawls out his words rather indistinctly than articulately, the paralytic arm and thigh are again animated, and recover but slowly their flexibility and extension. He tells me that he remembred nothing from the moment he received the injury, to the time he recovered and sat up; there was a temporary privation of the intellectual faculties. It may be worth while to observe, that though several large sloughs were thrown off

from

from the Eye, though the suppuration was in a large quantity; yet the bulk of the parts did not diminish, nor the inflammation lessen, till an astringent fustis of red rose-leaves and port wine was applied, which so effectually braced up the relaxed parts, that the lids now cover the deformity. Though it may be difficult to account satisfactorily for the paralysis of the opposite side, yet monsieur de la Faye* has something pertinent to the matter; the passage is not long, and may be worth transcribing.

La Moëlle Allongée n'est que le prolongement de la substance médullaire du cerveau, & du cervelet. Les fibres qui la composent, se croisent, de sorte que celles du côté gauche passent au côté droit, & celles du côté droit au côté gauche; c'est de cette Moëlle Allongée que partent immédiatement les dix paires de nerfs qui sortent du crane. Comme les fibres de la substance medullaire se croisent, les nerfs se croisent aussi, c'est à dire, que ceux qui viennent du côté droit, passent au côté gauche, & que ceux qui viennent du côté gauche, passent au côté droit. Delà vient, à ce qu'on pretend que la paralysie, lorsqu'elle est la suite de la compression de quelque endroit du cerveau, se trouve pour l'ordinaire au côté opposé à celui de l'endroit comprimé.

* Principes de Chirurgie, première partie.

Francis Geach.

XXXVII. *An*

**XXXVII. An Account of a new Die from
the Berries of a Weed in South Carolina :
In a Letter from Mr. Moses Lindo, dat-
ed at Charles Town, September 2, 1763,
to Mr. Emanuel Mendez da Costa, Li-
brarian of the Royal Society.**

Read Nov. 10, 1763. IN August 1757, I observed the mocking bird fond of a berry, which grows on a weed called Pouck, represented to me as of a poisonous quality; the juice of this berry being a blooming crimson. I was several times inclined to try, if I could extract a die from it; yet the very thoughts of its quality prevented me from proceeding, till observing these birds to void their excrement of the same colour as the berry, on the Chinese rails in my garden, convinced me it was not of the quality represented. I therefore made a tryal in the following manner.

1st. I ordered one of my negroes to gather me a pint of those berries, from which I extracted almost three quarters of a pint of juice, and boiled it with a pint of Bristol water, one quarter of an hour.

2dly. I then took two pieces of flannel and numbered them 1 and 2, boiled them in a separate tin pot with alum a quarter of an hour, and rinced them in cold water.

3dly. I then dipped the piece of flannel N° 1. into the pot, where the juice was, and left it to simmer five

five minutes, then took it out, and rinsed it in cold water; when, to my surprize, I found a superior crimson dye fixed on the flannel than the juice of the berry.

4thly. I then dipped the piece of flannel N° 2. in the same juice, and being desirous to clean my hands from the stain, which N° 1. had caused, I ordered some lime water to be brought me, such as we use to settle our indigo, and found the colour of the stain change to a bright yellow. This unexpected change urged me to throw a wine glafs-full of lime water into the pot, where the piece of flannel N° 2. was simmering; on which, all the juice, as well as the flannel, became of a bright yellow, by which I find alum fixed the crimson, and lime the yellow.

5thly. Having then put a quart of fresh juice in two pint decanters, in one of which I put a small quantity of powdered alum, I laid them up: about six weeks after, I then examined them, and found the juice in the decanter, which had no alum, was turned black, and the other retained its colour.

XXXVIII.

XXXVIII. An Account of the Eclipse of the Sun, April 1, 1764: In a Letter to the Right Honourable George Earl of Macclesfield, Pres. R. S. from Mr. James Ferguson, F. R. S.

My Lord,

Read Nov. 17, 1763. I Beg leave to present to the Royal Society a projection of the eclipse of the Sun, which will be on the 1st of April 1764. The diagram shews the time and phases of that eclipse, for the Royal Observatory at Greenwich, and the calculation is from Meyer's Tables.

According to Flamsteed's Tables, and Dr. Halley's, and M. De la Caille's, the eclipse should be annular at London; and De la Caille, in a map in his Ephemerides, makes it almost Central. But, according to Meyer's Tables, the appearance will be very different; for the southern limb of the Moon will be about the 20th part of a digit over the southern limb of the Sun; and Meyer makes the beginning, greatest obscuration, and ending, to be at least a quarter of an hour sooner than Flamsteed, Halley, and M. De la Caille do.

As the passage of the shadow will be north-eastward over Europe, and all these authors make it's center to pass more or less eastward of London and Greenwich; and since there is such a great difference in Meyer's time from that of all the others, and in the

the phasis at the greatest obscuration ; a clear sky is much to be wished for : and it might be proper to have observers placed about ten or twelve miles from one another, all the way between Greenwich and Deal, that the truth may be found by observation. For, if Meyer's tables are right, all the others must want much correction.

About five years ago, I delineated the path of the Moon's shadow over all the parts of the earth, where it will pass in this eclipse, from sun-rising till sun-setting, according to mean or equal time. I constructed the map much upon the same principle with that of Manfredi and De la Caille, and took most of the lunar elements from Meyer's tables.

According to this projection, the center of the penumbra will first touch the earth at sun-rise, in the Atlantic Ocean, between the Caribbee Islands and those of Cape Verd ; in 47 degrees west longitude from Greenwich, and 18 degrees north latitude ; from which it will go on, almost parallel to the equator, for about 20 degrees eastward : then it will bend north-eastward, and pass over the south of Portugal, near Cape St. Vincent : from thence, in its progress, it will go over Valadolid, cross the Bay of Biscay a little west of Bayonne, pass over Rochelle, then midway between Rouen and Paris, go over Holland a little west of Amsterdam, cross the German Sea about 40 miles west of the mouth of the river Elbe, enter Denmark, cross the Baltic by Gottenburg, travel over Sweden west of Stockholm, pass close by the west side of the Gulph of Bothnia, go a little west of Torneo, enter the Frozen Sea at Wardhuys, cross over to Nova Zembla, and a little to the east thereof

it will end in the North Sea with the setting Sun, in 105 degrees east longitude, and 72 degrees north latitude.

If the motions of the Sun and Moon were equable, any given eclipse would always return in a course of 223 lunations, which would consist of 18 years 11 days 7 hours 43 minutes 20 seconds (as was observed by the antients) for 1388 years; and would forever do so, if, at the end of each period, the Sun and Moon should be in conjunction either in the same node, or at the same distance from it as before. But that is not the case: for, if the Sun and Moon are once in conjunction at 18 degrees distance from the node, which is the greatest distance at which the Moon's shadow can touch the earth, at the next period of 18 years 11 days, &c. the Sun and Moon will be 28 minutes 12 seconds of a degree nearer the same node than they were at the period last before. And so by falling gradually nearer and nearer the same node every time, the Moon's shadow will pass over the center of the earth's enlightened disc, at the end of the 38th periodical return of the eclipse from the time of its first coming in at either of the earth's poles; because the conjunction falls in the node at the end of the 38th period.

In each succeeding period, the conjunctions of the Sun and Moon will be gradually farther and farther from the node, by the quantity of 28 minutes 12 seconds of a degree; which will cause the Moon's shadow to pass over the disc of the earth, farther and farther on the opposite side from its centre, till it quite leaves the earth, and travels *in expansion* for about 12,492 years, before it can come upon the earth again at the same pole as before.

The

The reason of this will be plain, when we consider, that 18 degrees from either of the nodes of the Moon's orbit is the greatest distance, at which her shadow can touch the earth at either of its poles. And as there are 18 degrees on each side of the node, within the limits of a solar eclipse; and twice 18 make 36, these are all of the 360 degrees of the Moon's orbit about either of the nodes, within which there can be an eclipse of the Sun: and as these eclipses shift through 28 minutes 12 seconds of these 36 degrees, in every Chaldean or Plinian period, they will shift through the whole limit in 77 periods, which include 1388 years and 3 months. And then, the periods have the remaining 324 degrees of the Moon's orbit to shift through, at the rate of only 28 minutes 12 seconds of a degree in each period, before they can be near enough to the same node again, for the Moon's shadow to touch the earth; and this cannot be gone through in less than 12,492 years: for, as 36 is to 1,388, so is 324 to 12,492.

The eclipse, April 1st, 1764, fell in the open space, quite clear of the earth at each return, ever since the creation till A. D. 1295, June 13th old stile, at 12° 52' 59" p. m. when it first touched the earth at the north pole, according to the mean (or supposed equable) motions of the Sun and Moon; their conjunction being then 17° 48' 27" from the moon's ascending node, in the northern part of her orbit. In each period since that time, the conjunction of the Sun and Moon has been 28' 12" nearer and nearer the same node, and the Moon's shadow has therefore gone more and more southerly over the earth. In the year 1962, July 18th, old stile, at 10^h 36' 21" p. m.

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the

the same eclipse will have returned 38 times; and as the conjunction will then be only $24' 45''$ from the node, the center of the Moon's shadow will fall but a little northward of the center of the earth's enlightened disc. At the end of the next following period, the conjunction of the Sun and Moon will have receded back $3' 27''$ from the Moon's ascending node, into the southern part of her orbit; which will cause the center of her shadow to pass a very small matter south of the center of the earth's disc. After which, in every following period, the conjunction of the Sun and Moon will fall $28' 12''$ farther and farther back from the node, and the Moon's shadow will go still further and further southward on the earth, until A. D. 2665, September 12, old style, at $23^h 46' 22'' p.m.$ when the eclipse will have finished its 77th period, and will finally leave the earth at the south pole; and cannot begin the same course over the earth again in less than 12,492 years, as above mentioned.

And thus, if the motions of the Sun and Moon were equable, the same eclipse would always return in 18 Julian years 11 days 7 hours 43 minutes 20 seconds, when the last day of February in leap years is four times included in the period: but when it is five times included, the period is one day less; or 18 years 10 days 7 hours 43 minutes 20 seconds.

But, on account of the various anomalies of the Sun and Moon, arising from their moving in ecliptic orbs, and the effects of the Sun's different attractions of the Moon in different parts of her orbit, the conjunctions of the Sun and Moon never succeed one another

another at equal intervals of time ; but differ sometimes by no less than 14, 15, or 16 hours ; and therefore, in order to know the true times of the returns of any eclipse, recourse must be had to long and tedious calculations.

In order to shew both the mean and true times of the above mentioned eclipse, through all its periods, whilst it is visible on this earth, together with the mean anomalies of the Sun and Moon, the true distance of each conjunction from the ascending node, with the true latitude of the Moon at the time of each of her true conjunctions with the Sun, according to the old Stile, I have calculated the four following tables, of which I beg the Royal Society's acceptance.

According to the mean (or supposed equitable) motions of the Sun, Moon, and nodes, the moon's shadow in this eclipse would have first touched the earth at the north pole, on the 13th of June, A. D. 1295; and would quite leave the earth at the south pole, on the 12th of September, A. D. 2665, at the completion of its 77th period ; as shewn in the first and second tables.

But, on account of the true (or unequable) motions of the Sun, Moon, and nodes, the true lines of conjunctions of the Sun and Moon, and the Sun's true distance from the Moon's ascending node, are as set down in the third and fourth tables : and the Moon's true latitude is too great at the end of the first mean period, to allow her shadow to touch the earth. So that the first time of the coming-in of this eclipse was at the end of its second mean period ; and the true time was on the 24th of June, A. D. 1313,
at

at $3^h 57' 3''$ past noon at London: and it will finally leave the earth on the 31st of July, A.D. 1793, at $10^h 25' 31''$ past noon, at the completion of its 72d period. So that, the true motions do not only alter the true times from the mean, but they also cut off five periods from those of the mean returns of this eclipse.

In this, and all other eclipses of the Sun, which happen about the ascending node of the Moon's orbit, the Moon's shadow first touches the earth at, or about, the north pole; and goes more and more southerly over the earth in each return, till it quite leaves the earth at, or near, the South pole. But when eclipses happen about the descending node, (as that of July 14th, A.D. 1748 did) the Moon's shadow first touches the earth at, or near, the south pole; and goes gradually more and more northward in each periodical return, till it finally leaves the earth at the north pole. And as the obliquity of the Moon's orbit to the ecliptic is the same about both the nodes, there must be the same number of eclipses about the one as about the other.

But I beg pardon, for mentioning things to your Lordship, and the Royal Society, which must be much better known to you all, than they can be to me; who am, with the highest degree of respect,

My LORD,

Your Lordship's

most obliged, and

most obedient humble servant,

Mortimer-Street,
Nov. 16, 1763.

James Ferguson.

T A B L E

T A B L E . I.

The mean time of new Moon, with the mean anomalies of the Sun and Moon, and the Sun's mean distance from the Moon's ascending node, at the mean time of each periodical return of the Sun's eclipse, March 21st, 1764, from the time of its first coming upon the earth since the creation, till it falls right against the earth's center, according to the old style.

| Period. | Years of Chift. | Mean time of new Moon. | | | | Sun's mean Anomaly. | | | Moon's mean Anomaly. | | | Sun's mean Distance from the node. | | | | | | |
|---------|--------------------|---------------------------|----|----|----|------------------------|----|----|-------------------------|----|----|--|----|----|---|----|----|----|
| | | M. | D. | H. | I. | S. | o | i. | " | S. | o | i. | " | S. | o | i. | " | |
| 0 | 1277 | June | 2 | 5 | 9 | 39 | 11 | 17 | 57 | 41 | 1 | 26 | 31 | 42 | 0 | 18 | 16 | 40 |
| 1 | 1295 | June | 13 | 12 | 52 | 58 | 11 | 28 | 27 | 38 | 1 | 23 | 40 | 19 | 0 | 17 | 48 | 27 |
| 2 | 1313 | June | 23 | 20 | 36 | 19 | 0 | 8 | 57 | 35 | 1 | 20 | 48 | 56 | 0 | 17 | 20 | 15 |
| 3 | 1331 | July | 5 | 4 | 19 | 39 | 0 | 19 | 27 | 32 | 1 | 17 | 57 | 33 | 0 | 16 | 52 | 2 |
| 4 | 1349 | July | 15 | 12 | 2 | 59 | 0 | 29 | 57 | 29 | 1 | 15 | 6 | 10 | 0 | 16 | 23 | 50 |
| 5 | 1367 | July | 26 | 19 | 46 | 19 | 1 | 10 | 27 | 26 | 1 | 12 | 14 | 47 | 0 | 15 | 55 | 37 |
| 6 | 1385 | Aug. | 6 | 3 | 29 | 39 | 1 | 20 | 57 | 23 | 1 | 9 | 23 | 24 | 0 | 15 | 27 | 25 |
| 7 | 1403 | Aug. | 17 | 11 | 12 | 59 | 2 | 1 | 27 | 20 | 1 | 6 | 32 | 1 | 0 | 14 | 59 | 12 |
| 8 | 1421 | Aug. | 27 | 18 | 56 | 19 | 2 | 11 | 57 | 17 | 1 | 3 | 40 | 38 | 0 | 14 | 31 | 0 |
| 9 | 1439 | Sept. | 8 | 2 | 39 | 39 | 2 | 22 | 27 | 14 | 1 | 0 | 49 | 15 | 0 | 14 | 2 | 47 |
| 10 | 1457 | Sept. | 18 | 10 | 2 | 59 | 3 | 2 | 57 | 11 | 0 | 27 | 57 | 52 | 0 | 13 | 34 | 35 |
| 11 | 1475 | Sept. | 29 | 18 | 6 | 19 | 3 | 13 | 27 | 8 | 0 | 25 | 6 | 29 | 0 | 13 | 6 | 22 |
| 12 | 1493 | Oct. | 10 | 1 | 49 | 39 | 3 | 23 | 57 | 5 | 0 | 22 | 15 | 6 | 0 | 12 | 38 | 10 |
| 13 | 1511 | Oct. | 21 | 9 | 32 | 59 | 4 | 4 | 27 | 2 | 0 | 19 | 23 | 43 | 0 | 12 | 9 | 57 |
| 14 | 1529 | Oct. | 31 | 17 | 16 | 19 | 4 | 14 | 56 | 59 | 0 | 16 | 32 | 20 | 0 | 11 | 41 | 45 |
| 15 | 1547 | Nov. | 12 | 0 | 59 | 40 | 4 | 25 | 26 | 56 | 0 | 13 | 40 | 57 | 0 | 11 | 13 | 34 |
| 16 | 1565 | Nov. | 22 | 8 | 43 | 0 | 5 | 5 | 56 | 53 | 0 | 10 | 49 | 34 | 0 | 10 | 45 | 20 |
| 17 | 1583 | Dec. | 3 | 16 | 26 | 20 | 5 | 16 | 26 | 50 | 0 | 7 | 58 | 9 | 0 | 10 | 17 | 7 |
| 18 | 1601 | Dec. | 14 | 0 | 9 | 40 | 5 | 26 | 56 | 47 | 0 | 5 | 6 | 48 | 0 | 9 | 48 | 55 |
| 19 | 1619 | Feb. | 25 | 7 | 53 | 0 | 6 | 7 | 26 | 44 | 0 | 2 | 15 | 25 | 0 | 9 | 20 | 42 |
| 20 | 1638 | Jan. | 4 | 15 | 36 | 20 | 6 | 17 | 56 | 41 | 11 | 29 | 24 | 2 | 0 | 8 | 52 | 30 |
| 21 | 1656 | Jan. | 15 | 23 | 19 | 40 | 6 | 28 | 26 | 38 | 11 | 26 | 32 | 39 | 0 | 8 | 24 | 17 |
| 22 | 1674 | Jan. | 26 | 7 | 3 | 0 | 7 | 8 | 56 | 35 | 11 | 23 | 41 | 14 | 0 | 7 | 56 | 5 |
| 23 | 1692 | Feb. | 6 | 14 | 46 | 20 | 7 | 19 | 26 | 32 | 11 | 20 | 49 | 53 | 0 | 7 | 27 | 52 |
| 24 | 1710 | Feb. | 16 | 22 | 29 | 40 | 7 | 29 | 56 | 29 | 11 | 17 | 58 | 30 | 0 | 6 | 59 | 40 |
| 25 | 1728 | Feb. | 28 | 6 | 13 | 0 | 8 | 10 | 26 | 26 | 11 | 15 | 7 | 7 | 0 | 6 | 31 | 27 |
| 26 | 1746 | Mar. | 10 | 13 | 56 | 20 | 8 | 20 | 56 | 53 | 11 | 12 | 15 | 44 | 0 | 6 | 3 | 15 |
| 27 | 1764 | Mar. | 20 | 21 | 39 | 40 | 9 | 1 | 26 | 20 | 11 | 9 | 24 | 21 | 0 | 5 | 35 | 2 |
| 28 | 1782 | April | 1 | 5 | 23 | 0 | 9 | 11 | 56 | 17 | 11 | 6 | 32 | 58 | 0 | 5 | 6 | 50 |
| 29 | 1800 | April | 11 | 13 | 6 | 20 | 9 | 22 | 26 | 14 | 11 | 3 | 41 | 35 | 0 | 4 | 38 | 37 |
| 30 | 1818 | April | 22 | 20 | 49 | 40 | 10 | 2 | 56 | 11 | 11 | 0 | 50 | 12 | 0 | 4 | 16 | 25 |
| 31 | 1836 | May | 3 | 4 | 33 | 0 | 10 | 13 | 26 | 8 | 10 | 27 | 58 | 49 | 0 | 3 | 42 | 12 |
| 32 | 1854 | May | 14 | 12 | 16 | 20 | 10 | 23 | 56 | 5 | 10 | 25 | 7 | 26 | 0 | 3 | 14 | 0 |
| 33 | 1872 | May | 24 | 19 | 59 | 40 | 11 | 4 | 26 | 2 | 10 | 22 | 16 | 3 | 0 | 2 | 45 | 47 |
| 34 | 1890 | June | 5 | 3 | 43 | 0 | 11 | 14 | 55 | 59 | 10 | 19 | 24 | 40 | 0 | 2 | 17 | 35 |
| 35 | 1908 | June | 15 | 11 | 26 | 20 | 11 | 25 | 25 | 56 | 10 | 16 | 33 | 17 | 0 | 1 | 49 | 22 |
| 36 | 1926 | June | 26 | 19 | 9 | 40 | 0 | 5 | 55 | 53 | 10 | 13 | 41 | 54 | 0 | 1 | 21 | 10 |
| 37 | 1944 | July | 7 | 2 | 53 | 0 | 0 | 16 | 25 | 50 | 10 | 10 | 50 | 31 | 0 | 0 | 52 | 57 |
| 38 | 1962 | July | 18 | 10 | 36 | 21 | 0 | 26 | 55 | 47 | 10 | 7 | 59 | 8 | 0 | 0 | 24 | 45 |

TABLE

T A B L E II.

The mean time of new Moon, with the mean anomalies of the Sun and Moon, and the Sun's mean distance from the Moon's ascending Node, at the mean time of each periodical return of the Sun's eclipse, March 21st, 1764, from the time of it's falling right against the earth's center, till it finally leaves the earth; according to the old style.

| Period. | Year Chift. | Mean Time of new Moon. | | | Sun's mean Anomaly. | | | Moon's mean Anomaly. | | | Sun's mean distance from the node. | | | |
|---------|----------------|---------------------------|----|----|------------------------|----|----|-------------------------|----|----|--|----|----|----|
| | | M. | D. | H. | S. | o | " | S. | o | " | S. | o | " | |
| 39 | 1980 | July | 28 | 18 | 19 | 41 | 1 | 7 | 25 | 44 | 10 | 5 | 7 | 45 |
| 40 | 1998 | Aug. | 9 | 2 | 3 | 1 | 1 | 17 | 55 | 41 | 10 | 2 | 16 | 22 |
| 41 | 2016 | Aug. | 19 | 9 | 46 | 21 | 1 | 28 | 25 | 38 | 9 | 29 | 24 | 59 |
| 42 | 2034 | Aug. | 30 | 17 | 29 | 41 | 2 | 8 | 59 | 36 | 9 | 26 | 33 | 36 |
| 43 | 2052 | Sept. | 10 | 1 | 13 | 1 | 2 | 19 | 25 | 33 | 9 | 23 | 42 | 13 |
| 44 | 2070 | Sept. | 21 | 8 | 56 | 21 | 2 | 29 | 55 | 30 | 9 | 20 | 50 | 50 |
| 45 | 2088 | Oct. | 1 | 16 | 39 | 41 | 3 | 10 | 25 | 27 | 9 | 17 | 59 | 27 |
| 46 | 2106 | Oct. | 13 | 0 | 23 | 1 | 3 | 20 | 55 | 24 | 9 | 15 | 8 | 4 |
| 47 | 2124 | Oct. | 23 | 8 | 6 | 21 | 4 | 1 | 25 | 21 | 9 | 12 | 16 | 41 |
| 48 | 2142 | Nov. | 3 | 15 | 49 | 41 | 4 | 21 | 55 | 18 | 9 | 9 | 25 | 18 |
| 49 | 2160 | Nov. | 13 | 23 | 31 | 1 | 4 | 82 | 25 | 15 | 9 | 6 | 33 | 56 |
| 50 | 2178 | Nov. | 25 | 7 | 26 | 21 | 5 | 2 | 55 | 12 | 9 | 3 | 42 | 33 |
| 51 | 2196 | Dec. | 5 | 14 | 59 | 41 | 5 | 13 | 25 | 9 | 9 | 0 | 51 | 10 |
| 52 | 2214 | Dec. | 16 | 22 | 43 | 1 | 5 | 23 | 55 | 7 | 8 | 27 | 59 | 47 |
| 53 | 2232 | Dec. | 27 | 6 | 20 | 21 | 6 | 4 | 25 | 4 | 8 | 25 | 8 | 24 |
| 54 | 2251 | Jan. | 7 | 14 | 9 | 41 | 6 | 14 | 55 | 1 | 8 | 22 | 17 | 1 |
| 55 | 2269 | Jan. | 17 | 31 | 53 | 1 | 6 | 25 | 24 | 58 | 8 | 19 | 25 | 38 |
| 56 | 2287 | Jan. | 29 | 5 | 36 | 21 | 7 | 5 | 54 | 55 | 8 | 16 | 31 | 15 |
| 57 | 2305 | Feb. | 8 | 13 | 19 | 41 | 7 | 16 | 24 | 52 | 8 | 13 | 42 | 52 |
| 58 | 2323 | Feb. | 19 | 31 | 3 | 1 | 7 | 26 | 54 | 49 | 8 | 10 | 51 | 29 |
| 59 | 2341 | Mar. | 2 | 4 | 40 | 21 | 8 | 7 | 24 | 46 | 8 | 8 | 0 | 6 |
| 60 | 2359 | Mar. | 13 | 12 | 29 | 42 | 8 | 17 | 54 | 43 | 8 | 5 | 8 | 43 |
| 61 | 2377 | Mar. | 23 | 20 | 13 | 2 | 8 | 28 | 24 | 40 | 8 | 2 | 17 | 20 |
| 62 | 2395 | April | 4 | 3 | 56 | 22 | 9 | 8 | 54 | 37 | 7 | 29 | 25 | 57 |
| 63 | 2413 | April | 14 | 11 | 39 | 42 | 9 | 19 | 24 | 34 | 7 | 26 | 34 | 34 |
| 64 | 2431 | April | 25 | 19 | 23 | 2 | 9 | 29 | 54 | 31 | 7 | 23 | 43 | 11 |
| 65 | 2449 | May | 6 | 3 | 6 | 22 | 10 | 10 | 24 | 28 | 7 | 20 | 51 | 48 |
| 66 | 2467 | May | 17 | 10 | 49 | 42 | 10 | 20 | 54 | 25 | 7 | 18 | 0 | 25 |
| 67 | 2485 | May | 27 | 18 | 33 | 2 | 11 | 1 | 24 | 22 | 7 | 15 | 9 | 2 |
| 68 | 2503 | June | 8 | 2 | 16 | 22 | 11 | 21 | 54 | 19 | 7 | 12 | 17 | 39 |
| 69 | 2521 | June | 18 | 9 | 59 | 42 | 11 | 22 | 24 | 17 | 7 | 9 | 26 | 16 |
| 70 | 2539 | June | 29 | 17 | 43 | 2 | 0 | 2 | 54 | 14 | 7 | 6 | 34 | 53 |
| 71 | 2557 | July | 10 | 1 | 26 | 22 | 0 | 13 | 24 | 11 | 7 | 3 | 43 | 30 |
| 72 | 2575 | July | 21 | 9 | 9 | 42 | 0 | 23 | 54 | 8 | 7 | 0 | 52 | 7 |
| 73 | 2593 | July | 31 | 16 | 53 | 2 | 1 | 4 | 24 | 5 | 6 | 28 | 0 | 44 |
| 74 | 2611 | Aug. | 12 | 0 | 36 | 22 | 1 | 14 | 54 | 2 | 6 | 25 | 9 | 21 |
| 75 | 2629 | Aug. | 22 | 8 | 19 | 42 | 1 | 25 | 23 | 59 | 6 | 22 | 17 | 58 |
| 76 | 2647 | Sept. | 3 | 16 | 3 | 2 | 2 | 5 | 53 | 56 | 6 | 19 | 26 | 35 |
| 77 | 2665 | Sept. | 12 | 2 | 23 | 46 | 2 | 16 | 23 | 53 | 6 | 16 | 35 | 12 |
| 0 | 2683 | Sept. | 24 | 7 | 29 | 42 | 2 | 26 | 53 | 50 | 6 | 13 | 43 | 39 |

T A B L E

T A B L E III.

The true time of new Moon, with the Sun's true distance from the Moon's ascending node, and the Moon's true latitude, at the true time of each periodical return of the Sun's eclipse, March 21st, 1764, old style, from the time of it's first coming upon the earth since the creation, till it falls right against the earth's center.

| Periods. | Years of Christ. | True time of new Moon. | | | | Sun's true Distance from the node. | | | Moon's true latitude. | | | |
|----------|------------------|------------------------|----|----|----|------------------------------------|----|----|-----------------------|----|--------|-------------|
| | | | | | | S. | o | i | o | i | o | |
| | | M. | D. | H. | s | | | | | | North. | |
| 0 | 1277 | June | 2 | 15 | 9 | 36 | 0 | 19 | 5 | 40 | 1 | 37 50 N. A. |
| 1 | 1295 | June | 13 | 21 | 54 | 32 | 0 | 18 | 40 | 54 | 1 | 33 45 N. A. |
| 2 | 1313 | June | 24 | 3 | 57 | 3 | 0 | 17 | 20 | 22 | 1 | 29 34 N. A. |
| 3 | 1331 | July | 5 | 10 | 42 | 8 | 0 | 16 | 29 | 35 | 1 | 25 20 N. A. |
| 4 | 1349 | July | 15 | 17 | 14 | 15 | 0 | 15 | 34 | 18 | 1 | 20 45 N. A. |
| 5 | 1367 | July | 26 | 23 | 49 | 24 | 0 | 14 | 46 | 8 | 1 | 16 39 N. A. |
| 6 | 1385 | Aug. | 6 | 6 | 41 | 17 | 0 | 13 | 59 | 43 | 1 | 12 43 N. A. |
| 7 | 1403 | Aug. | 17 | 13 | 32 | 19 | 0 | 13 | 16 | 44 | 1 | 9 3 N. A. |
| 8 | 1421 | Aug. | 27 | 20 | 30 | 17 | 0 | 12 | 37 | 4 | 1 | 5 42 N. A. |
| 9 | 1439 | Sept. | 8 | 3 | 51 | 46 | 0 | 12 | 1 | 54 | 1 | 2 41 N. A. |
| 10 | 1457 | Sept. | 18 | 10 | 23 | 11 | 0 | 11 | 30 | 27 | 0 | 58 53 N. A. |
| 11 | 1475 | Sept. | 29 | 17 | 57 | 7 | 0 | 11 | 3 | 56 | 0 | 57 43 N. A. |
| 12 | 1493 | Oct. | 10 | 1 | 44 | 3 | 0 | 10 | 41 | 55 | 0 | 55 49 N. A. |
| 13 | 1511 | Oct. | 21 | 9 | 29 | 53 | 0 | 10 | 25 | 11 | 0 | 54 28 N. A. |
| 14 | 1529 | Oct. | 31 | 17 | 9 | 18 | 0 | 10 | 11 | 27 | 0 | 53 12 N. A. |
| 15 | 1547 | Nov. | 12 | 0 | 51 | 25 | 0 | 10 | 1 | 10 | 0 | 52 19 N. A. |
| 16 | 1565 | Nov. | 22 | 8 | 54 | 56 | 0 | 9 | 51 | 49 | 0 | 51 46 N. A. |
| 17 | 1583 | Dec. | 3 | 16 | 48 | 17 | 0 | 9 | 48 | 4 | 0 | 51 11 N. A. |
| 18 | 1601 | Dec. | 14 | 0 | 51 | 5 | 0 | 9 | 43 | 42 | 0 | 50 49 N. A. |
| 19 | 1619 | Dec. | 25 | 8 | 54 | 59 | 0 | 9 | 40 | 23 | 0 | 50 31 N. A. |
| 20 | 1638 | Jan. | 4 | 16 | 56 | 1 | 0 | 9 | 34 | 57 | 0 | 50 3 N. A. |
| 21 | 1656 | Jan. | 15 | 0 | 54 | 41 | 0 | 9 | 29 | 24 | 0 | 49 57 N. A. |
| 22 | 1674 | Jan. | 26 | 8 | 48 | 24 | 0 | 9 | 19 | 44 | 0 | 48 44 N. A. |
| 23 | 1692 | Feb. | 6 | 16 | 36 | 28 | 0 | 9 | 8 | 58 | 0 | 47 49 N. A. |
| 24 | 1710 | Feb. | 17 | 8 | 8 | 37 | 0 | 8 | 54 | 20 | 0 | 45 43 N. A. |
| 25 | 1728 | Feb. | 28 | 7 | 43 | 40 | 0 | 8 | 34 | 53 | 0 | 44 52 N. A. |
| 26 | 1746 | Mar. | 10 | 15 | 14 | 33 | 0 | 8 | 10 | 38 | 0 | 43 46 N. A. |
| 27 | 1764 | Mar. | 20 | 22 | 30 | 26 | 0 | 7 | 42 | 14 | 0 | 40 18 N. A. |
| 28 | 1782 | April | 1 | 5 | 37 | 4 | 0 | 7 | 9 | 27 | 0 | 37 28 N. A. |
| 29 | 1800 | April | 11 | 12 | 36 | 38 | 0 | 6 | 35 | 30 | 0 | 34 31 N. A. |
| 30 | 1818 | April | 22 | 19 | 27 | 34 | 0 | 5 | 51 | 48 | 0 | 30 43 N. A. |
| 31 | 1836 | May | 3 | 2 | 12 | 7 | 0 | 5 | 5 | 5 | 0 | 26 40 N. A. |
| 32 | 1854 | May | 14 | 8 | 50 | 40 | 0 | 4 | 19 | 45 | 0 | 22 42 N. A. |
| 33 | 1872 | May | 24 | 15 | 28 | 15 | 0 | 3 | 26 | 3 | 0 | 18 1 N. A. |
| 34 | 1890 | June | 4 | 22 | 8 | 0 | 0 | 2 | 35 | 5 | 0 | 13 34 N. A. |
| 35 | 1908 | June | 15 | 4 | 38 | 23 | 0 | 1 | 41 | 43 | 0 | 8 54 N. A. |
| 36 | 1926 | June | 26 | 11 | 13 | 5 | 0 | 0 | 47 | 38 | 0 | 4 10 N. A. |
| 37 | 1944 | July | 16 | 17 | 50 | 35 | 11 | 29 | 55 | 28 | 0 | 0 24 S. A. |
| 38 | 1962 | July | 18 | 0 | 31 | 38 | 11 | 29 | 2 | 35 | 0 | 5 2 S. A. |

By the true motions of the Sun, Moon, and nodes, the Moon's shadow falls even with the earth's center two periods sooner than by their mean motions.

VOL. LIII.

L I

TABLE

T A B L E IV.

The true time of new Moon, with the Sun's true distance from the Moon's ascending Node, and the Moon's true latitude, at the true time of each periodical return of the Sun's eclipse, March 21st, 1764, old style, from the time of it's falling right against the earth's center, till it finally leaves the earth for upwards of 12,492 years.

| Periods. | Years of Christ. | True Time of new Moon. M. D. H. , " | Sun's true distance from the node. | | | | Moon's true latitude. | | |
|----------|------------------|--|------------------------------------|----|----|----|-----------------------|----|----------|
| | | | S. . , " | | | | South. | | |
| | | | M. | D. | H. | " | o | o | o |
| 39 | 1980 | July 28 7 18 53 | 11 | 28 | 11 | 32 | 0 | 9 | 29 S. A. |
| 40 | 1998 | Aug. 8 14 12 22 | 11 | 27 | 26 | 41 | 0 | 13 | 25 S. A. |
| 41 | 2016 | Aug. 18 21 14 53 | 11 | 26 | 42 | 16 | 0 | 17 | 18 S. A. |
| 42 | 2034 | Aug. 30 4 25 45 | 11 | 26 | 2 | 0 | 0 | 20 | 48 S. A. |
| 43 | 2052 | Sept. 9 11 45 17 | 11 | 25 | 26 | 46 | 0 | 23 | 53 S. A. |
| 44 | 2070 | Sept. 26 19 17 26 | 11 | 24 | 55 | 4 | 0 | 26 | 39 S. A. |
| 45 | 2088 | Oct. 1 2 57 8 | 11 | 24 | 27 | 43 | 0 | 28 | 58 S. A. |
| 46 | 2106 | Oct. 12 10 47 39 | 11 | 24 | 4 | 38 | 0 | 31 | 2 S. A. |
| 47 | 2124 | Oct. 22 18 37 39 | 11 | 23 | 48 | 28 | 0 | 32 | 26 S. A. |
| 48 | 2142 | Nov. 3 2 56 19 | 11 | 23 | 35 | 11 | 0 | 33 | 53 S. A. |
| 49 | 2160 | Nov. 13 11 11 20 | 11 | 23 | 22 | 22 | 0 | 34 | 42 S. A. |
| 50 | 2178 | Nov. 24 19 36 14 | 11 | 23 | 18 | 57 | 0 | 35 | 0 S. A. |
| 51 | 2196 | Dec. 5 4 4 9 | 11 | 23 | 14 | 40 | 0 | 35 | 22 S. A. |
| 52 | 2214 | Dec. 16 12 35 48 | 11 | 23 | 10 | 43 | 0 | 35 | 43 S. A. |
| 53 | 2232 | Dec. 26 20 29 9 | 11 | 23 | 6 | 47 | 0 | 36 | 1 S. A. |
| 54 | 2251 | Jan. 7 5 42 9 | 11 | 23 | 4 | 27 | 0 | 36 | 16 S. A. |
| 55 | 2269 | Jan. 17 14 14 8 | 11 | 23 | 0 | 41 | 0 | 36 | 35 S. A. |
| 56 | 2287 | Jan. 28 22 43 34 | 11 | 22 | 53 | 58 | 0 | 37 | 10 S. A. |
| 57 | 2305 | Feb. 8 7 8 30 | 11 | 22 | 44 | 44 | 0 | 37 | 59 S. A. |
| 58 | 2323 | Feb. 19 15 7 10 | 11 | 22 | 31 | 1 | 0 | 39 | 8 S. A. |
| 59 | 2341 | Mar. 2 0 6 5 | 11 | 22 | 17 | 46 | 0 | 40 | 28 S. A. |
| 60 | 2359 | Mar. 13 7 59 17 | 11 | 21 | 55 | 29 | 0 | 42 | 9 S. A. |
| 61 | 2377 | Mar. 23 15 51 59 | 11 | 21 | 39 | 40 | 0 | 43 | 41 S. A. |
| 62 | 2395 | April 3 23 45 7 | 11 | 21 | 0 | 53 | 0 | 46 | 58 S. A. |
| 63 | 2413 | April 14 7 32 40 | 11 | 20 | 26 | 22 | 0 | 49 | 48 S. A. |
| 64 | 2431 | April 25 15 12 57 | 11 | 19 | 47 | 34 | 0 | 53 | 17 S. A. |
| 65 | 2449 | May 5 22 45 14 | 11 | 19 | 6 | 22 | 0 | 56 | 50 S. A. |
| 66 | 2467 | May 17 6 17 30 | 11 | 18 | 21 | 16 | 1 | 0 | 40 S. A. |
| 67 | 2485 | May 27 13 46 30 | 11 | 07 | 34 | 20 | 1 | 4 | 42 S. A. |
| 68 | 2503 | June 7 21 10 31 | 11 | 16 | 43 | 17 | 1 | 9 | 3 S. A. |
| 69 | 2521 | June 18 4 24 42 | 11 | 15 | 51 | 48 | 1 | 13 | 26 S. A. |
| 70 | 2539 | June 29 11 58 46 | 11 | 15 | 1 | 12 | 1 | 17 | 43 S. A. |
| 71 | 2557 | July 9 19 24 7 | 11 | 14 | 9 | 13 | 1 | 22 | 6 S. A. |
| 72 | 2575 | July 21 2 52 34 | 11 | 13 | 19 | 22 | 1 | 26 | 16 S. A. |
| 73 | 2593 | July 31 10 25 31 | 11 | 12 | 13 | 43 | 1 | 31 | 44 S. A. |
| 74 | 2611 | Aug. 12 17 58 39 | 11 | 11 | 45 | 13 | 1 | 36 | 13 S. A. |
| 75 | 2629 | Aug. 22 1 41 37 | 11 | 11 | 1 | 49 | 1 | 39 | 50 S. A. |
| 76 | 2647 | Sept. 2 9 29 37 | 11 | 10 | 22 | 59 | 1 | 42 | 0 S. A. |
| 77 | 2665 | Sept. 22 17 25 23 | 11 | 9 | 46 | 48 | 1 | 45 | 45 S. A. |
| o | 2683 | Sept. 24 1 29 1 | 11 | 9 | 15 | 49 | 1 | 47 | 58 S. A. |

The true motions carry off the eclipse four periods sooner than the mean.

XXXIX. An

XXXIX. *An Account of an Earthquake at
Chattigaon: Translated from the Persian
by Mr. Edward Gulston, in the Service
of the Honourable East India Company,
and communicated by him to the Reverend
Mr. Hirst.*

To the Reverend Mr. HIRST.

Reverend Sir,

Read Nov. 17, 1763. THE following was written by a Persian writer, pursuant to an order of Harry Verelst Esquire, chief of the honorable East India company's province Chattgaon, in the kingdom of Bengal, and sent to Calcutta, for the information of messieurs Vansittart, Hastings, and others, acquainted with that language. As it is of indisputable authority, I have taken the pains to copy and translate it for your satisfaction, being,

Reverend Sir,

Your most obedient humble servant,

Calcutta, Nov. 1,
1762.

Edward Gulston.

ACCOUNT

A C C O U N T of an earthquake, which happened in the region of Islamabad on the 22d of the month Chytt 1168 Bengal æra, answering to the 2d of April 1762, on Friday about 5 o'clock in the afternoon, which, according to the best advices, I have written, and now send you.

Particulars are as follow;

The land of Mohamid Assad Chowdhry of the Pargannah Deeâng, at a place called Barâeah, is laid open by the shock from 10 to 12 cubits in width, and become, as it were, a deep creek; the water rising up so, that the ground of the farmers inhabiting the place is 8 cubits overflowed.

And at Deep in the chowdhrâij of Mohamid Athijâr the like hath come to pass.

And Moktarâm Fowtahdar, dwelling at Goyparah, has written, that to the north and east his house was cracked, and water there spouted up like a fountain, and the ground also sinks every day by little and little.

And by letter from Satoo Meſter Daroogah of the salt-works at Bansbareeâh, it so fell out, that, to the westward, Akl'poorah, an island of the salt-works, was levelled with the water on its east side, and on the north and south the ground opened from 5 to 7 cubits in width, and sunk like a pit to the depth of 10 cubits, the water spouting up; nor is there the least appearance of it's subsiding: we know not what will come of it.

And from the reports of the people there we hear, that these places were never before overflowed by the water,

water: we cannot at present tell what misfortune has happened. However all the government's salt was before this laid up in storehouses. Moreover, a mud-building of your servant's (the writer of this account) was almost destroyed by the shock, but it still stands upright.

And at Haldah about 12 doan of land belonging to Sacheeram Cannoongōeij is entirely sunk into the water.

In like manner in Takaleeah, about 5 doan of ground, the property of Barjallāāl Chowdhry has fallen something below its primitive level.

And at Do Hâzâry, Harry Singh's house, and a brick'd building of Sheer Zaman CHan's came down, and the CHan was hurt by the fall of his; and there opened a cavity like a ditch of 200 cubits in length, which filled with water.

At Howlâ, the house of Shiam Ram taxgatherer, broke down, and his whole inclosure was torn up, and in most places his house and fish-ponds were filled with sand-banks: even now the whole spot is two cubits under water.

And, at Dahrampoor, the house of Santeeram, the Cannoon-Goeys writer, intirely fell down.

The Kutwâll, of Islamabad, also informed us with his own mouth, that, in a place called Baramcharah, the water was up to a man's waist, and the people there have betaken themselves to flight, through fear of perishing; no living creature but the cattle now remaining.

And in the house of Santeeram Cannoon-Goey of Islamabad, a bricked room was ruined; and one of his

his brethren, named Rajah Ram, killed by the fall of the bricks.

And the house of Nandaram coming down in the same manner, a son of his was knocked of the head.

And to the eastward of Kadr Katcheeah a large hill, called Kaddaleah, very near Karn Phooly, was rent, and it stopped up the passage for boats in and out that river.

And at Bajaleeah, Sangotty and Do Hazary creeks were closed up by banks of sand rising from their bottom.

And at Gandarab Jowar, about 3 doan of ground belonging to Mohamid Aly Chowdhry, rent and was swallowed up, and the passage in and out to his house also cleaving asunder; the water rose up and has flowed all round the house.

Moreover (the factory house) a strong building in the fort of Islamâbad cracked from top to bottom and tumbled down, and an apartment newly built was also rent.

And to the eastward a large pond of Bilah CHan became a deep gulph ; and to the east also of Aghy Gange, belonging to the city of Islamâbad, the ground in different places clave asunder, water rising up as from so many springs.

And at Chehpâijttee about 12 kâty of land belonging to Shâh Sâgier Chowdry was overflowed and rendered unfit for tillage.

And, by letter from Chéhtarnarâijn surveyor of the lands, we learn, that the north side of the Chachlah Sowabeel, just by Haldah river, broke down and is

is swallowed up by the river, and also four people were overwhelmed in it's ruins.

And Mr. Griffith's bricked house (in Islamabad) has been cracked, also the house and walls of Juan de Baris, a Portuguese, here.

And from Nahar Charah there is news, that the greater part of the ground of that island clave asunder, and is swallowed up by the waters, and a number of people perished with it. Besides this, the state of that island will be known to you from a Bengal account.

From the Jooms, whose country is about 4 days off from Islamabad, we learn, that Reang Hill split in two and sunk 40 cubits; also that Kachalang Hill is even with the ground.

And Bahngoo Changee, a Joom hill, rent in twain, and is sunk 30 cubits, and the houses of most of the inhabitants in those parts thrown down.

And a Joom hill Chahter Páttuah split by little and little, till it is almost level with the plain: and because of the opening of the hills, and destruction of the trees on them, the way by which the Jooms used to pass is stopeed up.

And Bajaleeah, another Joom hill upon the river, opened 30 cubits, and sinking water rose up; and Palang, a Joom hill, split and sunk 25 cubits.

The design of this is to lay before you the wonderful disorders, that have come to pass in these regions, and which continue to happen, insomuch that from the time of Adam untill now, in this place, no one has heard of the like.

If

If I should describe them with a thousand instances and relations, and make mention of so many particulars, still there would not be a part in ten that I could bring within the compass of writing. But these few particulars I send for your excellency's information.

*XL. An Account of an Earthquake in the
East Indies, of two Eclipses of the Sun and
Moon, observed at Calcutta: In a Letter
to the Reverend Thomas Birch, D. D.
Secret. R. S. from the Reverend William
Hirst, M. A. F. R. S.*

*To the Reverend Thomas Birch, D. D. Secretary to
the Royal Society.*

Reverend Sir,

Calcutta, Nov. 3d. 1762.

Read Nov. 17, 1763. **T**O the inclosed accounts of the transit of Venus, I have subjoined others of an extraordinary earthquake felt in this part of the world, which, I flatter myself, will not be unacceptable to the Royal Society. This earthquake happened the second day of April last, was very violent in the kingdoms of Bengal, Aracan, and Pegu, but particularly at the metropolis of Aracan, where, according to the accounts of an English merchant residing there, the effects have been as fatal as at Lisbon,

bon, and where it is thought the chief force of the earthquake vented itself.

At Dacca, in this kingdom of Bengal, the consequences have been terrible : the rise of the waters in the river was so very sudden and violent, that some hundreds of large country boats were driven ashore, or lost, and great numbers of lives lost in them.

No less deplorable are the accounts from Chittagaoon in this same kingdom : three of these accounts I herewith inclose, one of them wrote by Mr. Edward Gulston, a young gentleman in the service of our East India Company, and two others, translations from a Persian original, made out by order of Mr. Vérelst, chief of our East India Company's affairs in that province ; in consequence of which account the Company's lands there, have not been so highly assessed as before this calamity. Both these accounts are translated from the same original ; but that, which I received from governor Vansittart, being thought exaggerated for interested reasons, I begged of Mr. Gulston to give me a litteral translation from the Persian, in which language he has made an uncommon progres, as much to his present honour, as I hope it will be to his future advantage. This favour he obligingly granted me, and I send it to you, Sir, not only to compare it with the other translation, but to give you some distant idea of the idiom and great simplicity of this eastern language.

The same earthquake was also very alarming at Ghirotty, where colonel Coote with His Majesty's troops are in cantonment about 18 miles up the river from this place. The waters in the river and tanks there were violently agitated, and, in many places,

rose to more than six feet perpendicular height, of which I had ocular conviction myself on my return from Chandernagore, a settlement lately belonging to the French, about three miles north from Ghirotty, and in latitude $22^{\circ} 54'$ N. where it was felt, but not in a great degree; for I myself knew nothing of it, till it was soon after told me by certain French gentlemen there.

Nearly at the same time was this earthquake felt at Calcutta, where, as I am informed, the agitation of the waters in the tanks rose upwards of six feet, and was in the direction north and south. The height of the thermometer on Farenheit's scale was then at Calcutta at $95^{\circ} 30'$ much higher than it had been observed to be during the whole month, the lowest descent of the mercury being 89 degrees. In this month was much thunder and lightening, and there were fresh gales of wind at S.E. the weather in general being close and sultry.

A subsequent earthquake was felt at Calcutta the 13th of July following at half past two in the afternoon. The thermometer was then at $87^{\circ} 4'$ at a medium, the wind S. W. and the weather fair: to this I was a witness myself, being then at dinner with captain Eiser, of his majesty's 84th regiment. The motion of the earth caused a very sensible vibration of the wine in our glasses, and the shock was repeated twice at the interval of a few seconds.

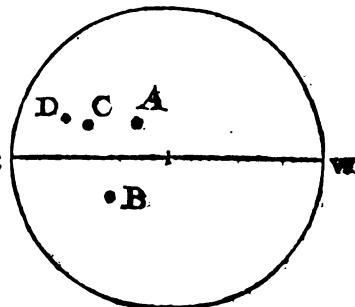
I conclude, Sir, with communicating to you the observations I made in these parts, of two remarkable eclipses of the Sun and Moon. The first was of the Sun, which I observed at Ghirotty on the banks of the Ganges, October 17th ult. where I

was then on a visit to colonel Coote. This indeed was a flying observation, or taken, as the French would say, en passant, being unprovided with a necessary apparatus. I had luckily adjusted (very carefully) my watch to apparent time, by the meridian line of a large sundial, on the noon immediately preceding the eclipse: my watch in general keeps time very well, but it not having a hand to shew seconds, I determined the seconds as near as I could by the minute hand. Though I had set my watch to the apparent time, I despaired of making any observation of this eclipse for want of a telescope, for which, happening to express some concern, not above half an hour before the eclipse was to come on, captain Eisler recollect'd he had a reflector, with which he immediately obliged me: it was about 16 inches long, and in very tolerable condition, so that I may venture to say this observation, though not perfect, may be depended on to be very near the truth.

I had not time nor conveniency to throw the Sun's image on a screen in a darkened room; so was obliged to lay it down as near as I could by my eye. The following scheme shews the solar maculæ, as they then appeared: the appulse of the Moon's limb to, and it's recess from them, being respectively noted by the literal references.

N. B. I examined my watch by the meridian line the succeeding noon of the eclipse, without being able to ascertain any sensible error, owing doubtless to the want of a better method of making the observations.

Observations of a solar eclipse made at Ghyrotty about the latitude $22^{\circ} 51'$ north, the watch adjusted to apparent time.



| | H | M | S |
|---|---|----|----|
| Beginning of the lunar immersion | 2 | 49 | 30 |
| Appulse of the Moon's eastern limb to the spot A | 3 | 35 | 48 |
| Appulse of the same to the spot B | 3 | 41 | 42 |
| Appulse of the same to the spot C | 3 | 49 | 43 |
| Appulse of the same to the spot D | 3 | 53 | 0 |
| Greater visible obscurity near 11 degits eclipsed | 4 | 5 | 38 |
| The Moon's western limb receding from the spot A | 4 | 46 | 50 |
| The same receding from the spot B | 4 | 50 | 20 |
| The same receding from the spot C | 5 | 0 | 8 |
| The same receding from the spot D | 5 | 3 | 35 |
| End of the eclipse | 5 | 12 | 20 |
| Total duration | 2 | 22 | 50 |

The next observation was of the eclipse of the Moon, which I made yesterday in Conjunction with Mr. Hancock, at his house in Calcutta, to whom I am greatly obliged for supplying me with some excellent astronomical instruments, particularly with a large land quadrant of two feet radius, made by Cole in Fleet-street, with which I took the correspondent

pondent altitudes of the Sun to adjust his watch (which was furnished with a hand to distinguish seconds) to the apparent time. Mr. Hancock himself marking the Times while I observed. The telescope I used was a reflector made by Dollond, in perfect order, being sent out of England by the last ships, and in length about 22 inches.

Our first observation was on November the first, the day preceding the eclipse, the Sun's upper limb being at the horizontal wire of the quadrant's movable telescope, on the eastern side of the

| | | By the watch |
|---|---|--------------|
| | | H M S |
| meridian when the watch marked - - - - | | 9 50 30 |
| Nov. 1. Sun's upper limb at the same on the | | 14 24 31 |
| Altitude, western side - - - - - | | <hr/> |
| 47° 8' | Dividing the Sum by - - - - - | 2/24 15 1 |
| | | <hr/> |
| Sun's center on the merid. by the watch | | 12 7 30 |
| Equation of the day - - - - - | | II 43 49 |
| | | <hr/> |
| Watch faster than equated solar time - | | 0 23 41 |
| Nov. 2, | Sun's upper limb at the horizontal wire | 10 24 33 |
| Altitude, on the eastern side - - - - | | <hr/> |
| 51° 26'. | The same at the same on the west side - | 13 59 50 |
| | | <hr/> |
| Dividing the Sun by - - - - - 2 | | 24 24 23 |
| | | <hr/> |
| Sun's center on the merid. by the watch | | 12 12 11 |
| Equation of the day - - - - - | | II 43 48 |
| | | <hr/> |
| Watch faster than equated solar time - | | 0 28 23 |
| Watch faster yesterday - - - - - | | 0 23 41 |
| | | <hr/> |
| Gain of the watch these 24 hours - - | | 0 4 42 |

Observations

Observation of a lunar eclipse November 2, 1762,
made at Calcutta in the kingdom of Bengal, latitude
 $22^{\circ} 30' N.$

| Immersions. | By the watch. | | | Apparent time. | | |
|--|------------------|------|---------|-------------------|----|----|
| | H | M | S | H | M | S |
| The beginning of the eclipse at | - | - | 1 15 10 | 1 | 7 | 40 |
| Mare Humorum immersing | - | - | 1 24 54 | 1 | 17 | 24 |
| Tycho immersing | - | - | 1 36 30 | 1 | 29 | 0 |
| The shadow at the middle of Copernicus | 2 | 9 33 | 2 | 2 | 3 | 3 |

Emerfions.

| | | | | |
|-------------------------------|---|---------|---------|----------|
| Middle of Copernicus emerging | - | 2 50 27 | 2 42 57 | |
| Total emerſion of Tycho | - | 3 44 30 | 3 37 0 | doubtful |
| End of the eclipse | - | 4 3 2 | 3 55 32 | |
| Total duration | - | 2 47 52 | 2 40 22 | |

Near eight digits eclipsed by ocular estimation.
By the preceding observations the watch gained
4 minutes 42 seconds these 24 hours.

I have the honour to be,

S I R,

Your obliged and very humble servant,

William Hirst.

XLI. Extract

XLI. Extract of a Letter from Mr. Edward Gulston, at Chittigong, to Major John Carnac, at Calcutta.

Dear Sir,

Read Nov. 17, 1763. THE reason principally of this address is to give you a particular account of the shocks of a violent earthquake, which were felt here on the 2d instant at 5 in the afternoon lasting the space of four minutes. The factory, a brick building, is totally spoiled, so as not to be safely habitable; for thereabouts, and in many other places, the earth opened, and the waters gushed out prodigiously; and in the chaise-road, especially towards the north quarter, there are great chasms two feet wide and upwards, so strange, that the morning after, riding that way, the horse started and went round another way, not willing to go over them.

At the time of the first shake, great explosions were heard like the noise of cannons, of which Mr. Plaisted and others counted 15.

All the tanks overflowed their banks, fish were cast up, and the river rushed upon the shore like the surf of the sea. It was the most extraordinary event I was ever witness to: by the enclosed paper you will discern how many alarms we had, however nothing equal to the first, in which the whole force of the earthquake seems to be exerted. At present, the afternoon of the 4th of April, all our heads seem to be quiet and still, and consequently the earth at rest; but really

really yesterday, from the repeated tremours of the ground, every one appeared giddy and alarmed, fancying the earth to be in perpetual vibration, which however an experiment of a glass of water upon the floor by no means admitted of. I would not that such a shock as the first should happen at Calcutta for all I am worth, since of necessity the terrassed houses must fall to ruin, and I please myself with the thoughts, that we have had the worst of it.

Chittigong, April 4, 1762.

I am, &c.

Copy of the Paper mentioned in the foregoing letter.

Chittigong, April 2, 1762.

April 2 at 5 o'clock P.M. a severe shock of an earthquake lasted 4 minutes.

5 12 a second lasted one minute.

5 30 a third.

7 o a fourth.

10 o a fifth.

1 o in the morning of the 3d, a sixth.

2 o a seventh.

3 o an eighth.

5 o a ninth.

10 25 a tenth.

10 30 an eleventh.

Between 6 and 7 in the evening I felt a twelfth shock; also others upon Marriet's hill, at a distance from mount Pleasant, which every one thought in continual motion.

XLII. An Account of the Earthquakes that have been felt in the Province of Islamabad, with the Damages attending them, from the 2d to the 19th of April, 1762: Translated from the Persian, and communicated to Henry Vansittart, Esq; President and Governor of Fort William in Bengal, by Mr. Verelst, Chief of the Hon. East India Company's Affairs at Islamabad.

Read Nov. 17, 1763. **T**HE weather being very close and warm for some days preceding, on the 2d of April, about 5 in the afternoon, we were alarmed by an earthquake; which beginning with a gentle emotion, increased to so violent a degree, for about two minutes, that the trees, hills, and houses shook so severely, that it was with difficulty many could keep their feet, and some of the black people were thrown on the ground; whose fears operated so powerfully, that they died on the spot; others again were so greatly affected, that they have not recovered themselves since.

On the plains, by the rivers, and near the sea, it was chiefly felt with great severity.

Our bungaloes proved very convenient on so melancholy an occasion; for had we been in brick houses, they must inevitably have been shattered or levelled with the ground; as there is not a brick wall or house but is either greatly damaged or fallen.

N n

Our

Our new room in the fort, though as strong as bricks and chunam could make it, is shivered on all sides from bottom to top ; and the old building equally cracked is in great part tumbled down.

The ground opened in several places in the town, throwing up water of a very sulphurous smell ; and several ditches and tanks were filled up, which are now level dry land.

The emotions were so complicated, that we could not well determine their direction ; being sometimes from west to east, and again from east to west ; and the tanks in some places overflowed north and south.

In Purgunnah Deang, Bursea Gong, the ground in several places opened ten and twelve cubits wide ; and in some parts so deep, that they could not fathom its bottom ; the water immediately overflowing the whole town, which is sunk about seven cubits.

Deep Gong, a village near the other, is also sunk, and now lies seven cubits under water.

From Patter Gottah to Howlah, about 8 cefs distance, the ground opened, and a great quantity of water was immediately thrown out, and in several places the ground entirely sunk.

At Bans Burreah, Akul Poor, near the sea, the earth opened in seven places, like wells, throwing up the water ten cubits high : the great Cutcherry there, with brick walls, is cracked and shivered to pieces.

At Hulda Creek, near Sancharam Conguy's house, twelve don of ground is entirely sunk.

In the Purgunnah Do Hazarree, Hurry Sing Hazarree's brick house was entirely thrown down : the hall of Seer Jumma Cawn's briek house also fell, and himself

himself was greatly hurt by the bricks : near which the ground opened 200 cubits, and immediately filled with water, which is now unfathomable.

In Howla Purgunnah, Sam Roy Gaffildar's house broke down, and his compound was filled with water of two cubits deep for two days.

In Berrum Cherra, the ground overflowed about two cubits deep.

The hall of Santaram Conguy's brick house fell down, and killed one of his relations.

Near Cutcha Gaut, Kurrolea hill opened, and a great part of it fell into the river.

Bazally Creek, and Do Hazarry Creek, are both stopped up.

At Gunderub Juwar, three don of ground is entirely funk.

Ali Chowdry's compound opened, and the water, that immediately flowed out, filled a deep ditch, that surrounded his house.

From Sawabill Purgunnah to Mooradabad, three Taluckdar's grounds are entirely funk, and four people killed.

At Bar Chara, near the sea, five or six eess of ground immediately funk, and out of four or five hundred people, above two hundred were lost, with all their cattle ; and the greatest part of the remaining inhabitants, who ran into the woods, have not yet been heard of.

Nulla Nundaram's brick house was broken down ; and his son, who was then in it, was so much bruised, that he died in three days afterwards.

At Lafettee Silcope Chuckla the ground in forme places opened, and threw up great quantities of salt

N n 2 water,

water, and in others entirely sunk : the channels of several creeks and little vallies between the hills were filled up with great quantities of sand : in some parts the water still continues twenty cubits deep, and in others unfathomable.

Silluk creak, and Issamuttee river are both stopped up ; several boats laden with goods then coming down are not now able to get out of them : the country around there opened greatly in some places, and in others entirely sunk ; and a great many tanks filled with sand.

Bur Coller hill opened about forty cubits wide.

Cess Lung Joom hill, one of the Mug mountains, is entirely sunk.

Chunggee hill opened between twenty and thirty cubits.

Puddooah creek, at that time without water, opened, and threw up two hills of sand ; and all the houses in these parts were broke down.

Joom Chater Pedea hill, is sunk so low, that its top is now on a level with the plains.

Rigerree hill, which was very large, opened thirty cubits wide.

Joom Palang hill opened twenty-five cubits.

By the accounts already come in, there are 120 * Dons of ground lost in different parts of the province; but these I am afraid will not be one eighth part of the whole damages, as we have further relations coming in every hour.

* One sye don of ground is 1920 cubits long, and 1600 cubits broad.

As

As we are informed, that there are two volcanoes opened, I am in great hopes these will prove a sufficient vent to discharge all the remaining sulphurous matter in the bowels of these countries, and put a stop to any further earthquakes here; at least for many years to come.

*XLIII. A Letter from the late Reverend Mr.
Thomas Bayes, F. R. S. to John Canton,
M. A. and F. R. S.*

S I R,

Read Nov. 24, 1763. If the following observations do not seem to you to be too minute, I should esteem it as a favour, if you would please to communicate them to the Royal Society.

It has been asserted by some eminent mathematicians, that the sum of the logarithms of the numbers 1.2.3.4. &c. to z , is equal to $\frac{1}{2} \log. c + z + \frac{1}{2} \times \log. z$ lessened by the series $z - \frac{1}{12z} + \frac{1}{360z^3} - \frac{1}{1260z^5} + \frac{1}{1680z^7} - \frac{1}{1188z^9} + \dots$ &c. if c denote the circumference of a circle whose radius is unity. And it is true that this expression will very nearly approach to the value of that sum when z is large, and you take in only a proper number of the first terms of the foregoing series: but the whole series can never properly express

pres any quantity at all ; because after the 5th term the coefficients begin to increase, and they afterwards increase at a greater rate than what can be compensated by the increase of the powers of z , though z represent a number ever so large ; as will be evident by considering the following manner in which the coefficients of that series may be formed. Take $a = \frac{1}{\pi}$, $5b = a^2$, $7c = 2ba$, $9d = 2ca + b^2$, $11e = 2da + 2cb$, $13f = 2ea + 2db + c^2$, $15g = 2fa + 2eb + 2dc$, and so on ; then take $A = a$, $B = 2b$, $C = 2 \times 3 \times 4c$, $D = 2 \times 3 \times 4 \times 5 \times 6d$, $E = 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8e$ and so on, and $A, B, C, D, E, F, \&c.$ will be the coefficients of the foregoing series : from whence it easily follows, that if any term in the series after the 3 first be called y , and its distance from the first term n , the next term immediately following will be greater than $\frac{n \times 2^{n-1}}{6^n + 9} \times \frac{y}{z^n}$. Wherefore at length the subsequent terms of this series are greater than the preceding ones, and increase in infinitum, and therefore the whole series can have no ultimate value whatsoever.

Much less can that series have any ultimate value, which is deduced from it by taking $z = 1$, and is supposed to be equal to the logarithm of the square root of the periphery of a circle whose radius is unity ; and what is said concerning the foregoing series is true, and appears to be so, much in the same manner, concerning the series for finding out the sum of the logarithms of the odd numbers 3. 5. 7. &c.... z , and those that are given for finding out the sum of the infinite progressions, in which the several terms have the same numerator whilst their denominators are

are any certain power of numbers increasing in arithmetical proportion. But it is needless particularly to insist upon these, because one instance is sufficient to shew that those methods are not to be depended upon, from which a conclusion follows that is not exact.

*XLIV. An Account of the Insect called the Vegetable Fly: by William Watson, M.D.
F. R. S.*

To the Royal Society.

Gentlemen,

Read Nov. 24.
1763. THE beginning of last month, I received a letter from our learned and ingenious member Dr. Huxham of Plymouth; in which among other things he informed me, that he lately had, by permission of commissioner Rogers, obtained a sight of what is called the *vegetable fly*, with the following description of it; both which he had from Mr. Newman, an officer of general Durore's regiment, who came from the island *Dominica*. As this description seemed to the doctor exceedingly curious, he has sent it me, exactly transcribed from Mr. Newman's account, and is as follows.

“ The *vegetable fly* is found in the island *Dominica*,
“ and (excepting that it has no wings) resembles the
“ drone both in size and colour more than any other
“ English insect. In the month of May it buries itself
“ in

" in the earth, and begins to vegetate. By the latter
 " end of July the tree is arrived at it's full growth,
 " and resembles a coral branch; and is about three
 " inches high, and bears several little pods, which
 " dropping off become worms, and from thence
 " flies, like the English caterpillar." An account of
 this extraordinary production, similar to the above,
 was given to Dr. Huxham by captain Gascoign, who
 lately commanded the Dublin man of war, which
 hath been at Dominica. The doctor subjoins, that
 possibly I may have heard of this fly; or seen it in
 the collections of the British Museum, or Royal So-
 ciety; but, if it is in neither, he believes he can pro-
 cure it to be sent to the Royal Society.

Though the doctor can by no means think the
 above relation true in all it's circumstances, yet he
 is persuaded there is something of reality in it; which
 perhaps further accounts and observations may set
 in a full and true light: though at present, as repre-
 sented, it seems quite repugnant to the usual order of
 nature.

As I had never seen this production myself, but
 had been informed that doctor Hill had had the ex-
 amination of some of them, I wrote to that gen-
 tleman to desire to be informed of the result of his
 enquiries. To which he very obligingly sent me the
 following answer.

" When colonel Melvil brought these flies from
 " Guadalupe, lord Bute sent me the box of them to
 " examine. The result was this. There is in Mar-
 " tinique a fungus of the Clavaria kind, different
 " in species from those hitherto known. It produces
 " soboles from its sides. I called it therefore Cla-
 " varia

“ varia Sobolifera. It grows on putrid animal bodies,
 “ as our fungus ex pede equino from the dead horses
 “ hoof.

“ The Cicada is common in Martinique, and in
 “ its nympha state, in which the old authors call it
 “ Tettigometra, it buries itself under dead leaves to
 “ wait it's change ; and when the season is unfavour-
 “ able, many perish. The seeds of the Clavaria find a
 “ proper bed on this dead insect, and grow.

“ The Tettigometra is among the Cicadæ in the
 “ British Museum : the Clavaria is just now known.
 “ This you may be assured is the fact, and all the
 “ fact ; though the untaught inhabitants suppose a
 “ fly to vegetate ; and though there exists a Spanish
 “ drawing of the plant's growing into a tri-foliate tree;
 “ and it has been figured with the creature flying
 “ with this tree upon its back.

“ So wild are the imaginations of Man ; so chaste
 “ and uniform is Nature !”

Commissioner Rogers, at Dr. Huxham's desire,
 has presented this extraordinary production to the
 Royal Society, and it now lies before you.

A careful examination of it seems to confirm, to
 me at least, Dr. Hill's opinion of the manner of this
 phænomenon's being produced.

The ingenious Mr. * Edwards has taken notice of
 this extraordinary production, in his *Gleanings of Natural History*, and has given us a figure of it in that
 elegant work.

There is in the British Museum among the Ci-
 cadæ one, nearly resembling the animal part of the

* Vol. III. page 262, plate 335.

production before you; but it came from the East Indies. There is likewise from the West Indies, in its perfect or winged state, the insect, of which this production is believed to be the nympha. [Vid. TAB. XXIII.]

I am with all possible regard,

Gentlemen,

Your most obedient humble servant,

Lincoln's-Inn Fields,
Nov. 15, 1763.

William Watson.

XLV. *An Attempt to explain a Punic Inscription, lately discovered in the Island of Malta. In a Letter to the Reverend Thomas Birch, D. D. Secret. R. S. from the Reverend John Swinton, B. D. of Christ-Church, Oxon. F. R. S. and Member of the Etruscan Academy of Cortona in Tuscany.*

Good Sir,

Read Nov. 24, 1763. I Received some months since from the Honourable Mr. Lyttelton of Christ-Church, son to the Right Honourable the Lord Lyttelton, a copy of a Punic inscription, lately discovered in the island of Malta, sent me from Rome by Sig. Abate Venuti, antiquary to the Pope, and a gentleman of profound erudition. This copy was inclosed in a letter to the Right Reverend the Lord Bishop of Carlisle,

Carlisle, who was so good as to transmit it to me at Oxford. The inscription has been mentioned, but not explained, by M. l'Abbé Barthelemy¹, in the *Journal des Scavans*, who has deduced a new Phœnician alphabet from it; though he seems to doubt whether any of the transcripts that had appeared, at least any of those he had seen, agreed perfectly in all particulars with the autograph itself. However, from the known accuracy of Sig. Abate Venuti, I think we may venture to suppose the copy now sent you to be in the main sufficiently exact. I shall therefore, at the request of several friends, submit to the consideration of the Royal Society a few cursory remarks upon this curious monument of antiquity; especially, as it has not yet in a proper manner been communicated to the learned world.

I.

The three first letters undoubtedly form the Hebrew word חֶדֶר, PENETRALE, CONCLAVE, INTIMVS RECESSVS, &c. for a farther account of which, recourse may be had to the² Hebrew lexicographers.

The next two elements seem to be Beth and Yod, of which is composed the Phœnician word בַּת, probably the same with the Hebrew בָּת, DOMVS; as the Phœnicians not seldom omitted, or suppressed, the letter Jod. This most evidently appears from צְרוּם, צְרוּם, &c. for צִדְוִינִים, צִדְוִינִים, &c. ex-

¹ *Journal des Scavans, Suite de Decembre 1761.* p. 82, 83, 84. A Amsterdam, 1761.

² Val. Schind. Jo. Buxtorf. Christian. Stock. Jo. Leonhard. Reckenberg. aliquic plur. lexicograph. Hebr.

hibited by the Tyrian and Sidonian coins. The form of the *Thau* here seems to indicate the inscription to be of a later date. This character bears some resemblance to the figure of *Tzade*, preserved on certain medals of Tyre and Sidon; though these two, whatever may have been insinuated to the contrary by a writer of considerable note, are sufficiently distinguishable from each other.

The three following letters present to our view the Hebrew word סְכָלֵם, SECVLVM, AETERNITAS, PERPETVITAS, DVRATIO HOMINIBVS ABSCONDITA, &c. Not the least difficulty occurs here.

The three preceding letters are succeeded by *Koph*, *Beth*, and *Resch*, forming the noun קְבָרָה, SEPVL-CHRVM; which, with the introduction just explained, sufficiently points out to us the nature of the inscription I am now upon.

The four next Phœnician elements answer to the Hebrew נְגַנּוּל, DEPOSITVS. The true signification of the term, as used here, is preserved in the ³ Syriac.

With regard to the following word נְקַה, CLARVS, INNOCENS, IVSTVS, &c. I shall only beg leave to observe, that it cannot well be misunderstood. It will be almost superfluous to remark, that both this and the preceding word assume the nature of substantives here; the term שִׁיר, VIR, by a most common ellipsis, being suppressed.

The four following characters combined produce the Hebrew בְּכָלַח, CONSVMMATIONIBVS, OMNINO, PENITVS, &c. The reality of this word, from what

³ Buxtorf. *Lex. Chaldaic. & Syriac.* p. 97. Basileæ, 1622.

has been laid down by the Hebrew * lexicographers, may be most clearly evinced.

The letters *He*, *Zain*, *He*, seem to constitute the participle הַיְן, DORMIENS, DECUMBENS, &c. M. l'Abbé Barthelemy, unless I am deceived, takes the second of these elements for *Jod*. But this will neither be admitted by the form itself, nor the tenor of the inscription. The small stroke, or scratch, above this character, seems to be only an accidental blemish, occasioned by the injuries of time.

The participle סָמֵךְ, VEHEMENTER AMANS, or INTIME DILIGENS, probably begins a new sentence. Some doubts may perhaps arise about the power of the first character. However, after the closest examination of the inscription, it appears to me to be certainly *Resch*.

The verb נִנְבַּת, TREMVIT, or COMMOTVS EST, immediately follows. This Chaldee word may likewise be rendered MAGNO CVM AFFECTV MOTVS EST, and deduced from the Arabic, according to Maius.

The substantive סָמֵךְ, POPVLVS, which immediately follows, comes in appositely enough here. The Carthaginians sometimes used the word סָמֵךְ in the same, or at least an extremely similar, sense. This appears from some of the medals * of Menæ, now called Menéo, an ancient town of Sicily, subject to the Carthaginians; on which we find מְחַנֵּת סָמֵךְ, PO-

* Val. Schind. *Lex. Pentaglot.* p. 866. Hanoviæ, 1612. Christ. Stock. *Clav. Ling. Sanct. Vet. Test.* p. 528, 529. Jenæ, 1727. Jo. Leonhard. Reckenberg. *Lib. Radic. sive. Lex. Hebraic.* p. 777. Jenæ, 1749.

⁵ Maius, apud. Jo. Leonhard. Reckenberg. ubi sup. p. 1386.

⁶ *Numism. Antiqu. &c.* à Thom. Pembr. et Mont. Gomer. Com. Collect. P. 2. T. 87.

PVLVS MENARVM, or POPVLVS MENENIVS, to omit others that might with equal facility be produced, as I have many years since observed. For a farther account of בָּנָן, I must beg leave to refer the curious to the Hebrew lexicographers, and particularly to Maius⁷.

The next word בָּשָׂת, IN PONENDO, or * rather QVVM PONERETVR, (i. e. לְאַרְצָה, or לְאַרְצָה, IN SEPVLCHRO, or IN TERRA) occurs in the very same sense, PSAL. xlix. 15. which passage throws considerable light upon this part of the inscription. That the Punic dialect of the Phœnician, the language of our inscription, was not without such ellipses as that mentioned here, must be allowed probable enough, if Bochart's Latin version of the Punic words in Plautus may be considered as not very remote from truth.

The three last words of the inscription are apparently חֲנִיבָעֵל בֶּן בָּרְמָלֵךְ, HANNIBAL FILIVS BARMELEC, BARMILC, BORMILC, OR BARMELECI. As the letter ר in the conversion of Oriental words into Greek is sometimes lost, the Carthaginian name BARMELEC, or BORMILC, might have been pronounced BOMILC (and perhaps BOMILCAR) both by the Greeks and the Romans. For that the genuine Carthaginian names, when either written or pronounced by the individuals of those nations, were not a little corrupted and depraved, I think we have no manner of reason to doubt.

⁷ Maius, apud Jo. Leonh. Reckenberg. ubi sup. p. 51.

* Vid. Stock. et Reckenberg. in vocib. שָׁוֹת et שָׁתָּה.

⁸ Boch. Chan. Lib. II. c. vi.

⁹ Id. ibid. c. vii, viii, xi.

II. From

II.

From the foregoing observations it most evidently appears, that the following arrangement of the words forming this inscription may be considered as not very remote from truth.

חֶדֶר נָתַע עַל־סְמִינָה
נָקָה בְּכָלָת הַזָּהָר
סְמִינָה אֲמֵם כְּשַׁת חַנְבָּה
עַל־בָּן בָּרְמָלֵךְ

The Latin and English versions of which words may, as I conceive, be appositely enough drawn up in the following terms.

PENETRALE DOMVS SECVLVI (five DOMVS PERPETVÆ)—SEPVLCRVM DEPOSITI (hic) CLARI (viri) CONSVMMATIONIBVS (i.e. OMNINO, PLANE, vel ARCTISSIME) DORMIENTIS—INTIME DILIGENS (eum) COMMOTVS (est) POPVLVS QVVM PONERETVR (scil. IN TERRA i.e. SEPELIRETVR) HANNIBAL BARMELEC (BARMILC BORMILC vel BARMELECI) FILIVS.

THE INTERIOR PART OF THE HOUSE OF LONG DURATION (or LONG HOME i.e. THE GRAVE)—THE SEPULCHRE OF AN UPRIGHT MAN DEPOSITED (here) IN A MOST SOUND (or DEAD) SLEEP—THE PEOPLE HAVING A GREAT AFFECTION FOR HIM WERE VASTLY CONCERNED WHEN HANNIBAL THE SON OF BARMELEC (BARMILC OR BORMILC) WAS PUT into the earth, or INTERRED.

It

It ought to be here remarked, that the word חניבעל terminates the second line, and begins the third; as also that the proper name חניבעל, HANNIBAL, by a similar kind of bisection, belongs both to the third and fourth lines. But this is by no means to be wondered at. The Greeks observed the same method of writing in their inscriptions¹⁰, both of an earlier and a later date.

III.

That the words above explained form a sepulchral inscription, will admit of no dispute. The three first of them in particular, which seem to be a sort of preface or introduction to the proper inscription, render this incontestable; and the others, either in conjunction with or exclusive of them, amount to an assertion of this in direct terms, and consequently prove it to demonstration. That the term בֵּית, the second word of the inscription, is equivalent to the Hebrew בית, notwithstanding the omission of *Jod*, is evident beyond contradiction, not only from the reason above assigned, but likewise because the expression בֵּית עַלְמָן denotes THE HOUSE OF LONG DURATION, A MAN'S LONG HOME, or THE GRAVE, the very sense it is used in here, ECCLES. XII. 5. Nor can the *Jod* well be looked upon as an essential part of the noun, since the plural of בֵּית in the Hebrew is בְּתִים, and the Ethiopic term for a house is בֵּית, agreeing in all respects with the second word here. M. l'Abbé Bar-

¹⁰ Chish. *Antiquitat. Asiatic.* pass. Vid. etiam Tho. Reines. *Synagm. Inscript. Antiqu.* pass. Lipsiae, 1682.

thelemy

thelemy¹¹ takes the third letter of the third line for *Vau*, and assigns it the place of that element in

" *Journal des Savans*, ubi sup. M. l'Abbé Barthélémy^a takes a very similar character for *Vau*, in the inscription of Carpentras; which probably induced him to assign this figure the power of that element, though the inscription of Carpentras does not appear to me to have been first discovered in the island of Malta. The letters forming the Maltese-Phœnician inscription, which the French Abbé has attempted to explain, are very different from those of the inscription I have been considering, and the two characters in particular imagined to represent *Vau* in these monuments bear scarce any resemblance to each other. Hence it should seem to follow, according to M. l'Abbé, who^b attributes the diversity of character in the Phœnician or Punic inscriptions rather to difference of place than distance of time, that the letter in question ought by no means to be looked upon as *Vau*. I shall not however pretend to avail myself of a notion, how hard soever it may bear upon him, that I consider at least as arbitrary and precarious, if not plainly false; but shall suspend any farther observations I may have to make on this head, 'till the publication of M. l'Abbé's famous memoir on the Phœnician letters, upon the superior merit of which he has himself with so much complacency^c been pleased to dilate, and which some of his^d admirers have placed in so glorious a light. In the mean time I must beg leave to remark, that the character before me does not only resemble one of the Chaldee forms of *Pè*, but likewise the^e ancient Samaritan and Greek forms of the same element; and that the word formed of *Resch* and *Pè*, ܪܰܲ, is consonant enough to the tenor of the inscription. This, I conceive, sufficiently authorizes me at present to ascribe to the supposed *Vau* the power of *Pè*. If in this point I should happen to be wrong, M. l'Abbé will most certainly^f rectify my mistake. I shall ever lye open to conviction, being determined in my researches and inquiries to sacrifice all inferior considerations to the love of truth.

^a M. de Guignes, *De l'Orig. des Chir.* p. 54. A Paris, 1760. *Recueil d'Antiquit. &c.* de Comte de Caylus, Tom. I. p. 73, 74. pl. XXVI. A Paris, 1752.

^b *Journal des Savants*, ubi sup.

^c *Journal des Savans*, Août 1760. p. 277.

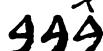
^d M. de Guign. ubi. sup. p. 60. *Journal des Savans*, Decembre 1760. p. 348.

^e Joan. Baptist. Biancon. *De Antiqu. Litter. Hebræor. et Græcor.* p. 31, 32. Bononiæ, 1748.

^f *Recueil des Médailles de Peuples et de Villes, &c.* Tom. III. p. 140. A Paris, 1763.

his alphabet. But the form of this character is totally different from that of the Phœnician *Vau*, especially as it is exhibited by the other Maltese inscription, of which he pretends to have given us so accurate a copy. Nor will the sense of that part of the inscription in which this letter appears afford the least countenance to such a supposition. On the contrary, the figure of this element well enough corresponds with that of the final square or Chaidee *Pe*, and the verb formed of *Resch* and *Pe* seems consonant enough to the tenor of the inscription. To what has been observed of the letter *Zain*, in the second line, we may add, that this character sufficiently resembles the Hebrew and Palmyrene *Zain*; and that the word נוֹמֵן, DORMIENTIS, very naturally concludes the sentence, of which it is a constituent part. All which if we admit, and allow Sig. Abate Venuti's copy to be in the main exact, as I cannot help thinking it is; the following alphabet, plainly deducible from that copy, will be found to contain seventeen of the Punic literary characters used in Malta, when our inscription first appeared.

The Maltese-Punic alphabet.

| | | | |
|------------------|---|------------------|---|
| Aleph - - - - - |  | Lamed - - - - - |  |
| Beth - - - - - |  | Mem - - - - - |  |
| Ghimel - - - - - |  | Nun - - - - - |  |
| Daleth - - - - - |  | Samech - - - - - | |
| He - - - - - |  | Ajin - - - - - |  |
| Vau - - - - - | | Pe - - - - - |  |
| Zain - - - - - |  | Tzade - - - - - | |
| Heth - - - - - |  | Koph - - - - - |  |
| Teth - - - - - | | Resch - - - - - |  |
| Jod - - - - - | | Schin - - - - - |  |
| Caph - - - - - |  | Thau - - - - - |  |

The Maltese-Punic inscription. [TAB. XVII.]

h u z f a z q u u p q t v
v y n i n h h q s n p s
s { o r u g u y x u a y
t h u f a f } s h u

Hence 'tis obvious at first sight, that the forms of some of these letters differ from those of the correspondent elements in M. l'Abbé Barthelemy's alphabet; and that the characters he takes for *Vau* and *Jod*, which has indeed been already remarked, I suppose to be *Pe* and *Zain*. Which of us is in the right, after a more accurate copy of the inscription can be procured, perhaps the learned may be able to decide.

IV.

Who Hannibal the son of Barmelet, Barmelc, or Bormilc, was, or when he lived, for want of sufficient light from ancient history, I cannot take upon me precisely to determine. We may however, I think, rest assured, that he died a considerable time (perhaps several centuries) after the Citiean inscriptions, or at least the earliest of them, first appeared. The forms of several of the letters, particularly of

the *Aleph*, *Gbimel*, *He*, *Heth*, *Capb*, *Ajin*, *Koph*, *Schin*, and *Thau*, so considerably differing from those of the same elements in the earlier Phœnician times, seem to render this uncontestedly clear. I know, indeed, that M. l'Abbé Barthelemy would be thought to insinuate, if he does not directly assert this, that such variations are "always to be attributed to difference of place,

¹² *Journ. des Scav. Suite de Decembre 1761.* p. 83. The character representing *Koph* in our inscription was not the original, nor even the earlier, form of that element. One somewhat resembling it was however used by the Carthaginians in Sicily, before they were dispossessed of that island by the Romans. This plainly enough appears from the Punic coin in Tab. XVII. now in my small cabinet, and never before published. This piece seems to exhibit the word קARTHADA, CARTHADA, the very Punic name of Carthage, according to ^a Solinus, in Punic characters; the first of which bears some resemblance to the later figure of *Koph*, as preserved on the monument under consideration. The letters on the reverse are not so legible. They nevertheless appear to me to form the names of two Carthaginian cities in Sicily. The first of these was perhaps MOTA, or rather MOTYA; the *Vau* and *Jod* having been not unfrequently omitted in Phœnician, and therefore probably in Punic, words. The name of the other town, as originally imprest on the medal, being in a great measure defaced; I shall not venture at a communication of it to the learned world, but leave it to be cleared up by some other coin or inscription that may possibly hereafter occur.

That the names of two Sicilian cities in Punic characters were sometimes imprest upon the reverse of one coin, may be fairly inferred from a Carthaginian medal now in my possession; of which I may probably, in a future paper, give a particular account. A form of *Koph* pretty similar to that visible on the monument I am considering likewise appears on a coin of ACHOLA, ACHOLLA, or ACHVLLA, as the medal presents the word to our view, struck in the Augustan age. From what has been here observed, as well as from the resemblance both these figures of *Koph* bear to the square or Chaldee form of the same element, we may collect the remains of antiquity that exhibit them not to have been the produce of the earlier Phœnician times.

* J. Solin. *Polybius*. cap. xxvii. Traject. ad Rhen. 1689.

rather

rather than distance of time. But besides that such a notion runs counter to what he had before ¹³ advanced, this by no means seems agreeable to truth, or the natural course of things. For the Punic and Phœnician alphabets were originally the very same, and continued so, or nearly so, I make not the least doubt, long after the foundation of Carthage. And this is rendered highly probable by the letters preserved on many Carthaginian coins. To what then can we so properly ascribe the aforesaid variations as to distance of time, since the letters so varied in the Carthaginian territories had undoubtedly the same forms with those of the correspondent elements in the more ancient Phœnician alphabet, (used both there and at Tyre, Sidon, Citium, &c.) several ages before? In fine, the same characters at first prevailed both at Carthage and in Phœnicia; though these, or at least several of them, in after ages, assumed pretty different forms. So that the more any Punic or Phœnician literary characters, in whatever country found, recede from those that formed the Samaritan or earliest Phœnician alphabet, the later they ought undoubtedly to be deemed, as I have elsewhere observed. Nor will M. l'Abbé, I flatter myself, notwithstanding the insinuation hinted at here, be displeased with me, if on this occasion I should *adopt* another ¹⁴ of his opinions.

After the Carthaginian provinces had been subdued by the Romans, the people still retained the use of their antient proper names, and spoke the Punic

¹³ *De l'Orig. des Chars.* par M. de Guignes, p. 39. A Paris, 1760.

¹⁴ M. de Guign. ubi sup. p. 39.

tongue.

tongue. The former of these points is abundantly clear from coins and inscriptions, published by the authors here referred to; and the latter of them is no less clear from writers, of the best and most undoubted authority. Nay, we have good reason to believe, that the Phœnician or Punic language was spoken and understood in some of those provinces even to the days of St. ¹⁶ Austin.

With regard to the island of Malta in particular, which was so long subject to the Carthaginians, it may not be improper to remark, that the entire reduction of it seems scarce to have been effected before the time of Julius Cæsar by the Romans. For although the people of that island were obliged to submit to the Roman power, after the destruction of Carthage; yet they found means afterwards to assert their independency, and shake off the Roman yoke. But notwithstanding they had been rendered a formidable maritime power, by the extensive commerce which they enjoyed, they were finally ¹⁷ subjugated by Cæsar, though with no small difficulty, about forty-five years before the birth of CHRIST. It may justly therefore be questioned whether the Latin tongue was ever much used in Malta before the death of that conqueror, or rather before the commencement of the Christian æra, which was but little posterior to it. Be that however as it will, that

¹⁵ Jo. Goth. Richter. *Nov. Num. in Colon. Karthag. African. Percus. &c.* p. 8. Lipsiæ, 1742. *Numism. Antiqu.* Thom. Pem-broch. et Montis Gemic. Com. P. 2. T. 89. Sam. Bochart. *Chan. Lib. II. c. xxiv.* Tho. Reines. ubi sup. p. 487, 488.

¹⁶ Christoph. Hendreich, in *Carthag.* p. 8, 9. Francofurti ad Oderam, 1664.

¹⁷ Appian. Alexandrin. apud Burchard. Nidersted. in *Malta Vet. et Nov.* lib. II. c. vi. p. 69. Helmestadii, 1660.

the

the use of the Punic language and the Punic proper names was retained in Malta, as an antient part of the Carthaginian territories, at least three or four centuries after the last mentioned period, if not much longer, from what has been here advanced, is abundantly clear. Nay, that the Punic tongue is even at this day the vernacular language of the lower part of the Maltese, though deformed by many corruptions, and disguised by the accession of various foreign words, after perusing what has been communicated on that head to the learned world by ¹⁸ Canonico Agius, I am strongly inclined to believe.

Since therefore the ducts of several of the letters indicate this inscription to be of a later date, we cannot but suppose it to have been many years (perhaps several centuries) posterior to the conclusion of the first Punic war. And since Hannibal Ben Barmelec, or Bormilc, is mentioned therein as a person of consideration, whose death was greatly lamented by the people; perhaps he was either a popular senator of Malta, or one of the suffetes there, (the Punic form of government not improbably prevailing in that island, even when dependent on the Romans, as it did in other ¹⁹ places that had been subject to the Carthaginian state) a century at least after Julius Cæsar had given the finishing stroke to the liberties of the Maltese. This, I say, appears to me by no means improbable; but that he really sustained either of the characters here mentioned, or lived at the time here supposed, I must not presume positively to affirm. The forms of some of the letters will not permit us however, I

¹⁸ Gio. Pietro Francesco Agius de Soldanis, *Della Lingua Punica presentamente usata da Maltesi, &c.* In Roma, 1750.

¹⁹ Hendreich, ubi sup. p. 316. Reines. ubi sup.

think,

think, to assign this inscription a higher age. They rather announce a later than an earlier date.

. V.

The words forming this inscription are for the most part either Hebrew or Punic. Of the former sort are הוה, רחם, קבר, עלם, בשת, בְּ; of the latter נכה, בְּכַלָּת, so that only גָּעֵל and רָן seem to bear any relation to the Chaldee and Syriac. Hence we may plainly see, as well as from what I have formerly observed, that neither the Punie nor Phœnician was almost entirely Syriac; and consequently, that the opposite notion, advanced by M. l'Abbé Barthélémy²⁰ and M. de Guignes, together with the superstructure they have erected upon it, must necessarily fall to the ground.

'Tis worthy observation here, that we have not met with the proper name of a Carthaginian in Punic characters, on any of the remains of antiquity, before the monument whose inscription I have been considering occurred; and it likewise ought to be remarked, that the word HANNIBAL is formed of the very same Punic letters in this inscription that it has been supposed to have antiently consisted of by the "learned."

With regard to the ellipses pointed out to us in the Latin and English versions of this inscription, they are such as have ever been common in the eastern world; and similar ones will present themselves to

²⁰ M. de Guign. ubi sup. p. 60. *Journal des Scav.* Decembre 1760. p. 348.

²¹ Boch. *Chon. Lib. II. c. xii.* Hendr. ubi sup. p. 149. Ad. Littlet. *Ling. Latin. Dic.*

our

our view in passages of scripture, too numerous, as well as too obvious, to be cited here ²².

The length of the inscription, as it seems to have only a single person for it's object, as well as the forms of it's letters, will undoubtedly evince it to be the produce of a later age ; though the precise time of it's first appearance, for want of sufficient light from antient history, I cannot take upon me to ascertain. Nor shall I be so vain as to pronounce the explication now submitted to the judgment of the Royal Society in all points true, as I have not yet met with a copy of the inscription absolutely to be depended upon. However, I hope it will not be found very remote from truth. If hereafter, by means of a more accurate transcript, I should discover any errors in what has been here advanced, I shall most readily retract them, and ever with great pleasure listen to better information. All farther remarks on this curious monument of antiquity, so highly meriting the attention of the learned, I must at present supersede ; having now only time to beg you would believe me to be, with the most perfect consideration and regard,

S I R,

Your much obliged,

and most obedient servant,

Christ-Church, Oxon.
May 20th, 1763.

John Swinton.

²² Vid. Johan. Buxtorf. *Thesaur. Grammat. Ling. Sanct. Hebr.*
& Christian. Nold. *Concordant. Particular. Ebraeo-Chaldaic.* pass.
Vid. etiam Boch. *Chan. Lib. I. c. xxxv. p. 705.* Francfurti
ad Moenum, 1681.

**XLVI. Problems by Edward Waring, M.A.
and Lucasian Professor of Mathematics in
the University of Cambridge, F.R.S.**

P R O.

Read April 21, } 1. Nvenire, quot radices impossibilis
1763. } habet data biquadratica æquatio
 $x^4 + qx^2 - rx + s = 0$.

1^{mo} Sit $256 s^3 - 128 q^2 s^2 + \overline{144 r^2 q + 16 q^4} x s - 27 r^4 - 4 r^2 q^3$ negativa quantitas, & duas &c non plures impossibilis radices habet data æquatio.

2^{do} Sit affirmativa quantitas, & vel $-q$ vel $q^2 - 4 s$ negativa quantitas, & datæ æquationis quatuor radices erunt impossibilis.

3^{ti}. Sit nihilo æqualis, & vel $-q$ vel $q^2 - 4 s$ negativa quantitas, & datæ æquationis duæ inæquales radicis erunt impossibilis.

2. Invenire, quot radices impossibilis habet data æquatio $x^5 + qx^3 - rx^2 + sx - t = 0$.

$$\begin{aligned} 1^{\text{mo}} \quad & \text{Si signa terminorum æquationis } w^{10} + 10 q w^9 \\ & + 39 q^2 + 10 s x w^8 + 80 q^3 + 50 q s + 25 r^2 x w^7 + \\ & 95 q^4 + 124 q^2 s - 95 s^2 + 92 q r^2 + 200 r t x w^6 + \\ & 66 q^5 - 360 q s^2 + 196 q^2 s + 118 q^3 r + 260 r^2 s + 625 \\ & t^2 + 400 q r t x w^5 + 25 q^6 + 40 s^3 - 53 r^4 + 52 q^3 r^2 - \\ & 522 q^2 s^2 + 194 q^4 s + 708 q r^2 s + 240 q^3 r t + 1750 \\ & q t^3 - 950 s r t x w^4 + 4 q^7 + 106 q^5 s - 80 q s^3 - 308 \\ & q^3 s^2 - 102 q r^4 - 7 q^2 r^2 + 570 r^2 s^2 + 612 q^3 r^2 s + 700 \\ & r^3 t - 3750 t^2 s + 2500 t^2 q^4 + 80 r t q^3 - 2150 q r s t \\ & x w^3 + 400 s^5 - 360 q^2 s^3 - 15 q^4 s^2 + 24 q^6 s - 8 q^8 r^2 \end{aligned}$$

$$\begin{aligned}
 & -45 q^2 r^3 - 270 r^4 s + 140 r^2 s q^2 + 960 r^2 s^2 q + 1875 \\
 & t^2 r^2 + 1000 t r s^3 - 5000 t^2 q s + 1750 t^2 q^3 + 40 t r q^4 \\
 & + 600 t r^3 q - 1650 t r s q^2 \times w^2 + 36 q^3 s^2 - 224 q^2 s^3 \\
 & + 320 q s^4 + 4 q^3 r^4 + 27 r^6 - 40 r^2 s^2 + 434 r^2 q^2 s^2 - \\
 & 24 r^2 s q^3 - 198 r^4 q s + 5000 t^2 s^2 - 450 t r^3 s - 6250 \\
 & t^3 r + 675 t^2 q^4 - 3750 t^2 q^2 s + 3000 t^2 r^2 q + 60 t r^3 q^2 \\
 & + 200 t r s^2 q - 330 t r q^3 s \times w + 3125 t^4 - 3750 q r t^3 \\
 & + 2000 s^2 q + 2250 r^2 s - 900 s q^3 + 825 r^2 q^2 + 108 q^5 \\
 & \times t^2 - 1600 s^3 r - 560 r q^2 s^2 - 16 r^3 q^3 + 630 r^3 q s + \\
 & 72 r s q^4 - 108 r^5 \times t + 256 s^3 - 128 q^2 s^4 + 144 r^2 q s^3 \\
 & + 16 q^4 s^3 - 27 r^4 s^2 - 4 r^2 q^3 s^2 = 0. \text{ continuo muten-} \\
 & \text{tur de } + \text{ in } - ; \& - \text{ in } + ; \text{ nullas impossibiles ra-} \\
 & \text{dices habet data æquatio.}
 \end{aligned}$$

2^{do}. Si signa terminorum æquationis haud continuo mutentur de + in — & — in + ; duæ vel quatuor datæ æquationis radices erunt impossibiles, prout ultimus ejus terminus sit negativa vel affirmativa quantitas.

3^{to}. Si ultimus ejus terminus nihilo sit æqualis, & signa terminorum æquationis haud continuo mutentur de + in — & — in + ; tum vel quatuor vel duæ radices datæ æquationis erunt impossibiles, prout duo & non plures ultimi datæ æquationis termini nihilo sint æquales, necne.

P R O.

Sint x, y, v , abscissa, ordinata & area datæ curvæ, & sit $y^n + a + bx \times y^{n-1} + c + ax + ex^2 \times y^{n-2} + f + g x$
 $+ bx^2 + kx^3 \times y^{n-3} + \&c. = 0$. invenire, utrum area (v) quadrari potest, necne.

Supponamus æquationem ad aream esse $v^n + A + Bx + Cx^2 v^{n-1} + D + Ex + Fx^2 + Gx^3 + Hx^4 \times$

$$\begin{aligned}
 & v^{n-2} + I + Kx + Lx^2 + Mx^3 + Nx^4 + Ox^5 + Px^6 \\
 & x v^{n-3} + \text{&c.} = 0. \quad \& \text{consequenter erit } nyv^{n-1} + n - 1 \\
 & \overline{A + Bx + Cx^2 y v^{n-2}} + n - 2 \times \overline{DxEx + Fx^2 + Gx^3 + Hx^4} \\
 & \quad \overline{B + 2Cx} \quad v^{n-1} + \overline{E + 2Fx + 3Gx^2 + 4Hx^3} \\
 & x y v^{n-3} + \text{&c.} \quad \left. \right\} = 0. \\
 & x v^{n-2} + \text{&c.} \quad \left. \right\} = 0.
 \end{aligned}$$

Ex quibus æquationibus, si methodis notis exterminetur (v), habebimus æquationem, quæ exprimit relationem inter (x) & (y). Hujus autem æquationis coefficientes æquari debent coefficientibus datæ æquationis $y^n + \overline{a+b}x y^{n-1} + \overline{c+d}x + \overline{e}x^2 + \overline{y} + \text{&c.} = 0$; & si quantitates $A, B, C, \&c.$ exinde determinari possunt, curva quadratur, est enim $v^n + \overline{A+Bx}$
 $+ \overline{C}x^2 \times v^{n-1} + \overline{D+E}x + \overline{F}x^2 + \overline{G}x^3 + \overline{H}x^4$
 $\times v^{n-2} + \text{&c.} = 0$; aliter autem quadrari non potest.

Ex. Sit data æquatio $y^2 + x^2 - 1 = 0$, & supponamus æquationem ad aream $v^2 + D + Ex + Fx^2 + Gx^3 + Hx^4 = 0$; & erit $2vy + E + 2Fx + 3Gx^2 + 4Hx^3 = 0$, ita reducantur hæ duæ æquationes in unam, ut exterminatur (v), & resultat æquatio $y^2 + 16H^2x^6 + 24HGx^5 + 16HF + 9G^2x^4 + 8EH + 12FG$
 $- 4 \times Hx^4 + Gx^3 + Fx^2 + Ex + D$
 $x^3 + 6GE + 4F^2x^2 + 4FEx + E^2 = 0$; debet autem
fractio $\frac{16H^2x^6 + 24HGx^5 + 16HF + 9G^2x^4 + 8EH + 12FG}{4 \times Hx^4 + Gx^3 + Fx^2 +$
 $x^3 + 6GE + 4F^2x^2 + 4FEx + E^2}$ esse $x^2 - 1$; & consequenter

$$\begin{aligned}
 4H &= 16H^2 \\
 4G &= 24HG \\
 4F - 4H &= 16HF + 9G^2 \\
 4E - 4G &= 8HE + 12FG \\
 4D - 4F &= 6GE + 4F^2 \\
 - 4E &= 4FE \\
 - 4D &= E^2
 \end{aligned}$$

sed e methodo communes divisores inveniendi constat has æquationes inter se contradictorias esse, & consequenter curvam haud generaliter esse quadrabilem.

THEO.

Sint x, y, v , abscissa & ordinatæ curvarum ABCD EFGHI &c. & $A\beta\gamma\delta\epsilon$ &c. & sit $y = px^n$, & $v =$

$$\begin{aligned}
 &\frac{n}{2 \cdot 3} pa^{n-1} x - \frac{n \times n-1 \times n-2}{30 \times 2 \times 3} pa^{n-3} x^3 + \frac{n \times n-1 \times n-2}{42 \times 2 \times 3} \\
 &\times n-3 \times n-4 pa^{n-5} x^5 - \frac{n \times n-1 \times n-2 \times n-3 \times n-4 \times n-5}{30 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7} \\
 &\times n-6 pa^{n-7} x^7 + \frac{5n \times n-1 \times n-2 \times n-3 \times n-4 \times n-5}{66 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8} \\
 &\times n-6 \times n-7 \times n-8 pa^{n-9} x^9 - \frac{691 \times n \times n-1 \times n-2 \times n-3}{2730 \times 2 \times 3 \times 4 \times 5 \times 6} \\
 &\times n-4 \times n-5 \times n-6 \times n-7 \times n-8 \times n-9 \times n-10 pa^{n-11} \\
 &\times 7 \times 8 \times 9 \times 10 \times 11
 \end{aligned}$$

$x^n +$ &c. cuius ultimus terminus debet esse x^{n-1} vel x^{n-2} , prout (n) est par vel impar numerus.

Sit $x = AP = a$, bisecetur AP in T in duas æquales partes, & ducatur linea ET δ, & si AE, EM, AM, jungantur; erit triangulum AEM = TP δ T areæ.

Deinde,

Deinde, bisecentur TP , AT in R and V , & ducantur RG , $CV\gamma$; & jungantur AC , CE , EG , GM ; & erunt duo triangula $ACE + EGM = VT\delta\gamma V$ areae.

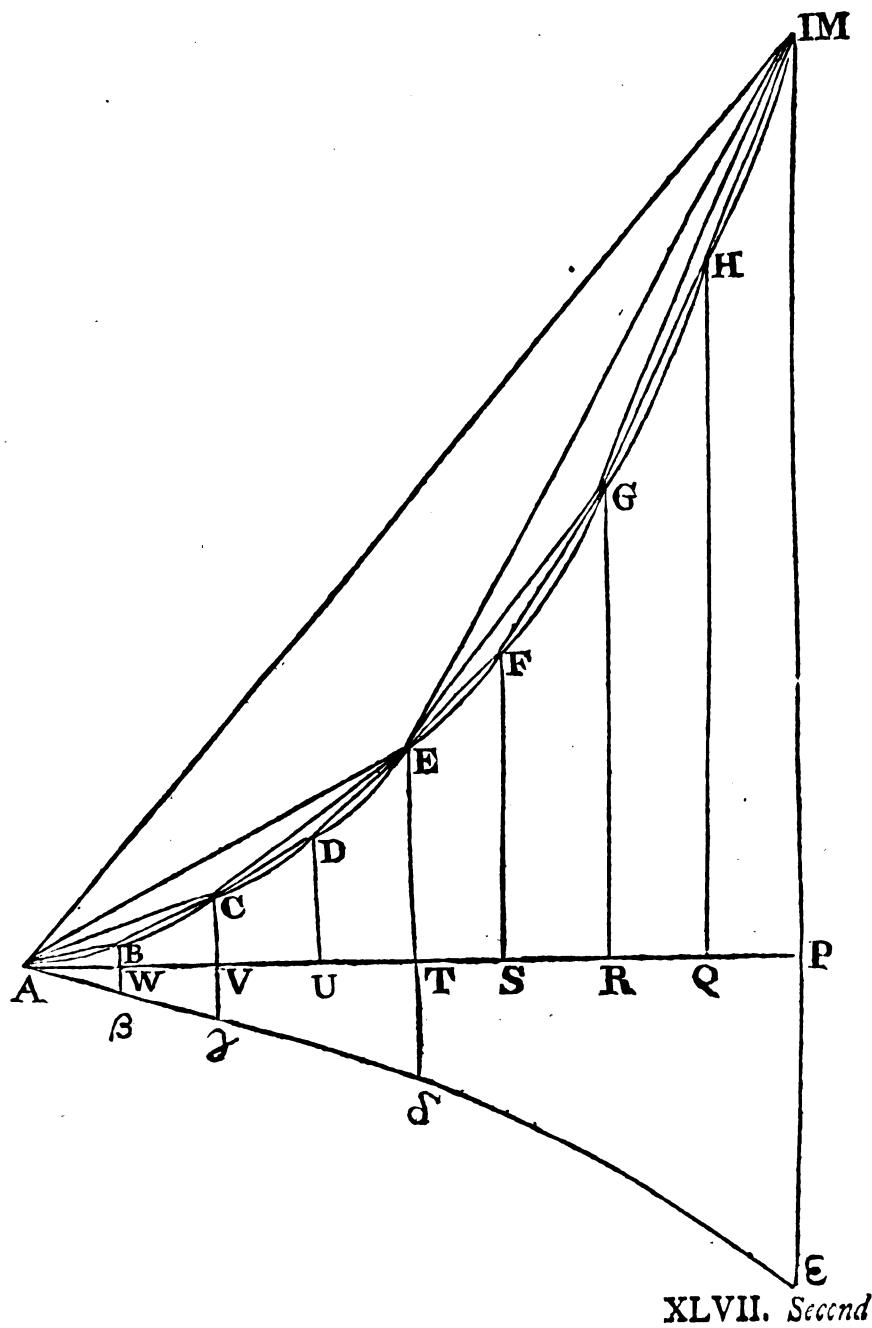
Eodem modo, si partes AV , VT , TR , RP iterum bisecentur in W , U , S , Q , & ducantur lineæ $BW\beta$, UD , SF , QH ; & jungantur AB , BC , CD , DE , EF , FG , GH , HM ; erunt quatuor triangula $ABC + CDE + EFG + GHM = WV\gamma\beta W$ areae; & sic deinceps.

Cor. 1. Si curva ABC & M sit conica parabola, $(c, e) y = p i x^2$, erit $v = \frac{1}{3} p a x$; & $A\beta\gamma\delta$ &c. erit recta linea; & propositio eadem est cum notissimâ propositione Archimedis de quadraturâ parabolæ.

Cor. 2. Si $y = p x^3$, erit $v = \frac{1}{4} p a^2 x$, & $A\beta\gamma\delta$ &c. iterum recta linea.

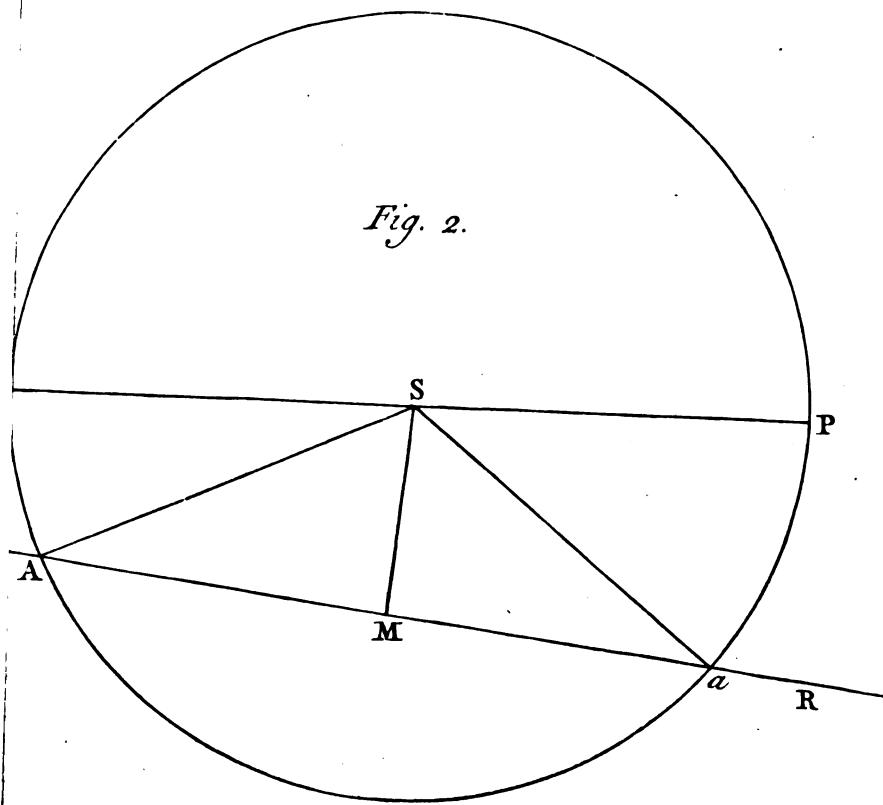
Cor. 3. Datâ curvâ, cuius æquatio est $y = p x^n$, inveniri potest altera curva, cuius dimensiones sunt $(2n-1)$, in quâ summæ triangulorum ad singulas bisectiones erunt respectivè æquales summis triangulorum datæ curvæ.

His adjici potest, quod si loco bisectionis abscissa AP aliâ quâvis ratione in æquales partes dividatur, summæ triangulorum curvæ $ABCD$ &c. ad singulas divisiones æquales erunt segmentis curvæ $A\beta\gamma\delta$ &c.



XLVII. Second Paper concerning the Parallax of the Sun determined from the Observations of the late Transit of Venus, in which this Subject is treated of more at length, and the Quantity of the Parallax more fully ascertained. By James Short, M.A. and F. R. S.

Read Dec. 8, 1763. IN the last volume of the Memoirs of the Royal Academy at Paris for the year 1761, there is a Memoir by M. Pingré, who went to the island of Rodrigues, and observed the transit of Venus there; in this memoir M. Pingré endeavours to determine the parallax of the Sun, by the observation of the late transit of Venus, to be $= 10''$. both by the observed durations, the least distance of the centers, and by the internal contact at the egress; and seems to think that there must be some mistake in the observation of Mr. Mason at the Cape of Good Hope, particularly with regard to the difference of longitude between Mr. Mason's observatory and Paris, because by comparing the observation of Mr. Mason at the Cape with the European observations, he finds the parallax of the Sun, from thence resulting, to be between $8''$ and $9'$, consequently differing from the determination by the observation at Rodrigues when compared with the same places. I shall therefore, in this paper, endeavour to prove, beyond all doubt, by a comparison of the observations





observations on this side of the Equinoctial Line alone, that the Sun's parallax is between 8" and 9", and that this determination is the same or very nearly the same, as when the observation at the Cape is compared with the same places. I shall also endeavour to prove, that there is a mistake of one minute in time in writing down the time of the internal contact at the egress at Rodrigues, and that this being corrected, the results of the Sun's parallax, by a comparison of the observation at Rodrigues with the observations at the several places on this side of the *Line*, is the same with that which results from all the rest; and this agreement is also an argument that there must have been such a mistake in setting down the time of the internal contact at the egress at Rodrigues. I shall also shew that the parallax of the Sun, determined from the observed durations, and from the least distance of the centers is very nearly the same as that which is determined from the internal contact at the egress, though these last determinations cannot be so much depended on because of the minute elements from which they are drawn.

I shall therefore proceed to compare the observations of the internal contact made on this side of the *Line* only, and from thence determine the Sun's parallax. In order to do this it is necessary that the differences of longitude between the places of observation, compared together, be well ascertained: and in the doing of this, in all places where the ingress was observed, I have been much obliged to a very ingenious method, proposed by M. Pingré in his aforesaid Memoir, to which I refer; and for the longitudes of other places I have consulted the Philosophical Transactions

actions and the *Connoissance des Temps.* These differences of longitude are as follow.

| | | | | | | |
|-------------------------------|---|---|----|----|-------|-------|
| Tobolsk and Abo - - - - - | = | 3 | 4 | 37 | " | West. |
| Bologna - - - - - | = | 3 | 47 | 46 | W. | |
| Calcutta - - - - - | = | 1 | 20 | 38 | East. | |
| Cajaneburg - - - - - | = | 2 | 42 | 12 | W. | |
| Calmar - - - - - | = | 3 | 27 | 32 | W. | |
| Cape of G. Hope - - - - - | = | 3 | 19 | 32 | W. | |
| Florence - - - - - | = | 3 | 49 | 3 | W. | |
| Gottingen - - - - - | = | 3 | 53 | 35 | W. | |
| Grand Mount - - - - - | = | 0 | 46 | 26 | E. | |
| Greenwich - - - - - | = | 4 | 33 | 7 | W. | |
| Hernofand - - - - - | = | 3 | 21 | 55 | W. | |
| Lefkeard - - - - - | = | 4 | 51 | 39 | W. | |
| Madras - - - - - | = | 0 | 47 | 11 | E. | |
| Paris - - - - - | = | 4 | 23 | 51 | W. | |
| Rodrigues - - - - - | = | 0 | 20 | 25 | W. | |
| Rome - - - - - | = | 3 | 43 | 14 | W. | |
| Savile-house, Lond. - - - - - | = | 4 | 33 | 37 | W. | |
| Stokolm - - - - - | = | 3 | 20 | 41 | W. | |
| Tornea - - - - - | = | 2 | 56 | 7 | W. | |
| Tranquebar - - - - - | = | 0 | 46 | 9 | E. | |
| Upsal - - - - - | = | 3 | 22 | 40 | W. | |

I have deviated from the above mentioned method of M. Pingré in settling the longitude of Stokolm, by

* The latitude of Savile-house, London, is = $51^{\circ} 30' 50''$ N. The latitude of Florence is = $43^{\circ} 46' 30''$ N. and that of Gottingen = $51^{\circ} 31' 54''$ N. The latitudes of the rest of the places are set down in my former paper on this subject, only that of Tranquebar should be = $11^{\circ} 30' 0''$.

the

the ingress, because it appears clear to me that there must have been a mistake in the observation of the internal contact at the ingress at Stokholm, owing, as I mentioned in my former paper, to the small altitude of the Sun at the time of the ingress: for by comparing the times of ingress and egress observed at Stokholm and Upsal, we find that the difference of longitude between these two places is $1' 39''$, and $1' 59''$, and as we are sure that the observation at the egress, gives the difference of longitude the most certain in this case, therefore it follows that the error was at the ingress, and it is easy to prove that the error is in the observation at Stokholm.

To avoid all uncertainty, and to be as clear and distinct as possible, I shall set down, in the following table, the observation at the egress at each place compared, the difference of longitude between each place compared, the effect of the parallaxes resulting from the comparison, and also the effect of the parallaxes computed on a supposition that the Sun's parallax is $= 8''.5$, in order, that if there is any mistake, it may, the more easily, be discovered.

I compare Cajarteburg with 18 places, Bologna with 17 places, and Tobolsk with 18 places, and they are as follow.

| 10 7 59 | Cajan. | 2 59 C. | 10 7 59 | Cajan. | 2 59 C. |
|---------|--------------|---------|---------|--------------|---------|
| 0 38 29 | = D. M. | 2 18 S. | 0 40 28 | = D. M. | 2 21 U. |
| <hr/> | | | <hr/> | | |
| 9 29 30 | | 41 | 9 27 31 | | 38 |
| 9 30 11 | Stokholm. | | 9 28 9 | Upsal. | |
| <hr/> | | | <hr/> | | |
| 41 | | | 38 | | |
| | Par. = 8. 50 | R r 2 | | Par. = 8. 50 | 10 7 |

[304]

| | | | | | | |
|--|--|---|-----------|---------------------------------------|---|----|
| $\begin{array}{r} 10 \\ 0 \end{array}$ | $\begin{array}{r} 7 \\ 22 \end{array}$ | $\begin{array}{r} 59 \\ 25 \end{array}$ | Cajan. | $\begin{array}{r} 2 \\ 2 \end{array}$ | $\begin{array}{r} 59 \\ 30 \end{array}$ | C. |
| $\underline{\underline{}}$ | $\underline{\underline{}}$ | $\underline{\underline{}}$ | $= D. M.$ | $\begin{array}{r} 2 \\ 1 \end{array}$ | $\begin{array}{r} 59 \\ 59 \end{array}$ | A. |
| 9 | 45 | 34 | | 29 | | |
| 9 | 45 | 59 | Abo. | | | |
| | 25 | | | | | |
| | | | Par. = | 7.33 | | |

| | | | | | | |
|--|--|---|------------|---------------------------------------|---|---------------|
| $\begin{array}{r} 10 \\ 0 \end{array}$ | $\begin{array}{r} 7 \\ 39 \end{array}$ | $\begin{array}{r} 59 \\ 43 \end{array}$ | Cajan. | $\begin{array}{r} 2 \\ 2 \end{array}$ | $\begin{array}{r} 59 \\ 26 \end{array}$ | C. |
| $\underline{\underline{}}$ | $\underline{\underline{}}$ | $\underline{\underline{}}$ | $= D. M.$ | $\begin{array}{r} 2 \\ 1 \end{array}$ | $\begin{array}{r} 59 \\ 44 \end{array}$ | T. |
| 9 | 28 | 16 | | 33 | | |
| 9 | 28 | 52 | Hernosand. | | | |
| 0 | 0 | 36 | | | | |
| | | | Par. = | 9.27 | | |
| | | | | | | Par. = 8.64 |

| | | | | | | |
|--|--|---|------------|---------------------------------------|---|---------------|
| $\begin{array}{r} 10 \\ 1 \end{array}$ | $\begin{array}{r} 7 \\ 11 \end{array}$ | $\begin{array}{r} 59 \\ 23 \end{array}$ | Cajan. | $\begin{array}{r} 2 \\ 1 \end{array}$ | $\begin{array}{r} 59 \\ 18 \end{array}$ | C. |
| $\underline{\underline{}}$ | $\underline{\underline{}}$ | $\underline{\underline{}}$ | $= D. M.$ | $\begin{array}{r} 1 \\ 1 \end{array}$ | $\begin{array}{r} 59 \\ 12 \end{array}$ | G. |
| 8 | 56 | 36 | | 141 | | |
| 8 | 58 | 26 | Gottingen. | | | |
| 0 | 1 | 50 | | | | |
| | | | Par. = | 9.25 | | |
| | | | | | | Par. = 9.09 |

| | | | | | | |
|--|--|---|---------------|---------------------------------------|---|---------------|
| $\begin{array}{r} 10 \\ 1 \end{array}$ | $\begin{array}{r} 7 \\ 51 \end{array}$ | $\begin{array}{r} 59 \\ 25 \end{array}$ | Cajan. | $\begin{array}{r} 2 \\ 1 \end{array}$ | $\begin{array}{r} 59 \\ 11 \end{array}$ | C. |
| $\underline{\underline{}}$ | $\underline{\underline{}}$ | $\underline{\underline{}}$ | $= D. M.$ | $\begin{array}{r} 1 \\ 0 \end{array}$ | $\begin{array}{r} 59 \\ 21 \end{array}$ | S.H. |
| 8 | 16 | 34 | | 148 | | |
| 8 | 18 | 22 | Savile-house. | | | |
| 0 | 1 | 48 | | | | |
| | | | Par. = | 8.50 | | |
| | | | | | | Par. = 8.06 |

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[3^o5]

| | | |
|--------------|-------------------------|------------------------|
| ^b | 10 7 59 Cajan. 2 59 C. | 10 7 59 Cajan. 2 59 C. |
| | 1 41 39 = D. M. 0 53 P. | 1 5 34 = D. M. 0 29 B. |
| | <hr/> | <hr/> |
| | 9 26 20 2 6 | 9 2 25 2 30 |
| | 9 28 25 Paris. | 9 4 54 Bologna. |
| | <hr/> | <hr/> |
| | 0 2 5 | 0 2 29 |
| | Par. = 8. 43 | Par. = 8. 44 |

| | | |
|--------------|------------------------|------------------------|
| ^b | 10 7 59 Cajan. 2 59 C. | 10 7 59 Cajan. 2 59 C. |
| | 1 1 2 = D. M. 0 13 R. | 1 6 51 = D. M. 0 24 F. |
| | <hr/> | <hr/> |
| | 9 6 57 2 46 | 9 1 8 2 35 |
| | 9 9 36 Rome. | 9 3 28 Florence. |
| | <hr/> | <hr/> |
| | 0 2 39 | 0 2 20 |
| | Par. = 8. 14 | Par. = 7. 68 |

| | | |
|--------------|------------------------|--------------------------|
| ^b | 10 7 59 Cajan. 2 59 C. | 10 7 59 Cajan. 2 59 C. |
| | 4 2 50 = D. M. 2 22 C. | 3 28 38 = D. M. 1 0 G.M. |
| | <hr/> | <hr/> |
| | 14 10 49 0 37 | 13 36 37 1 59 |
| | 14 11 34 Calcutta. | 13 38 30 G. Mount. |
| | <hr/> | <hr/> |
| | 0 0 45 | 0 1 53 |
| | Par. = 10. 34 | Par. = 8. 07 |

| | | |
|--------------|-------------------------|------------------------|
| ^b | 10 7 59 Cajan. 2 59 C. | 10 7 59 Cajan. 2 59 C. |
| | 3 28 21 = D. M. 0 52 T. | 3 29 23 = D. M. 1 0 M. |
| | <hr/> | <hr/> |
| | 13 36 20 2 7 | 13 37 22 1 59 |
| | 13 38 25 Tranquebar. | 13 39 38 Madras. |
| | <hr/> | <hr/> |
| | 0 2 5 | 0 2 16 |
| | Par. = 8. 36 | Par. = 9. 78 |

10 7

[306]

| | | | |
|--------------|--|--------------|---|
| ^h | 9 4 54 Bologna 0 29 B. 0 27 5 = D. M. 2 18 S. | ^h | 9 4 54 Bologna 0 29 B. 0 25 5 = D. M. 2 21 Up. |
| | 9 31 59 I 49 9 30 8 Stokholm. | | 9 29 59 I 52 9 28 9 Upfsl. |
| | 0 1 51 Par. = " 8. 65 | | 0 1 50 Par. = " 8. 35 |

| | | | |
|--------------|--|--------------|---|
| ^h | 9 4 54 Bologna 0 29 B. 0 43 9 = D. M. 2 30 A. | ^h | 9 4 54 Bologna 0 29 B. 0 20 14 = D. M. 1 59 C. |
| | 9 48 3 2 I 9 45 59 Abo. | | 9 25 8 I 30 9 23 40 Calmar. |
| | 0 2 4 Par. = " 8. 71 | | 0 1 28 Par. = " 8. 31 |

| | | | |
|--------------|---|--------------|---|
| ^h | 9 4 56 Bologna 0 29 B. 0 25 51 = D. M. 2 26 H. | ^h | 9 4 54 Bologna 0 29 B. 3 47 46 = D. M. 3 44 T. |
| | 2 30 47 I 57 9 28 52 Hernosand. | | 12 52 40 3 15 12 49 23 Tobolsk. |
| | 0 1 55 Par. = " 8. 36 | | 0 3 17 Par. = " 8. 58 |

| | | | |
|--------------|---|--------------|--|
| ^h | 9 5 0 Bologna 0 29 B. 0 5 49 = D. M. 1 18 G. | ^h | 9 5 0 Bologna 0 29 B. 0 45 21 = D. M. 1 12 G. |
| | 8 59 11 0 49 8 58 26 Gottingen. | | 8 19 39 0 43 8 19 0 Greenwich. |
| | 0 0 45 Par. = " 8. 80 | | 0 0 39 Par. = " 7. 71 |

9 4

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^b
9 4 54 " Bologna 0 29 B.
0 45 51 = D. M. 1 11 S.H.

8 19 3 0 42
8 18 22 Savile-Houfe.

0 0 41

Par. = 8. 30

^b
9 4 54 " Bologna 0 29 B.
1 3 53 = D. M. 1 4 L.

8 1 1 0 35
8 0 21 Leafkeard.

0 0 40

Par. = 9. 71

^b
9 4 54 " Bologna 0 29 B.
0 36 5 = D. M. 0 53 P.

8 28 49 0 24
8 28 25 Paris.

0 0 24

Par. = 8. 50

^b
9 5 0 Bologna 0 29 B.
5 8 24 = D. M. 2 22 C.

14 13 24 1 53
14 11 34 Calcutta.

0 1 50

Par. = 8. 28

^b
9 4 54 Bologna 0 29 B.
4 34 12 = D. M. 1 0 G.M.

13 39 6 0 31
13 38 30 G. Mount.

0 0 36

Par. = 9. 87

^b
9 4 54 Bologna 0 29 B.
4 33 55 = D. M. 0 52 T.

13 38 49 0 23
13 38 25 Tranquebar.

0 0 24

Par. = 8. 86

^b
9 5 0 Bologna 0 29 B.
4 34 59 = D. M. 1 0 M.

13 39 59 0 31
13 39 38 Madras.

0 0 21

Par. = 5. 76

^b
9 5 0 Bologna 0 29 B.
0 51 39 = D. M. 3 5 T.

9 56 39 2 36
9 54 8 Tornea.

0 2 31

Par. = 8. 23

12 49

8

[308]

| | |
|---|---|
| ^b 12 49 23 Tobolsk 3 44 T. 3 20 41 = D. M. 2 18 S. | ^b 12 49 23 Tobolsk 3 44 T. 3 22 40 = D. M. 2 21 U. |
| 9 28 42 9 30 8 Stokholm. | 9 26 43 9 28 9 Upfal. |
| 0 1 26 | 0 1 26 |

Par. = 8. 50

Par. = 8. 80

| | |
|--|---|
| ^b 12 49 23 Tobolsk 3 44 T. 3 4 37 = D. M. 2 30 A. | ^b 12 49 23 Tobolsk 3 44 T. 3 27 32 = D. M. 1 59 C. |
| 9 44 46 9 45 59 Abo. | 9 21 51 9 23 40 Calmar. |
| 0 1 13 | 0 1 49 |

Par. = 8. 40

Par. = 8. 82

| | |
|---|---|
| ^b 12 49 23 Tobolsk 3 44 T. 3 21 55 = D. M. 2 26 H. | ^b 12 49 23 Tobolsk 3 44 T. 3 53 35 = D. M. 1 18 G. |
| 9 27 28 9 28 52 Herno-sand. | 8 55 48 8 58 26 Gottingen. |
| 0 1 24 | 0 2 38 |

Par. = 9. 02

Par. = 10. 57

| | |
|--|---|
| ^b 12 49 23 Tobolsk 3 44 T. 4 33 7 = D. M. 1 12 G. | ^b 12 49 23 Tobolsk 3 44 T. 4 33 37 = D. M. 1 11 S.H. |
| 8 16 16 8 19 0 Greenwich. | 8 15 46 8 18 22 Savile-House. |
| 0 2 44 | 0 2 36 |

Par. = 9. 11

Par. = 8. 66

12 49

| | |
|---|---|
| $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{4}{4} \frac{51}{5} \frac{39}{3} = D.M. \frac{1}{0} \frac{4}{5} L.$ | $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{4}{4} \frac{23}{2} \frac{51}{5} = D.M. \frac{0}{0} \frac{53}{5} P.$ |
| $\frac{7}{8} \frac{57}{0} \frac{44}{21}$ Leskeard. | $\frac{8}{8} \frac{25}{28} \frac{32}{25}$ Paris. |
| $\frac{0}{0} \frac{2}{2} \frac{37}{37}$ | $\frac{0}{0} \frac{2}{2} \frac{53}{53}$ |
| Par. = $\frac{8}{8} . 34$ | Par. = $\frac{8}{8} . 60$ |
| $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{3}{3} \frac{49}{4} \frac{3}{3} = D.M. \frac{0}{0} \frac{24}{24} F.$ | $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{3}{3} \frac{43}{4} \frac{14}{14} = D.M. \frac{0}{0} \frac{13}{13} R.$ |
| $\frac{9}{9} \frac{0}{3} \frac{20}{28}$ Florence. | $\frac{9}{9} \frac{6}{9} \frac{9}{36}$ Rome. |
| $\frac{0}{0} \frac{3}{3} \frac{8}{8}$ | $\frac{0}{0} \frac{3}{3} \frac{27}{27}$ |
| Par. = $\frac{7}{7} . 99$ | Par. = $\frac{8}{8} . 34$ |
| $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{1}{1} \frac{20}{20} \frac{38}{38} = D.M. \frac{2}{1} \frac{22}{22} C.$ | $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{0}{0} \frac{46}{46} \frac{26}{26} = D.M. \frac{1}{0} \frac{0}{0} G.M.$ |
| $\frac{1}{14} \frac{10}{11} \frac{1}{34}$ Calcutta. | $\frac{1}{13} \frac{35}{38} \frac{49}{30}$ G. Mount. |
| $\frac{0}{0} \frac{1}{1} \frac{33}{33}$ | $\frac{0}{0} \frac{2}{2} \frac{41}{41}$ |
| Par. = $\frac{8}{8} . 64$ | Par. = $\frac{8}{8} . 34$ |
| $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{0}{0} \frac{46}{46} \frac{9}{9} = D.M. \frac{0}{0} \frac{52}{52} Tr.$ | $\frac{1}{12} \frac{49}{4} \frac{23}{2}$ Tobolsk $\frac{1}{3} \frac{44}{4}$ T. $\frac{0}{0} \frac{47}{47} \frac{11}{11} = D.M. \frac{1}{0} \frac{0}{0} M.$ |
| $\frac{1}{13} \frac{35}{38} \frac{32}{25}$ Tranquebar | $\frac{1}{13} \frac{36}{39} \frac{34}{38}$ Madras. |
| $\frac{0}{0} \frac{2}{2} \frac{53}{53}$ | $\frac{0}{0} \frac{3}{3} \frac{4}{4}$ |
| Par. = $\frac{8}{8} . 55$ | Par. = $\frac{9}{9} . 54$ |

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S s

I have

I have explained the manner of this table in a former paper on this subject to which I refer. I shall now set down the result from each comparison in the following order, that they may be the more easily seen.

| Sun's Parallax. | Sun's Parallax. | Sun's Parallax. |
|-----------------------------|-----------------------------|------------------------------|
| Cajan, and Stokholm = 8. 50 | Bolog. and Stokholm = 8. 65 | Tobolsk and Stokholm = 8. 50 |
| Upsal - - = 8. 50 | Upsal - - = 8. 35 | Upsal - - = 8. 80 |
| Abo - - = 7. 33 | Abo - - = 8. 71 | Abo - - = 8. 40 |
| Calmar - = 8. 64 | Calmar - = 8. 31 | Calmar - = 8. 82 |
| Hernofand - = 9. 27 | Hernofand - = 8. 36 | Hernofand - = 9. 02 |
| Tobolsk - = 9. 06 | Tobolsk - = 8. 38 | Göttingen = 10. 57 |
| Göttingen - = 9. 25 | Göttingen = 7. 80 | Greenwich = 9. 11 |
| Greenwich - = 9. 09 | Greenwich = 7. 71 | Savile-House = 8. 66 |
| Savile-House = 8. 50 | Savile-House = 8. 30 | Lefkard - = 8. 34 |
| Lefkard - = 8. 06 | Lefkard - = 9. 71 | Paris - - = 8. 68 |
| Paris - - = 8. 43 | Paris - - = 8. 50 | Florence - = 7. 99 |
| Bologna - = 8. 44 | Calcutta - = 8. 28 | Bologna - = 8. 58 |
| Rome - = 8. 14 | G. Mount = 9. 87 | Rome - = 8. 34 |
| Florence - = 7. 68 | Tranquebar = 8. 86 | Calcutta - = 9. 64 |
| Calcutta - = 10. 34 | Madras - = 5. 76 | G. Mount - = 8. 34 |
| G. Mount = 8. 07 | Cajaneburg = 8. 44 | Tranquebar = 8. 55 |
| Tranquebar = 8. 36 | Tornea - = 8. 23 | Madras - = 9. 54 |
| Madras - = 9. 71 | | Cajaneburg = 9. 07 |

The mean of these 53 comparisons gives the Sun's Parallax = 8'', 61.

Rejecting all those results which differ more than one second from the mean of the whole, the mean of the remaining 45 results gives the Sun's parallax = 8'', 55.

Rejecting all those results which differ more than half a second from the mean of the whole, the mean of the remaining 37 results gives the Sun's parallax = 8'', 57.

The mean of these three means gives the Sun's parallax = 8'', 58.

I shall next compare the observations of the internal contact at the egress made at Paris, Greenwich, Savile-House, Bologna, Madras, Grand Mount, and Tranquebar, with those made at Stokholm, Upsal, Tornea,

[311]

Tornea, Cajaneburg, Tobolsk, Abo, Calmar, Hernosand and Calcutta. They are as in the following table.

| | |
|--|--|
| ^b 9 28 52 Hernosand 2 26 H. 4 8 4 = D. M. o 51 T. | ^b 14 11 34 Calcutta 2 22 C. o 34 29 = D. M. o 51 T. |
| ^b 13 36 56 I 35 13 38 25 Tranquebar. | ^b 13 37 5 I 31 13 38 25 Tranquebar. |
| ^b o 1 29 Par. = " 7. 96 | ^b o 1 20 Par. = " 7. 47 |
| ^b 8 28 27 Paris o 53 P. I 3 10 = D. M. 2 18 S. | ^b 8 28 27 Paris o 53 P. I 1 11 = D. M. 2 21 U. |
| ^b 9 31 37 I 25 9 30 10 Stokholm. | ^b 9 29 38 I 28 9 28 9 Upsal. |
| ^b o 1 27 Par. = " 8. 70 | ^b o 1 29 Par. = " 8. 60 |
| ^b 8 28 27 Paris o 53 P. I 27 44 = D. M. 3 5 T. | ^b 8 28 27 Paris o 53 P. I 19 14 = D. M. 2 29 A. |
| ^b 9 56 11 2 12 9 54 8 Tornea. | ^b 9 47 41 I 36 9 45 59 Abo. |
| ^b o 2 3 Par. = " 7. 92 | ^b o 1 42 Par. = " 9. 03 |
| ^b 8 28 27 Paris o 53 P. o 56 19 = D. M. 1 58 C. | ^b 8 28 27 Paris o 53 P. I 1 56 = D. M. 2 26 H. |
| ^b 9 24 46 I 5 9 23 40 Calmar. | ^b 9 30 23 I 33 9 28 52 Hernosand. |
| ^b o 1 6 Par. = " 8. 63 S : 2 | ^b o 1 31 Par. = " 8. 42 S : 2 |

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| | | | |
|--------------|--|--------------|--|
| ^b | 8 28 27 Paris 6 53 P. 5 44 29 = D. M. 2 22 C. | ^b | 9 30 11 Stokholm 2 18 S. 1 12 26 = D. M. 1 12 G. |
| | 14 12 56 1 29 14 11 34 Calcutta. | | 8 17 45 1 6 8 19 0 Greenwich. |
| 0 1 22 | Par. = 7.83 | 0 1 15 | Par. = 9.66 |
| ^b | 9 28 9 Upsal 2 21 U. 1 10 27 = D. M. 1 12 G. | ^b | 9 54 8 Tornea 3 5 T. 1 37 0 = D. M. 1 12 G. |
| | 8 17 42 1 9 8 19 0 Greenwich. | | 8 17 8 1 53 8 19 0 Greenwich. |
| 0 1 18 | Par. = 9.61 | 0 1 52 | Par. = 8.42 |
| ^b | 9 45 59 Abo 2 29 A. 1 28 30 = D. M. 1 12 G. | ^b | 9 23 40 Calmar 1 58 C. 1 5 35 = D. M. 1 12 G. |
| | 8 17 29 1 17 8 19 0 Greenwich. | | 8 18 5 0 46 8 19 0 Greenwich. |
| 0 1 31 | Par. = 10.04 | 0 0 55 | Par. = 10.16 |
| ^b | 9 28 52 Hernosand 2 26 H. 1 11 12 = D. M. 1 12 G. | ^b | 14 11 34 Calcutta 2 22 C. 5 53 45 = D. M. 1 12 G. |
| | 8 17 40 1 14 8 19 0 Greenwich. | | 8 17 49 1 10 8 19 0 Greenwich. |
| 0 1 20 | Par. = 9.20 | 0 1 11 | Par. = 8.62 |
| | | | 9 30 |

[313]

^b
9 30 11 Stockholm 2 18 S.
1 12 56 = D.M. 1 11 S.H.

^b
8 17 15 1 7
8 18 22 Savile-House.

^b
0 1 7 Par. = 8. 50

^b
9 28 9 Uppsala 2 21 U.
1 10 57 = D.M. 1 11 S.H.

^b
8 17 12 1 10
8 18 22 Savile-House.

^b
0 1 10 Par. = 8. 50

^b
9 54 8 Tornio 3 5 T.
1 37 30 = D.M. 1 11 S.H.

^b
8 16 38 1 54
8 18 22 Savile-House.

^b
0 1 44 Par. = 7. 75

^b
9 45 59 Abo 2 29 A.
1 29 0 = D.M. 1 11 S.H.

^b
8 16 59 1 18
8 18 22 Savile-House.

^b
0 1 23 Par. = 9. 04

^b
9 23 40 Cattmar 1 58 C.
1 6 5 = D.M. 1 11 S.H.

^b
8 17 35 0 47
8 18 22 Savile-House.

^b
0 0 47 Par. = 8. 50

^b
9 28 52 Herno sand 2 26 H.
1 11 42 = D.M. 1 11 S.H.

^b
8 17 10 1 15
8 18 22 Savile-House.

^b
0 1 12 Par. = 8. 16

^b
14 11 34 Calcutta 2 22 C.
5 54 15 = D.M. 1 11 S.H.

^b
8 17 19 1 11
8 18 22 Savile-House.

^b
0 1 3 Par. = 7. 54

^b
9 30 10 Stockholm 2 18 S.
4 7 52 = D.M. 0 59 M.

^b
13 38 2 19
13 39 38 Madras.

^b
0 1 36 Par. = 10. 33

9 28

[314]

| | | | |
|---|--|---|--|
| $\begin{array}{r} 9 \ 28 \ 9 \\ 4 \ 9 \ 51 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 38 \ 0 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 9 \ 21 \ 2 \\ 0 \ 59 \end{array} \text{ U.}$ <hr/> $1 \ 22.$ | $\begin{array}{r} 9 \ 54 \ 8 \\ 3 \ 43 \ 18 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 37 \ 26 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 3 \ 5 \ 5 \\ 2 \ 6 \end{array} \text{ T.}$ <hr/> $2 \ 6$ |
| $0 \ 1 \ 38$ | $\text{Par.} = 10. \ 15$ | $0 \ 2 \ 12$ | $\text{Par.} = 8. \ 90$ |

| | | | |
|--|---|--|---|
| $\begin{array}{r} 9 \ 45 \ 59 \\ 3 \ 51 \ 48 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 37 \ 47 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 2 \ 29 \ 2 \\ 0 \ 59 \end{array} \text{ A.}$ <hr/> $1 \ 30$ | $\begin{array}{r} 9 \ 23 \ 40 \\ 4 \ 14 \ 43 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 38 \ 23 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 1 \ 58 \ 5 \\ 0 \ 59 \end{array} \text{ C.}$ <hr/> $0 \ 59$ |
| $0 \ 1 \ 51$ | $\text{Par.} = 10. \ 48$ | $0 \ 1 \ 15$ | $\text{Par.} = 10. \ 80$ |

| | | | |
|--|---|--|---|
| $\begin{array}{r} 9 \ 28 \ 52 \\ 4 \ 9 \ 6 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 37 \ 58 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 2 \ 26 \ 2 \\ 0 \ 59 \end{array} \text{ H.}$ <hr/> $1 \ 27$ | $\begin{array}{r} 14 \ 11 \ 34 \\ 0 \ 33 \ 27 \end{array} = \text{D.M. o } 59 \text{ M.}$ <hr/> $\begin{array}{r} 13 \ 38 \ 7 \\ 13 \ 39 \ 38 \end{array} \text{ Madras.}$ | $\begin{array}{r} 2 \ 22 \ 2 \\ 0 \ 59 \end{array} \text{ C.}$ <hr/> $1 \ 23$ |
| $0 \ 1 \ 40$ | $\text{Par.} = 9. \ 77$ | $0 \ 1 \ 31$ | $\text{Par.} = 9. \ 32$ |

| | | | |
|---|---|---|---|
| $\begin{array}{r} 9 \ 30 \ 8 \\ 4 \ 7 \ 7 \end{array} = \text{D.M. o } 59 \text{ G.M.}$ <hr/> $\begin{array}{r} 13 \ 37 \ 15 \\ 13 \ 38 \ 30 \end{array} \text{ G. Mount.}$ | $\begin{array}{r} 2 \ 18 \ 2 \\ 0 \ 59 \end{array} \text{ S.}$ <hr/> $1 \ 19$ | $\begin{array}{r} 9 \ 28 \ 9 \\ 4 \ 9 \ 6 \end{array} = \text{D.M. o } 59 \text{ G.M.}$ <hr/> $\begin{array}{r} 13 \ 37 \ 15 \\ 13 \ 38 \ 30 \end{array} \text{ G. Mount.}$ | $\begin{array}{r} 2 \ 21 \ 2 \\ 0 \ 59 \end{array} \text{ U.}$ <hr/> $1 \ 22$ |
| $0 \ 1 \ 15$ | $\text{Par.} = 8. \ 07$ | $0 \ 1 \ 15$ | $\text{Par.} = 7. \ 77$ |

9 54

[315]

| | | | | | |
|---|--------|-------------------------------|--|-----|--------------------------------|
| $\frac{9}{3} \frac{54}{42} \frac{8}{33} = D.M. o$ | Tornea | $\frac{3}{59} G.M.$ | $\frac{9}{3} \frac{45}{51} \frac{8}{3} = D.M. o$ | Abo | $\frac{2}{59} \frac{29}{G.M.}$ |
| $\frac{13}{13} \frac{36}{38} \frac{41}{30} = G. Mount.$ | | $\frac{2}{59} \frac{6}{G.M.}$ | $\frac{13}{13} \frac{37}{38} \frac{2}{30} = G. Mount.$ | | $\frac{1}{59} \frac{30}{G.M.}$ |
| $0 \ 1 \ 49$ | | Par. = " 35 | $0 \ 1 \ 28$ | | Par. = " 31 |

| | | | | | |
|---|--------|-------------------|---|---------|--------------------------------|
| $\frac{9}{4} \frac{23}{13} \frac{40}{58} = D.M. o$ | Calmar | $\frac{1}{59} C.$ | $\frac{9}{4} \frac{28}{8} \frac{52}{21} = D.M. o$ | Hernof. | $\frac{2}{59} \frac{26}{G.M.}$ |
| $\frac{13}{13} \frac{37}{38} \frac{38}{30} = G. Mount.$ | | $\frac{0}{59}$ | $\frac{13}{13} \frac{37}{38} \frac{13}{30} = G. Mount.$ | | $\frac{1}{59} \frac{27}{G.M.}$ |
| $0 \ 0 \ 52$ | | Par. = " 50 | $0 \ 1 \ 17$ | | Par. = " 52 |

| | | | | | |
|---|----------|-------------------------------------|---|---------|-------------------------------------|
| $\frac{14}{0} \frac{11}{34} \frac{34}{12} = D.M. o$ | Calcutta | $\frac{2}{59} C.$ | $\frac{9}{4} \frac{30}{6} \frac{8}{50} = D. M. o$ | Stokolm | $\frac{2}{51} \frac{18}{T. S.}$ |
| $\frac{13}{13} \frac{37}{38} \frac{28}{30} = G. Mount.$ | | $\frac{1}{59} \frac{23}{G. Mount.}$ | $\frac{13}{13} \frac{36}{38} \frac{58}{25} = Tranquebar.$ | | $\frac{1}{51} \frac{27}{G. Mount.}$ |
| $0 \ 1 \ 8$ | | Par. = " 96 | $0 \ 1 \ 27$ | | Par. = " 50 |

| | | | | | |
|---|-------|-------------------------------------|---|--------|-------------------------------------|
| $\frac{9}{4} \frac{28}{8} \frac{9}{49} = D. M. o$ | Upsal | $\frac{2}{51} \frac{21}{T. U.}$ | $\frac{9}{3} \frac{54}{42} \frac{8}{16} = D. M. o$ | Tornea | $\frac{3}{51} \frac{5}{T.}$ |
| $\frac{13}{13} \frac{36}{38} \frac{58}{25} = Tranquebar.$ | | $\frac{1}{51} \frac{30}{G. Mount.}$ | $\frac{13}{13} \frac{36}{38} \frac{24}{25} = Tranquebar.$ | | $\frac{2}{51} \frac{14}{G. Mount.}$ |
| $0 \ 1 \ 27$ | | Par. = " 23 | $0 \ 2 \ 1$ | | Par. = " 68 |

| | | | | | |
|-------------------------------|------------------------|--------------------|-------------------------------|------------------------|--------------------|
| $9^{\circ} 45' 59''$ | Abo | $2^{\circ} 29' A.$ | $9^{\circ} 23' 40''$ | Calmar | $1^{\circ} 58' C.$ |
| $3^{\circ} 50' 46'' = D.M. o$ | $51' T.$ | | $4^{\circ} 13' 41'' = D.M. o$ | $51' T.$ | |
| $13^{\circ} 36' 45''$ | | $1^{\circ} 38'$ | $13^{\circ} 37' 21''$ | | $1^{\circ} 7'$ |
| $13^{\circ} 38' 25''$ | Tranquebar. | | $13^{\circ} 38' 25''$ | Tranquebar. | |
| $0^{\circ} 1^{\circ} 40'$ | | | $0^{\circ} 1^{\circ} 4$ | | |
| | Par. = $8^{\circ} .67$ | | | Par. = $8^{\circ} .12$ | |

The results are set down in the following table.

| | Stok. | Upfal. | Torn. | Cajaa. | Tobsp. | Abo. | Calm. | Hern | Calcu. | O's Par. | Mean. |
|------------------|------------------|------------------|-----------------|-----------------|-----------------|------------------|------------------|-----------------|-----------------|-----------------|-------|
| Paris. | $8^{\circ} .70$ | $8^{\circ} .60$ | $7^{\circ} .92$ | $8^{\circ} .43$ | $8^{\circ} .60$ | $9^{\circ} .03$ | $8^{\circ} .63$ | $8^{\circ} .42$ | $7^{\circ} .83$ | $8^{\circ} .46$ | |
| Greenwich. | $9^{\circ} .66$ | $9^{\circ} .61$ | $8^{\circ} .42$ | $9^{\circ} .09$ | $9^{\circ} .11$ | $10^{\circ} .04$ | $10^{\circ} .16$ | $9^{\circ} .20$ | $8^{\circ} .62$ | $9^{\circ} .32$ | |
| Savile-House. | $8^{\circ} .50$ | $8^{\circ} .50$ | $7^{\circ} .75$ | $8^{\circ} .50$ | $8^{\circ} .65$ | $9^{\circ} .04$ | $8^{\circ} .50$ | $8^{\circ} .16$ | $7^{\circ} .54$ | $8^{\circ} .36$ | |
| Bologna. | $8^{\circ} .62$ | $8^{\circ} .55$ | $8^{\circ} .23$ | $8^{\circ} .44$ | $8^{\circ} .58$ | $8^{\circ} .77$ | $8^{\circ} .31$ | $8^{\circ} .36$ | $8^{\circ} .28$ | $8^{\circ} .43$ | |
| Madras. | $10^{\circ} .33$ | $10^{\circ} .15$ | $8^{\circ} .90$ | $9^{\circ} .72$ | $9^{\circ} .54$ | $10^{\circ} .48$ | $10^{\circ} .80$ | $9^{\circ} .77$ | $9^{\circ} .38$ | $9^{\circ} .89$ | |
| Grand Mount. | $8^{\circ} .87$ | $7^{\circ} .77$ | $7^{\circ} .35$ | $8^{\circ} .07$ | $8^{\circ} .34$ | $8^{\circ} .31$ | $7^{\circ} .50$ | $7^{\circ} .52$ | $6^{\circ} .96$ | $7^{\circ} .76$ | |
| Tranquebar. | $8^{\circ} .50$ | $8^{\circ} .23$ | $7^{\circ} .68$ | $8^{\circ} .36$ | $8^{\circ} .55$ | $8^{\circ} .67$ | $8^{\circ} .12$ | $7^{\circ} .96$ | $7^{\circ} .47$ | $8^{\circ} .37$ | |
| Sun's Par. mean. | $8^{\circ} .91$ | $8^{\circ} .75$ | $8^{\circ} .03$ | $8^{\circ} .66$ | $8^{\circ} .77$ | $9^{\circ} .18$ | $8^{\circ} .86$ | $8^{\circ} .48$ | $8^{\circ} .00$ | $8^{\circ} .63$ | |

The mean of these 63 results gives the Sun's parallax = $8^{\circ} .63$; and if we reject all those which differ more than one second from the mean of the whole the mean of the remaining 49 results gives the Sun's parallax = $8^{\circ} .50$.

And if we reject all those which differ more than half a second from the mean of the whole, the mean of the remaining 37 results gives the Sun's parallax = $8^{\circ} .535$; the mean therefore of these three means gives the Sun's parallax = $8^{\circ} .55$.

Thus

Thus by the mean of 53 comparisons the Sun's parallax is determined to be $= 8', 58$, and by the mean of 63 comparisons the Sun's parallax is determined to be $= 8'', 55$. The mean of these two means gives $8'', 565$ for the parallax of the Sun on the day of the transit.

It may be objected, that this determination cannot be depended on to a very great precision, because the greatest difference of the effect of the parallaxes in any of these comparisons does not exceed $3' 31''$: consequently that this is too small a base, from which we can expect any great exactness in the determination of the Sun's parallax: But if we consider the great number of comparisons (no less than 116), the certainty of the differences of longitude of most of the places of observation, and the small differences in the results themselves, I cannot help thinking that the force of this objection is in some measure removed; and that this determination of the Sun's parallax, by the observations at places on this side of the *Line* only, must be very near the truth.

In order, therefore, to remove the force of this objection entirely, let us next consider the observation at the Cape of Good Hope, by which we shall have a base very near three times greater than the former; and also the observation at Rodrigues, by which the base is nearly double of the former. But before I proceed I must take notice, that, in the Mémoire, by M. Pingré, before mentioned, the time of the internal contact at the egress at Rodrigues is set down at ob $36' 49''$. But in the same volume there is an account of M. Pingré's observation sent to the R. Academy before his arrival in Europe, and the

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time

time of the internal contact is therein set down at $0^h 34' 47''$. Also in a letter from him to the R. Society, on his arrival in Europe at Lisbon, and dated the 6th of March 1762, and which letter is printed in the Phil. Transactions, vol. LII. part I. the time of the internal contact is therein set down at $0^h 34' 47''$ true time. In another letter from him at Lisbon to the Royal Society, dated the 14th of March 1762, the time of the internal contact is again set down at $0^h 34' 47''$ true time, and he ends this letter in these words, "Notez que l'attouchement interne des Bords s'est faite à $0^h 34' 47'$. Je fais cette remarque, parceque, vû la proximité de prononciation, qui dans notre langue est entre 30 and 40, celle attouchement se trouvoit marqué 10'' plutot qu'il ne devoit l'être, dans une copie que j'ai faite pour mon usage; cette erreur aura peut être passé dans quelque autre copie. Mais, selon l'original, il faut absolument lire $0^h 34' 47''$. M. Pingré has no where, that I can find, in the said memoir given any reason for this alteration of the time of the contact. If the internal contact at the egress at Rodrigues happened at $0^h 34' 47''$, and if this is compared with the same observation at Tobolsk, the parallax of the Sun comes out = 7'', 36. If the time of the contact at Rodrigues was at $0^h 35' 47''$, and if this is compared with the same observation at Tobolsk, then the parallax of the Sun is found = 8'', 62. Again if the time of the contact at Rodrigues was at $0^h 36' 49''$, and if this is compared with the observation at Tobolsk, the parallax of the Sun will be found = 9'', 93. But to return.

M. Pingré, in his letter to the Royal Society dated at Lisbon the 14th of March 1762, sets down the time of the internal contact at the egress at $0^h 34' 47''$ true-

true time, and with regard to the time of the external contact expresses himself thus "à $0^h\ 53' 18''$ le soleil a paru pendant 3 ou 4 secondes. Je n'ai pas vu le disque du soleil bien fermé, il me paroissait un peu alteré au lieu de la sortie de Venus. M. Thuillier ne voyoit rien avec la Lunette de 9 pieds. J'ai de la peine à me persuader que Venus soit sortie plutot." It is plain from these words that M. Pingré believed that the external contact did not happen before $0^h\ 53' 18''$. This being allowed, let us compute the duration of the egress at Rodrigues, which we shall find = $17' 55''$. It follows, therefore, that the internal contact happened at $0^h\ 35' 23''$. But this supposes that the observer could see the very last contact of Venus with the Sun's limb, the contrary of which I have shewn in a former paper on this subject. We are therefore certain that the external contact happened later than $0^h\ 53' 18''$, by several seconds, consequently the internal contact happened later than $0^h\ 35' 23''$ by several seconds. Upon the whole, therefore, we may safely conclude that there is a mistake of one minute in setting down the time of the internal contact at the egress at Rodrigues, and that, instead of $0^h\ 34' 47''$, it should be $0^h\ 35' 47''$. This sort of mistake has happened several times in the observations of this transit, but they are easily discovered.

I shall now proceed to compare the observation of the internal contact at the Cape, with the observation of the same contact at Rodrigues and at 20 places to the north of the *Line*, and also the observation at Rodrigues with the same 20 places, and they are as follow.

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| | |
|--|--|
| ^a 9 39 50 Cape 6 8 C. 0 29 7 = D.M. 0 29 R. | ^b 9 39 50 Cape 6 8 C. 1 4 19 = D.M. 0 53 P. |
| ^a 12 38 57 3 9 12 35 47 Rodrigues. | ^b 8 35 31 7 1 8 28 27 Paris. |
| 0 3 10 Par. = 8. 54 | 0 7 4 Par. = 8. 56 |

| | |
|---|---|
| ^a 6 39 50 Cape 6 8 C. 0 28 14 = D.M. 0 29 B. | ^b 9 39 50 Cape 6 8 C. 0 23 42 = D.M. 0 13 R. |
| ^a 9 11 36 6 37 9 4 57 Bologna. | ^b 9 16 8 6 21 9 9 36 Rome. |
| 0 6 39 Par. = 8. 54 | 0 6 32 Par. = 8. 74 |

| | |
|---|--|
| ^a 9 39 50 Cape 6 8 C. 0 29 31 = D.M. 0 24 F. | ^b 9 39 50 Cape 6 8 C. 0 34 3 = D.M. 1 18 G. |
| ^a 9 10 19 6 32 9 3 28 Florence. | ^b 9 5 47 7 26 8 58 26 Gottingen. |
| 0 6 51 Par. = 8. 91 | 0 7 21 Par. = 8. 40 |

| | |
|---|---|
| ^a 9 39 50 Cape 6 8 C. 1 13 35 = D.M. 1 12 G. | ^b 9 39 50 Cape 6 8 1 14 5 = D.M. 1 11 S.H. |
| ^a 8 26 15 7 20 8 19 0 Greenwich. | ^b 8 25 45 7 19 8 18 22 Savile-House. |
| 0 7 15 Par. = 8. 40 | 0 7 23 Par. = 8. 57 |

[321.]

^b 9 39 50 Cape 6 8 C.
^a 1 32 7 = D. M. 1 4 L.

8 7 43 7 12
8 0 21 Leskeard.

0 7 22

Par. = 8. 69

^b 9 39 50 Cape 6 8 C.
^a 0 8 0 = D. M. 1 59 C.

9 31 50 8 7
9 23 40 Calmar.

0 8 10

Par. = 8. 55

^b 9 39 50 Cape 6 8 C.
^a 0 2 23 = D. M. 2 26 H:

9 37 27 8 34
0 28 52 Hernosand.

0 8 35

Par. = 8. 51

^b 9 39 50 Cape 6 8 C.
^a 0 3 8 = D. M. 2 21 U:

9 36 42 8 29
9 28 9 Upsal.

0 8 33

Par. = 8. 57

^b 9 39 50 Cape 6 8 C.
^a 0 1 9 = D. M. 2 18 S:

9 38 41 8 26
9 30 10 Stokholm.

0 8 31

Par. = 8. 58

^b 9 39 50 Cape 6 8 C:
^a 0 14 55 = D. M. 2 30 A:

9 54 45 8 38
9 45 59 Abo.

0 8 46

Par. = 8. 63

^b 9 39 50 Cape 6 8 C:
^a 0 37 20 = D. M. 2 59 C:

10 17 10 9 7
10 7 59 Cajaneburg.

0 9 11

Par. = 8. 56

^b 9 39 50 Cape 6 8 C:
^a 0 23 25 = D. M. 3 5 T:

10 3 15 9 13
9 54 8 Tornea.

0 9 7

Par. = 8. 41

[322]

| | | | | | | | |
|---|----------|--|------|--|-----------|--|------|
| $\begin{array}{r} 9 \\ 39 \\ 50 \end{array}$ | Cape | $\begin{array}{r} 6 \\ 8 \end{array}$ | C. | $\begin{array}{r} 9 \\ 39 \\ 50 \end{array}$ | Cape | $\begin{array}{r} 6 \\ 8 \end{array}$ | C. |
| $\underline{3 \ 19 \ 32}$ | = D. M. | $\underline{3 \ 44}$ | T. | $\underline{4 \ 40 \ 10}$ | = D. M. | $\underline{2 \ 22}$ | C. |
| $\begin{array}{r} 12 \\ 59 \\ 22 \end{array}$ | | $\begin{array}{r} 9 \\ 52 \end{array}$ | | $\begin{array}{r} 14 \\ 20 \\ 0 \end{array}$ | | $\begin{array}{r} 8 \\ 30 \end{array}$ | |
| $\underline{12 \ 49 \ 23}$ | Tobolsk. | | | $\underline{14 \ 11 \ 34}$ | Calcutta. | | |
| $\begin{array}{r} 0 \\ 9 \\ 59 \end{array}$ | | | | $\begin{array}{r} 0 \\ 8 \\ 26 \end{array}$ | | | |
| | Par. | = | " 64 | | Par. | = | " 43 |

| | | | | | | | |
|---|---------|---------------------------------------|------|---|--------------|---------------------------------------|------|
| $\begin{array}{r} 9 \\ 39 \\ 50 \end{array}$ | Cape | $\begin{array}{r} 6 \\ 8 \end{array}$ | C. | $\begin{array}{r} 9 \\ 39 \\ 50 \end{array}$ | Cape | $\begin{array}{r} 6 \\ 8 \end{array}$ | C. |
| $\underline{4 \ 6 \ 43}$ | = D. M. | $\underline{0 \ 59}$ | M. | $\underline{4 \ 5 \ 58}$ | = D. M. | $\underline{0 \ 59}$ | G.M. |
| $\begin{array}{r} 13 \\ 46 \\ 33 \end{array}$ | | $\begin{array}{r} 7 \\ 7 \end{array}$ | | $\begin{array}{r} 13 \\ 45 \\ 48 \end{array}$ | | $\begin{array}{r} 7 \\ 7 \end{array}$ | |
| $\underline{13 \ 39 \ 38}$ | Madras. | | | $\underline{13 \ 38 \ 30}$ | Grand Mount. | | |
| $\begin{array}{r} 0 \\ 6 \\ 55 \end{array}$ | | | | $\begin{array}{r} 0 \\ 7 \\ 18 \end{array}$ | | | |
| | Par. | = | " 28 | | Par. | = | " 70 |

| | | | | |
|---|-------------|--|------|--|
| $\begin{array}{r} 9 \\ 39 \\ 50 \end{array}$ | Cape | $\begin{array}{r} 6 \\ 8 \end{array}$ | C. | Rodrigues and the following places compared together, |
| $\underline{4 \ 5 \ 41}$ | = D. M. | $\underline{0 \ 51}$ | M. | |
| $\begin{array}{r} 13 \\ 45 \\ 31 \end{array}$ | | $\begin{array}{r} 6 \\ 59 \end{array}$ | | |
| $\underline{13 \ 38 \ 25}$ | Tranguebar. | | | |
| $\begin{array}{r} 0 \\ 7 \\ 6 \end{array}$ | | | | |
| | Par. | = | " 60 | |

| | | | | | | | |
|---|---------|--|------|---|----------|--|------|
| $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | Rodr. | $\begin{array}{r} 2 \\ 59 \end{array}$ | R. | $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | Rodr. | $\begin{array}{r} 2 \\ 59 \end{array}$ | R. |
| $\underline{4 \ 3 \ 26}$ | = D. M. | $\underline{0 \ 53}$ | P. | $\underline{3 \ 27 \ 21}$ | = D. M. | $\underline{0 \ 29}$ | B. |
| $\begin{array}{r} 8 \\ 32 \\ 21 \end{array}$ | | $\begin{array}{r} 3 \\ 52 \end{array}$ | | $\begin{array}{r} 9 \\ 8 \\ 26 \end{array}$ | | $\begin{array}{r} 3 \\ 28 \end{array}$ | |
| $\underline{8 \ 28 \ 27}$ | Paris. | | | $\underline{9 \ 4 \ 57}$ | Bologna. | | |
| $\begin{array}{r} 0 \\ 3 \\ 54 \end{array}$ | | | | $\begin{array}{r} 0 \\ 3 \\ 29 \end{array}$ | | | |
| | Par. | = | " 58 | | Par. | = | " 54 |

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| | | | |
|---|--|---|--|
| $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ | $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ |
| $\begin{array}{r} 3 \\ 22 \\ 49 \end{array}$ | $= D. M. o$ | $\begin{array}{r} 13 \\ R. \end{array}$ | $\begin{array}{r} 3 \\ 28 \\ 38 \end{array}$ |
| $\underline{\underline{9}} \quad \underline{\underline{12}} \quad \underline{\underline{58}}$ | | $\begin{array}{r} 3 \\ 12 \end{array}$ | $\begin{array}{r} 9 \\ 7 \\ 9 \end{array}$ |
| $\begin{array}{r} 9 \\ 9 \\ 36 \end{array}$ | Rome. | | $\begin{array}{r} 3 \\ 23 \end{array}$ |
| $\begin{array}{r} 0 \\ 3 \\ 22 \end{array}$ | | | $\begin{array}{r} 9 \\ 3 \\ 28 \end{array}$ |
| | Par. = 8. 94 | | Florence. |
| | | $\begin{array}{r} 0 \\ 3 \\ 41 \end{array}$ | |
| | | | Par. = 9. 24 |

| | | | |
|--|--|---|--|
| $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ | $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ |
| $\begin{array}{r} 3 \\ 33 \\ 10 \end{array}$ | $= D. M. i$ | $\begin{array}{r} 18 \\ G. \end{array}$ | $\begin{array}{r} 4 \\ 12 \\ 42 \end{array}$ |
| $\underline{\underline{9}} \quad \underline{\underline{2}} \quad \underline{\underline{37}}$ | | $\begin{array}{r} 4 \\ 17 \end{array}$ | $\begin{array}{r} 8 \\ 23 \\ 5 \end{array}$ |
| $\begin{array}{r} 8 \\ 58 \\ 26 \end{array}$ | Gottingen. | | $\begin{array}{r} 8 \\ 19 \\ o \end{array}$ |
| $\begin{array}{r} 0 \\ 4 \\ 11 \end{array}$ | | | Greenwich. |
| | Par. = 8. 30 | | |
| | | $\begin{array}{r} 0 \\ 4 \\ 5 \end{array}$ | |
| | | | Par. = 8. 33 |

| | | | |
|---|--|---|--|
| $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ | $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ |
| $\begin{array}{r} 4 \\ 13 \\ 12 \end{array}$ | $= D. M. i$ | $\begin{array}{r} 11 \\ S.H. \end{array}$ | $\begin{array}{r} 4 \\ 31 \\ 14 \end{array}$ |
| $\underline{\underline{8}} \quad \underline{\underline{22}} \quad \underline{\underline{35}}$ | | $\begin{array}{r} 4 \\ 10 \end{array}$ | $\begin{array}{r} 8 \\ 4 \\ 33 \end{array}$ |
| $\begin{array}{r} 8 \\ 18 \\ 22 \end{array}$ | Savile-House. | | $\begin{array}{r} 8 \\ 0 \\ 21 \end{array}$ |
| $\begin{array}{r} 0 \\ 4 \\ 13 \end{array}$ | | | Leskeard. |
| | Par. = 8. 59 | | |
| | | $\begin{array}{r} 0 \\ 4 \\ 12 \end{array}$ | |
| | | | Par. = 8. 81 |

| | | | |
|---|--|---|--|
| $\begin{array}{r} 12 \\ 39 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ | $\begin{array}{r} 12 \\ 35 \\ 47 \end{array}$ | $\begin{array}{r} 2 \\ 59 \\ R. \end{array}$ |
| $\begin{array}{r} 3 \\ 7 \\ 7 \end{array}$ | $= D. M. i$ | $\begin{array}{r} 59 \\ C. \end{array}$ | $\begin{array}{r} 3 \\ 1 \\ 30 \end{array}$ |
| $\underline{\underline{9}} \quad \underline{\underline{28}} \quad \underline{\underline{40}}$ | | $\begin{array}{r} 4 \\ 58 \end{array}$ | $\begin{array}{r} 9 \\ 34 \\ 17 \end{array}$ |
| $\begin{array}{r} 9 \\ 23 \\ 40 \end{array}$ | Calmar. | | $\begin{array}{r} 5 \\ 25 \end{array}$ |
| $\begin{array}{r} 0 \\ 5 \\ 0 \end{array}$ | | | $\begin{array}{r} 9 \\ 28 \\ 52 \end{array}$ |
| | Par. = 8. 56 | | Hernosand. |
| | | $\begin{array}{r} 0 \\ 5 \\ 45 \end{array}$ | |
| | | | Par. = 8. 50 |

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| | | | | | | | |
|-------------------------|-----------|--------------------|----|-------------------------|-------------|--------------------|----|
| $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. | $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. |
| $3 \ 2 \ 15 = D. M.$ | | $2 \ 21$ | U. | $3 \ 0 \ 16 = D. M.$ | | $2 \ 18$ | S. |
| $9 \ 33 \ 32$ | | $5 \ 20$ | | $9 \ 35 \ 31$ | | $5 \ 17$ | |
| $9 \ 28 \ 9$ | Upsal. | | | $9 \ 30 \ 11$ | Stokholm. | | |
| $0 \ 5 \ 23$ | | | | $0 \ 5 \ 20$ | | | |
| | Par. = | $8. \ 58$ | | | Par. = | $8. \ 58$ | |
| $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. | $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. |
| $2 \ 44 \ 12 = D. M.$ | | $2 \ 30$ | A. | $2 \ 21 \ 47 = D. M.$ | | $2 \ 59$ | C. |
| $9 \ 51 \ 35$ | | $5 \ 29$ | | $10 \ 14 \ 0$ | | $5 \ 58$ | |
| $9 \ 45 \ 59$ | Abo. | | | $10 \ 7 \ 59$ | Cajaneburg. | | |
| $0 \ 5 \ 36$ | | | | $0 \ 6 \ 1$ | | | |
| | Par. = | $8. \ 68$ | | | Par. = | $8. \ 57$ | |
| $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. | $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. |
| $2 \ 35 \ 42 = D. M.$ | | $3 \ 5$ | T. | $0 \ 20 \ 25 = D. M.$ | | $3 \ 44$ | T. |
| $10 \ 0 \ 5$ | | $6 \ 4$ | | $12 \ 56 \ 12$ | | $6 \ 43$ | |
| $9 \ 54 \ 8$ | Tornea. | | | $12 \ 49 \ 23$ | Tobolsk. | | |
| $0 \ 5 \ 57$ | | | | $0 \ 6 \ 49$ | | | |
| | Par. = | $8. \ 33$ | | | Par. = | $8. \ 62$ | |
| $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. | $\frac{1}{2} \ 35 \ 47$ | Rodr. | $\frac{1}{2} \ 59$ | R. |
| $1 \ 41 \ 3 = D. M.$ | | $2 \ 22$ | C. | $1 \ 7 \ 36 = D. M.$ | | $0 \ 59$ | M. |
| $14 \ 16 \ 50$ | | $5 \ 21$ | | $13 \ 43 \ 23$ | | $3 \ 58$ | |
| $14 \ 11 \ 34$ | Calcutta. | | | $13 \ 39 \ 38$ | Madras. | | |
| $0 \ 5 \ 16$ | | | | $0 \ 3 \ 45$ | | | |
| | Par. = | $8. \ 37$ | | | Par. = | $8. \ 03$ | |

12 35

| | | | | | |
|---------------------|-----------------------|-----------|---------------------|-----------------------|------|
| $\frac{1}{2} 35 47$ | " | Rodr. | $\frac{2}{2} 59$ | " | R. |
| 1 6 51 | =D.M. | o 59 G.M. | 1 6 34 | = D. M. o 51 T. | |
| $\frac{1}{2} 35 47$ | " | Rodr. | $\frac{2}{2} 59$ | " | R. |
| 1 6 34 | = D. M. o 51 T. | | | | |
| $\frac{1}{3} 42 38$ | | 3 58 | $\frac{1}{3} 42 21$ | | 3 50 |
| $\frac{1}{3} 38 30$ | Grand Mount. | | $\frac{1}{3} 38 25$ | Tranquebar. | |
| 0 4 8 | | | 0 3 56 | | |
| | Par. = $8^{\circ} 85$ | | | Par. = $8^{\circ} 74$ | |

The results of the Sun's parallax from these several comparisons are as follow.

| Sun's Parallax. | Sun's Parallax. |
|---|---|
| Cape of G. Hope and Rodrigues = $8^{\circ} .54$ | Rodrigues and Cape of G. Hope = $8^{\circ} .54$ |
| Paris - - = 8.56 | Paris - - = 8.58 |
| Bologna - = 8.54 | Bologna - = 8.54 |
| Rome - = 8.74 | Rome - = $8.94 r.$ |
| Florence - = $8.91 r.$ | Florence - = $9.24 r.$ |
| Göttingen - = 8.40 | Göttingen - = $8.30 r.$ |
| Greenwich - = 8.40 | Greenwich - = $8.33 r.$ |
| Savile-House = 8.57 | Savile-House = 8.59 |
| Lefkard - = 8.69 | Lefkard - = $8.81 r.$ |
| Calmar - = 8.55 | Calmar - = 8.56 |
| Hernofand - = 8.51 | Hernofand - = 8.50 |
| Upfal - = 8.57 | Upfal - - = 8.58 |
| Stokholm - = 8.58 | Stokholm - - = 8.58 |
| Abo - - = 8.63 | Abo - - = 8.68 |
| Cajaneburg - = 8.50 | Cajaneburg - = 8.57 |
| Tornea - - = 8.41 | Tornea - - = $8.33 r.$ |
| Tobolik - = 8.64 | Tobolik - = 8.62 |
| Calcutta - = 8.43 | Calcutta - = 8.37 |
| Madras - = $8.28 r.$ | Madras - = $8.03 r.$ |
| G. Mount - = 8.70 | G. Mount - = $8.85 r.$ |
| Tranquebar = 8.60 | Tranquebar = 8.74 |

The mean of the 21 comparisons with the observation at the Cape, gives the Sun's parallax = $8^{\circ}, 56$. There are only two of these 21 comparisons, marked with the letter *r*, which differ more than $\frac{1}{10}$ of a second from the mean of the whole; let these be rejected, and the mean of the remaining 19 results gives the Sun's parallax = $8^{\circ}, 56$.

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If we select out of these 21 comparisons those places whose difference of longitude may be supposed to be the best determined, the mean of these may be regarded as the most exact determination, viz. Paris, Bologna, Greenwich, Savile-House, Upsal, Stokholm, Cajaneburg and Tobolsk; the mean of these gives the Sun's parallax = $8'',\ 55$, and if we leave out the results of Greenwich and Tobolsk, which differ the most from the rest, the mean of the remaining 6 results gives it = $8'',\ 56$ the same as before.

The mean of the 21 comparisons with the observation at Rodrigues gives the Sun's parallax = $8'',\ 57$, and if we reject 8 of them, which are marked with the letter *r*, and which differ more than $\frac{2}{3}$ of a second from the mean of the whole, the mean of the remaining 13 results gives the Sun's parallax $8'',\ 57$, differing only one hundredth part of a second from that which was determined from the observation at the Cape, and agreeing in a most surprizing manner with what was formerly determined by the comparisons of the observations at places on this side of the *Line* only, where the base was so small, as I said before; a most convincing proof of the great precision with which the parallax of the Sun is determined by the late transit of Venus.

We shall now enquire into the limits of the error that may attend the determination of the parallax by the observation of the internal contact. An error of $1' 10''$ of time in the observation at Tobolsk when compared with the observation at the Cape, will produce an error of $1''$ in the Sun's parallax: and if we suppose an error of $35''$ of time in the observation,

vation at Tobolsk, and an error of the same quantity in the observation at the Cape, and both in contrary directions, this also will produce an error of only $1''$ in the Sun's parallax. If therefore no greater error could be committed in the observations at Tobolsk and the Cape, we are certain that the comparison of Tobolsk and the Cape gives the Sun's parallax so exact, that the error does not exceed one second from the true parallax. But this is too great an error to be supposed in the observations, because I have shewn, in my former paper, that an error of only $6''$ in time was committed in the observation of the contact by persons observing even in the same place; therefore, if we suppose an error of $6''$ of time in the observation at Tobolsk, and an error of the same quantity in the observation at the Cape, and both in contrary directions, the error produced in the parallax by those $12''$, will amount only to $\frac{1}{2}$ of a second, even though we had only these two observations to determine the Sun's parallax: But since we have a great number of very good observations, made at other places, it follows that the mean of all these, must give the Sun's parallax to a less error than $\frac{1}{2}$ of a second, and consequently very near the truth.

In all places where the internal contact at the egress was observed, and where there were more observers than one, we find a difference in the time of each observer; the observation at Greenwich is an exception to this, as the three observers all agree to the same second, in the observation of the contact of Venus with the Sun's limb; which is the more surprising as they used telescopes of different constructions

tions and of different magnifying powers. This coincidence not only surprized me, but also the reverend Mr. Hornsby, now Savilian professor of Astronomy at Oxford. Mr. Hornsby went to Greenwich in the beginning of the year 1762, and on his return told me, that his surprize was at an end, for he had been informed at Greenwich, that Mr. Green, the assistant observer there, as soon as he judged that the internal contact was formed, called out *now*. This must certainly have caused some disturbance to the other observers, and might possibly influence their judgment: and the fact (as I am informed) was that each observer had a second watch in his hand, and they instantly stoped their watches, each having his hand at his watch ready to stop. This problem, therefore, is easily solved, and the surprize at the coincidence entirely vanishes; so that this observation can be looked on as no more than the observation of one person, and he too not much practised in observing. Moreover it is proper I should observe that another person was present at this observation, who confirmed the above account.

The very near coincidence of the three observers at Greenwich, in the time of the external contact remains now to be accounted for. Mr. Green did not call out at this time, because he was forbid by Dr. Bradley, who was present, though not in a condition to observe because of his bad state of health. This problem therefore may be solved in the following manner. The observation of the external contact was undoubtedly more uncertain than the former, and yet we find two of the observers agreeing to the same second, and the third differing only one second from them. If we attend to the following circum-

stances, we shall be immediately satisfied by them. Each observer had a second-watch in his hand; the three observers were at the same window of the same room, one of them on the leads immediately without the window, and the other two within the window; therefore each observer was within hearing and seeing of each other; consequently the instant one of the observers stopped his watch, may it not be presumed that the noise of the ticking of it might be heard by the rest? especially as there was a profound silence during the time of the observation.

I have thought proper to take notice of these facts, because several persons both at home and abroad have expressed their surprize at this coincidence, and that such an exactness may not be established as a precedent in these sort of observations; and because I think it essentially necessary, in all sorts of observations, especially in one of so much importance in astronomy as this, that every the minutest circumstance should be particularly related.

We are now to find the limits of the error arising from the difference of longitude between Tobolsk and the Cape. I find that an error of $1' 10''$ in time in the difference of longitude between these two places will cause an error of $1''$ in the Sun's parallax. But as we are certain that this error in longitude does not take place; therefore we are certain that the error in the parallax is within one second of the truth. The difference of longitude between the Cape of Good Hope and Paris is determined, both by the observations of M. de la Caille and Mr. Masson; the difference of longitude between Paris and Upsal in Sweden is settled by the observations of Jupiter's

piter's first satellite, and the difference of longitude between Upsal and Tobolsk is settled, by the observations of the contact at the ingress at both places, by the method of M. Pingré above mentioned. Therefore the difference of longitude between the Cape and Tobolsk is very exactly settled, so exactly, that I am perswaded that the error does not amount to 5 or 6 seconds. Therefore the error in the parallax arising from the error of the difference of longitude is extremely small, scarcely amounting to $\frac{1}{10}$ part of a second. Therefore we are certain that the error in the sun's parallax arising both from the error of observation and the error of longitude does not exceed $\frac{1}{4}$ of a second in the comparison of the observations of the internal contact at Tobolsk and at the Cape, even though we had no more observations to determine the Sun's parallax; but the mean of a great many more must bring it very near the truth.

I now proceed to determine the parallax of the Sun from the total durations observed at different places. If therefore we compare the durations observed at Tobolsk, Cajaneburg, Abo, and Tornea, with the durations observed at Madras, Grand Mount and Tranquebar, which give the greatest differences, the results of the Sun's parallax will be as follow.

Tobolsk

| | Sun's Par. | Difference of duration. |
|--------------------------|------------|-------------------------|
| Tobolsk and Madras - - = | " 9. 61 | ' 2' 50" |
| G. Mount = | 8. 33 | 2' 27" |
| Tranquebar = | 8. 52 | 2' 40" |
| Cajaneburg and Madras = | 10. 09 r. | 1' 49" |
| G. Mount = | 8. 00 | 1' 26" |
| Tranquebar = | 8. 33 | 1' 39" |
| Abo and Madras - - - = | 10. 66 r. | 1' 34" |
| G. Mount = | 8. 33 | 1' 11" |
| Tranquebar = | 8. 60 | 1' 24" |
| Tornea and Madras - - = | 9. 20 | 1' 34" |
| G. Mount - = | 7. 00 r. | 1' 11" |
| Tranquebar = | 7. 50 r. | 1' 24" |

The mean of these 12 results gives the Sun's parallax = 8", 68, and if we reject four of them, which are marked with the letter *r*, and which differ the most from the rest, the mean of the remaining 8 gives the Sun's parallax = 8", 61.

This determination of the Sun's parallax cannot be depended on to any great precision, because of the small differences between the durations compared, the greatest of which amounts only to 2' 50", and also because of the small number of comparisons. It serves only to shew nearly what is the quantity of the Sun's parallax.

We are now to determine the limits of the error in the determination of the Sun's parallax by the durations observed at two different places. The greatest difference of duration is between Tobolsk and Madras, which amounts only to 2' 50". If therefore

an error of $20''$ in time is committed in the observations of the ingress and egress at both the places compared, this error of $20''$ in time will cause an error of $1''$ in the result of the Sun's parallax, and in the comparisons of those places where the difference of duration is less, will occasion a greater error, and therefore the determination of the parallax, by this method, cannot be depended on to any great exactness, because of the small differences of the durations compared. In this method, however, we are free from the uncertainty arising from the difference of longitude not being exactly known.

I now proceed to the determination of the Sun's parallax by the least distance of the centers. There are come to my hands only two measurements of the greatest distance of the limbs of the Sun and Venus, one at Tobolsk and the other at Rodrigues. I shall only consider the measurement at Rodrigues, because there seems to me to be some mistake in the measurement at Tobolsk. If we suppose the Sun's parallax = $8'', 5$, then the apparent middle of the transit happened at Rodrigues at $9^h 37' 30''$. There is a measurement by M. Pingré of the greatest distance of the limbs at $9^h 38' 13''$, which is so near the middle of the transit that we may safely take this quantity, viz. $5' 54'', 6^*$, for the greatest distance of the limbs of the Sun and Venus, and especially as it is marked an exact observation. This measurement, therefore, gives the apparent least distance of the centers of the

* I all along consider the observation of M. Pingré at Rodrigues as it is printed, from his own letter, in the Philosophical Transactions.

Sun

Sun and Venus at Rodrigues = $9^{\circ} 21''$, 4. Supposing then this measurement to be exact, here follows an irrefragable argument, independent of all other methods, to prove that the parallax of the Sun is very nearly = $8'', 5.$ Let us suppose the Sun's parallax = $10'',$ and let us compute, by the following method, the apparent least distance of the centers at Tobolsk; from thence we shall find that the geocentric least distance of the centers at Tobolsk is $567'', 416;$ and by the observation at Rodrigues the geocentric least distance of the centers is = $572'', 612,$ so that, on this supposition, we have two different geocentric least distances of the centers, which being absurd, it follows that the Sun's parallax is not $10''.$ Again let us suppose that the Sun's parallax is = $7'',$ we shall find that the geocentric least distance of the centers by the observation at Tobolsk is = $575'', 356,$ and by the observation at Rodrigues it is = $569'', 248.$ Thus then, again, we have two different geocentric least distances of the centers, which being absurd, it follows that the parallax of the Sun is not $7''.$ Again if we suppose the Sun's parallax = $8''$ or $9'',$ we shall find that the same absurdity will follow, but in these two last suppositions we shall find that the differences of the geocentric least distances of the centers are not so great as on the suppositions of $10''$ and $7'',$ it therefore follows that the parallax of the Sun is less than $9''$ and more than $8,$ and if we continue to reason in the same manner we shall find, that on the supposition that the Sun's parallax is = $8'', 5,$ the geocentric least distances of the centers severally found by the observation

vation at Tobolsk and at Rodrigues is very nearly the same, consequently that the Sun's parallax is very nearly $= 8'', .5$. If we pursue this subject to a greater precision, and suppose that the measurement of the greatest distance of the limbs of the Sun and Venus, taken by M. Pingré, to be perfectly exact, and compute on true * principles the apparent least distances of the centers from the durations observed at the different places in the north (the method of which I shall afterwards give) the parallax of the Sun will come out as follows, when they are compared with that measured at Rodrigues :

| Rodrigues | Cajan. | Calm. | Tobol. | Torn. | Upsal. | Stoka. | Abo. | Heno. |
|-----------|----------|----------|----------|----------|----------|----------|----------|----------|
| | 8'', .60 | 8'', .58 | 8'', .65 | 8'', .48 | 8'', .60 | 8'', .40 | 8'', .63 | 8'', .55 |

The mean of these eight comparisons gives the Sun's parallax $= 8'', .56$ being the very same, as that which we found before by the comparisons of the internal contacts.

Again let us reduce the observed durations, at the following several places, to the center, on the supposition that the Sun's parallax is $= 8'', .56$ as in the following table.

| Tobolsk. | Cajaneburg. | Tornea. |
|---------------------|---------------------|---------------------|
| 5 48. 53 = Obs. Du. | 5 49. 54 = Obs. Du. | 5 50. 9 = Obs. Du. |
| 0 9. 6 = Parallax. | 0 8. 8 = Parallax. | 0 8. 3 = Parallax. |
| 5 58. 59 = Cent. D. | 5 58. 2 = Cent. D. | 5 58. 12 = Cent. D. |

* I say on true principles, because I have reason to think that there is a mistake in the method given by M. Pingré in the aforesaid Memoir.

Upsal.

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| Upfal. | Stokholm. | Abo. |
|---|---|---|
| $5^{\text{h}} 50' 26'' = \text{Obs. Du.}$ | $5^{\text{h}} 50' 42'' = \text{Obs. Du.}$ | $5^{\text{h}} 50' 9'' = \text{Obs. Du.}$ |
| $0^{\text{m}} 7.36 = \text{Parallax.}$ | $0^{\text{m}} 7.37 = \text{Parallax.}$ | $0^{\text{m}} 7.49 = \text{Parallax.}$ |
| $5^{\text{h}} 58' 2'' = \text{Cent. D.}$ | $5^{\text{h}} 58' 19'' = \text{Cent. D.}$ | $5^{\text{h}} 57' 58'' = \text{Cent. D.}$ |
| Hernosand. | Calmar. | Calcutta. |
| $5^{\text{h}} 50' 26'' = \text{Obs. Du.}$ | $5^{\text{h}} 50' 39'' = \text{Obs. Du.}$ | $5^{\text{h}} 50' 36'' = \text{Obs. Du.}$ |
| $0^{\text{m}} 7.39 = \text{Parallax.}$ | $0^{\text{m}} 7.24 = \text{Parallax.}$ | $0^{\text{m}} 7.35 = \text{Parallax.}$ |
| $5^{\text{h}} 58' 5'' = \text{Cent. D.}$ | $5^{\text{h}} 58' 3'' = \text{Cent. D.}$ | $5^{\text{h}} 58.11'' = \text{Cent. D.}$ |
| Madras. | Grand Mount. | Abo. |
| $5^{\text{h}} 51' 43'' = \text{Obs. Du.}$ | $5^{\text{h}} 51' 20'' = \text{Obs. Du.}$ | $5^{\text{h}} 51' 33'' = \text{Obs. Du.}$ |
| $0^{\text{m}} 6.35 = \text{Parallax.}$ | $0^{\text{m}} 6.35 = \text{Parallax.}$ | $0^{\text{m}} 6.26 = \text{Parallax.}$ |
| $5^{\text{h}} 58' 18'' = \text{Cent. D.}$ | $5^{\text{h}} 57' 55'' = \text{Cent. D.}$ | $5^{\text{h}} 57' 59'' = \text{Cent. D.}$ |

The mean of these 12 central durations gives the mean central duration $= 5^{\text{h}} 58' 5''$; from this central duration, we shall find that the geocentric least distance of the centers is $= 571''$, or $9' 31''$. Let us compare the above apparent least distance of the centers measured at Rodrigues with this geocentric least distance of the centers, and we shall find that the parallax of the Sun from thence resulting is $= 8''$, 56 the same as before. These results of the parallax, arising from the comparisons of the apparent least distances of the centers, agreeing with the former determinations of the parallax by the internal contacts, are a proof of the accuracy of this measurement of the greatest distance of the limbs made by M. Pingré at Rodrigues.

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There

There are 12 places at which the total duration was observed, three of these had a northern parallax of latitude at the middle of the transit, the other nine had a southern parallax of latitude; let the apparent least distance of the centers at each place of observation be found, by the following method, let these be compared together, and we shall have the parallax of the Sun resulting from them. For this purpose I have computed the apparent least distance of the centers at the 8 following places, and have compared them with the apparent least distance of the centers at the four following places, and from each comparison I have computed the parallax of the Sun, and they are as in the following table.

| | Cajan. | Calin. | Tobol. | Torne. | Upal. | Stoko. | Abo. | Heno. | O's Par. mean. |
|--------------|--------|--------|--------|--------|-------|--------|-------|-------|-------------------|
| Tranquebar | 8. 48 | 8. 45 | 8. 54 | 8. 31 | 8. 48 | 8. 20 | 8. 52 | 8. 42 | 8. 42 |
| Madras - | 8. 79 | 8. 76 | 8. 93 | 8. 61 | 8. 79 | 8. 50 | 8. 82 | 8. 73 | 8. 74 |
| G. Mount - | 8. 42 | 8. 38 | 8. 45 | 8. 24 | 8. 42 | 8. 12 | 8. 45 | 8. 35 | 8. 35 |
| Calcutta - | 8. 69 | 8. 65 | 8. 81 | 8. 43 | 8. 68 | 8. 35 | 8. 73 | 8. 61 | 8. 62 |
| O's P. mean. | 8. 59 | 8. 56 | 8. 68 | 8. 41 | 8. 59 | 8. 29 | 8. 63 | 8. 53 | 8. 53 |

The mean of these 32 comparisons gives the Sun's parallax = 8", 53. This very near agreement with the former determinations is somewhat surprizing, when we consider the smallness of the base from which they are computed, the greatest scarcely exceeding 20" of an angle; but we are also to consider, that the apparent least distance of the centers may be found, from the duration observed, to a very great exactness, and nothing affects the accuracy of it, but the errors in the observation. Let us suppose then that an error, of 6" in time, happened in each of

of the observations of the ingress and egress, both in contrary directions; the sum of the errors, therefore, in each comparison, will amount to $24''$ of time; this will produce an error of $1''$ of space in the apparent least distance of the centers by computation, but this error of $1''$ cannot produce an error of so much as half a second in the determination of the Sun's parallax. It therefore follows, on the above supposition of an error of $24''$ of time in the observation, that though we had no other observations of the transit of Venus than two of the above total durations, (suppose that of Cajaneburg and Madras) yet we should have been absolutely certain of the parallax of the Sun within less than an error of half a second; and therefore of course it follows, that the mean of so great a number of results must be very near the truth.

This determination of the Sun's parallax, by the least distance of the centers, is also a convincing proof that there is no mistake in the observation of Mr. Mason at the Cape, as alledged by M. Pingré, and that there must be a mistake of $1'$ in setting down the time of the internal contact at the egress at Rodrigues, notwithstanding M. Pingré, in the aforesaid memoir, prefers his observation to that of Mr. Mason, because, as he says, that after a strict examination of all the circumstances attending his observation, *he could not find any mistake in it, but also because he has proved that no mistake could possibly be committed.* In this determination of the parallax by the apparent least distance of the centers, we are not embarrassed with an exact knowledge of the difference of longitude between the places compared, which therefore in

in some measure compensates for the smallness of the base.

The same irrefragable argument, made use of in the apparent least distance of the centers, measured at Rodrigues, to prove that the parallax of the Sun is very nearly $= 8'$, 5, may likewise be deduced from the apparent least distance of the centers, computed from the total durations observed at these 12 places, but with more certainty; because the determination of the apparent least distances of the centers from the observed total durations may be depended on to a very great precision, but the same cannot be said with regard to the apparent least distance of the centers measured at Rodrigues: For M. Pingré tells us that he used a very good micrometer fitted to a refracting telescope of nine feet focus, the object-glass of which was but an indifferent one; and we are very certain, that in measuring, with a micrometer of this sort, dark objects on a white field or ground, if the image is any way indistinct, the angle measured will be less than the true angle, and *vice versa* when a bright object is measured on a dark ground; as a proof of this remark, we find that M. Pingré measured and found the diameter of Venus, when on the Sun, $= 54''$, 7, whereas we are certain that it was above $58''$, and therefore we may presume that the measurements of the greatest distance of the limbs might be greater than the true distance, and as a further proof of the uncertainty of the measurements made with this instrument we find that M. Pingré makes the distance of the limbs greatest, several minutes after it was past the greatest.

I shall

I shall now produce, at one view, the means of the several determinations of the Sun's parallax, by the before-mentioned three several methods, which will contain the substance of this whole paper.

1^o. The mean of 116 comparisons of the internal contacts observed at places to the north of the *Line* only, gives the Sun's parallax - - - - - } = 8. 565

2^o. The mean of 21 comparisons of the internal contacts, with that at the Cape, gives the Sun's parallax - - - - - } = 8. 56

3^o. The mean of 21 comparisons of the internal contacts, with that at Rodriguez, gives the Sun's parallax - - - - - } = 8. 57

4^o. The mean of the comparisons of the total durations gives the Sun's parallax } = 8. 61

5^o. The mean of the apparent least distances of the centers compared with that measured at Rodriguez, gives the Sun's parallax - - - - - } = 8. 56

6^o. The mean of the apparent least distances of the centers by computation from the total durations compared together, gives the Sun's parallax - - - - - } = 8. 53

The mean of these 6 means gives the Sun's parallax - - - - - } = 8. 566

And if we reject the mean arising from the comparisons of the total durations, which is the least certain, the mean of the other 5 means gives the Sun's parallax - - - - - } = 8. 557

Thus.

Thus is the Sun's parallax, on the day of the transit, concluded to be $\approx 8''$, 56, and that from three different modes of comparing together a great number of observations variously combined; the several results so nearly coinciding that to me it seems impossible, that the mean of them all can err $\frac{1}{2}$ of a second, and that probably the error does not exceed $\frac{1}{100}$ part of the whole quantity, as the great Dr. Halley had, many years since, confidently presaged*, and thereupon I cannot but congratulate our age and nation, particularly this Society on being enabled, through the royal munificence, to send fit observers to the Cape of Good Hope, whose position affords the largest base, and consequently the safest foundation for the truth.

P. S. M. Pingré, in his aforesaid memoir, seems to think that there must be some mistake in Mr. Mason's observation at the Cape, because by comparing the observations of Jupiter's satellites made by Mr. Mason at the Cape, with those made by M. Messier at Paris, he finds the difference of longitude between these two places less by $1'$ of time, than between Paris and the observatory of M. de la Caille at the Cape, and therefore imagines that Mr. Mason's observatory was to the west of M. de la Caille's. If M. Pingré had looked into the map of the Cape by M. de la Caille, he would have seen, that, if Mr. Mason's observatory had been $1'$ of time to the west

* Ut junioribus nostris astronomis, quibus forsitan haec obser-
vare, ob minorem ætatem, obtinere potest, viam præmonstrem,
qua immensam solis distantiam, intra quingentesimam sui partem,
rite dimitiri poterint. Ph. Tr. N. cccxlviij. p. 454.

of M. de la Caille's, it must have been in the ocean. I am not at all surprized to see a difference or error of 1' of time in deducing the difference of longitude between Paris and the cape, by comparing Mr. Mason's observations with those of M. Messier; for I find, in the last volume of the Memoirs for 1761, a difference of 1' 5" between M. de la Lande and M. Messier in an immersion of the first satellite of Jupiter, both of these gentlemen observing at Paris, owing I suppose to the different goodness of the telescopes used on this occasion, for M. de la Lande says that he used an 18 foot refracter, the object-glaſs of which was tolerably good, and that M. Messier made use of a very good reflecter of 30 inches. If M. Pingré will take the trouble of looking into the Philosophical Transactions, vol. LII. part I. he will there find observations made at the Cape, and in Surrey-street, London, of the immersions of the first and second satellites of Jupiter with reflecting telescopes, of equal goodness, of two feet focal length, where the difference of determination of the longitude of these two places, does not exceed one second in those of the first satellite, and not above 16" in those of the second satellite. Mr. Mason's observatory at the Cape was about half a mile to the south of M. de la Caille's, and about 10 or 12 yards to the west of the meridian of the same.

M. Pingré also seems to think that the time shewn by Mr. Mason's clock was taken from a false meridian. When M. Pingré shall read the account given by Mr. Mason of his observations at the Cape, which he says in his Memoir he has not seen, I am perswaded he will be fully satisfied, from the many e-

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qual-

qual-altitudes taken by Mr. Mason, that there can be no doubt of the times of his observations being found from a true meridian.

I cannot leave this subject without taking notice of a remarkable expression in the history of the Memoirs of the R. Academy at Paris page 96, for the year 1757. It is there said that the English intended to send an astronomer to North America to observe the transit of Venus (according to the plan laid down by Dr. Halley) before they saw the map of the transit by M. de L'isle, and the authority produced for this assertion, are the English news papers, which, if they had understood the nature of these papers, can be no authority at all. I must therefore, on the best authority, inform the gentlemen, who are the compilers of the history of these memoirs, that the R. Society never once thought of sending an observer to North America, even before they saw the map of the transit by M. de L'isle.

N. B. In this paper I have employed the same elements as in my former paper on this subject, except that in reducing time to space I have made use of $4' 0''$, 03 for the horary motion of Venus in her path.

A method

A method of determining the apparent least distance of the centers of the Sun and Venus from the observation of the total duration of the transit observed any one place, and also the geocentric least distance of the centers.

LET BCPL [Tab. XVII. Fig. 1.] represent the disk of the Sun, LSP the ecliptic, OR the geocentric path of Venus over the Sun, AD the apparent path at any place, to the north of the plane of Venus's orbit, SM the geocentric least distance of the centers, AK the parallax of latitude at the internal contact at the ingress, ND the parallax of latitude at the internal contact at the egress, Ab the parallax of longitude at the ingress, and cD the parallax of longitude at the egress. It is required to find SF, which is a perpendicular let fall from the center of the Sun on the apparent path, and from thence to find SM the geocentric least distance of the centers of the Sun and Venus.

If the parallax of longitude at the ingress retards, and the parallax of longitude at the egress accelerates, the total duration will be shortened by the sum of these two parallaxes of longitude, viz. by Ab and cD, and if we make no allowance for these parallaxes, the apparent path will appear to have been BC, consequently a perpendicular from the center of the Sun on BC will be SE, longer than the perpendicular on the true apparent path by FE. But since it is certain that the parallaxes of longitude do not depress or elevate the planet, and only alter the po-

Y y 2 position

sition of the planet in a direction perpendicular to the axis of the orbit of the planet, therefore the parallaxes of longitude, in time, are, in this case, to be added to the observed time of the total duration; in consequence of which the observed time of total duration, $b c + A b + c D$ are = to the chord described by the planet in its passage over the Sun; and if the semidiameters of the Sun and Venus are known, their difference is known, which is = to the line $A S$; $A F$, from what has been said is also known, therefore $S F$ may be found. But this $S F$ is not the apparent least distance of the centers, for if we compute the parallax of latitude for the apparent middle of the transit, we shall find it greater than $M F$, which $M F$ is only a mean between the parallaxes of latitude at the ingress and egress. Let therefore the difference between $M F$ and the parallax of latitude computed for the middle of the transit be added to $S F$, and the sum will be = to the apparent least distance of the centers nearly; and if from this sum we subtract the parallax of latitude, computed for the middle of the transit, the remainder will be the geocentric least distance of the centers nearly.

A true and more ready method to find the geocentric least distance of the centers, consequently the apparent least distance of the centers at any place, where the total duration has been observed.

Reduce the total duration observed to the center, reduce the central semi-duration, in time, into space; then in the right-angled triangle $S M A$ [Fig. 2.] or $S M \alpha$, we have the two sides $S A$ or $S \alpha$, and $A M$ or

or αM given, therefore the third side SM may be found, therefore SM the geocentric least distance of the centers is found; and if to SM , we add or subtract the parallax of latitude for the apparent middle of the transit, the sum or difference will be the apparent least distance of the centers.

E X A M P L E.

To find the apparent least distance of the centers at Tobolsk.

The total duration observed at Tobolsk was $5^h 48' 53''$ add $9' 3''$ (= to the effect of the parallaxes of longitude and latitude both for the ingress and egress, on the supposition that the Sun's parallax is $= 8''$, 5) to this total duration, the sum $5^h 57' 56''$ is $=$ to the central duration, consequently $2^h 58' 58''$ is $=$ the central semi-duration: reduce this time into space, and it will be found $=$ to $715'', 956 = A M$ or αM , and SA or $S\alpha$ ($=$ difference of the semi-diameters of the Sun and Venus) $= 916''$, therefore SM will be found $= 571'', 37 =$ the geocentric least distance of the centers of the Sun and Venus. The parallax of latitude, computed on the above supposition of the Sun's parallax, for the apparent middle of the transit at Tobolsk, will be found $= 14'', 13$, which being added to the geocentric least distance of the centers above found, the sum $585'', 50$ will be the apparent least distance of the centers at Tobolsk.

*XLVIII. An Account of a Case, in which
Green Hemlock was applied: In a Letter
to the Rt. Hon. Hugh Lord Willoughby of
Parham, V. P. of the R. S. by Mr. Josiah
Colebrook, F. R. S.*

My Lord,

Read Dec. 15, 1763. I take the liberty, from the friend-
ship you are pleased to honour me with, to address the enclosed case to your Lordship, and hope you will think it worth communicating to the Royal Society. It is a bare relation of matters of fact, most of them within my own knowledge, the others attested by persons whose veracity I can depend on. As the hemlock taken in this manner gave great relief to this poor woman, labouring under the most dreadful disease human nature is liable to; it may be attended with the same success to other persons, in the same circumstances.

I am well assured your Lordship rejoices at every opportunity of doing good to mankind, by communicating any beneficial discoveries of your own, or your friends; among whom you will excuse my vanity in placing myself, who am,

with the greatest respect,

your Lordship's

most humble Servant,

J. Colebrook.

A N N

ANN James of the parish of Boughton Monchelsea in Kent, aged 55 years, a married woman, had for some years complained of a pain, and hard lump in each breast. In September 1762 she asked my advice about them : upon examining them I found a very hard schirrus in each breast : that in the left breast, had the mamillary glands indurated and knobbed like ramifications toward the axilla, a little adhesion to the pectoral muscle ; was as big as a turkey's egg, and she was under daily apprehensions, that it would break. That in the right breast was not near so large, or had ramifications nor adhered like the other. She complained of most excruciating stabbing pains in both breasts, which prevented her having any rest in the night, and made her so very miserable all day, whether she lay down, stood, sat, or walked, that she was unable, not only to go out to work, but even to do any thing for her family at home, not even to make her own bed ; and she had totally lost her appetite : her usual employ was spinning, washing, brewing, and what we in London call the busines of a chairwoman. The breasts were but little discoloured, but the pains she described, and the ramifications attending the schirrus, in the left breast, induced me to pronounce it a cancer.

I advised her to take the green hemlock, viz. cicuta major vulgaris caule maculoso ; mince it with parsly (to disguise the taste) and eat it with bread and butter twice or three times in a day, the third part of a leaf, or one of the three divisions, which are in each leaf, at a time ; that her constant drink

should be lime water and milk; that she should take as many millepedes every day, as her stomach would bear, or she could get, that her body should be kept open by Rhubarb, or Magnesia, as occasion required; that she should have an issue in her arm, and lose six or eight ounces of blood once in six or eight weeks, if her pains continued.

A good lady in the neighbourhood, whose humanity is only to be equalled by her good sense, generously promised to see, that she pursued this regimen, and from time to time give me an account of the success.

I desired a leaf might be weighed, that I might ascertain the quantity of each dose, and found she took fifteen grains of the green plant three times in a day: finding it agree with her stomach, and that it eased her pains, though it caused a tingling to her fingers ends: she encreased the quantity. In the beginning of November she had a very large menstrual discharge, which had not happened to her for many years before; the schirrus was much lessened, and her pains were considerably abated.

About the end of November she found her breast more swelled, and the pain more acute than it had been for six weeks before, had a restlessness, giddiness in her head, and weight over her eyes; the discharge of the issue stopped, and a violent humour came all round the orifice. As I had desired a little blood might be taken away, if occasion required it, she was bled about the last day of November, on which she fainted away, and afterwards had fainting fits two or three times in a day, great sickness at her stomach, and sometimes bled at the nose. On these symptoms

symptoms coming on, notwithstanding she had taken somewhat purgative twice in a week, from from her first beginning to take the hemlock, it was thought proper to suspend the taking the hemlock for some days.

I then ordered her an infusion of the cortex Peruvianus an ounce, in powder, to a quart of spring water, to let it stand three or four days, shaking it every day; and then that she should take three spoonfuls, twice in a day; that she should repeat the hemlock in the same quantity she took at the first; that she should not again exceed that quantity on any account; and that she should continue the lime-water and the millepedes.

About the latter end of December she had a regular appearance of her menses, but very moderate; her pains were very much abated, and the schirrus much less, though she often complained of a swimming in her head, and a restlessness in the night. From this time, viz. the end of December, she continued mending in all respects so much, that I heard nothing of her 'till March 1763; when Mrs. Savage (the lady under whose inspection she took the hemlock) came to London, and told me, that Ann James was surprizingly recovered; that her cancer was much lessened, that she could use her arms, work for herself and family, and that her pains were so much abated, that she was quite happy.

In September last I was at Boughton, saw her, and examined her breasts: the schirrus in her left breast was not half so big as when I saw it before; the ramifications were all gone, and it did not at

all adhere to the pectoral muscle ; her appetite was good, and she was able to do her business as usual, and had that day I saw her been brewing : she said she sometimes felt some of those stabbing pains she before complained of, but they were not frequent nor very severe.

The beginning of this November I had a farther account of her from Sir Thomas Ryder, who lives in that neighbourhood, and whom I desired to be so kind as to inform me of her present state of health : he with his usual benevolence (than whom no man hath more) sent for the woman, and had the following account from herself ;

That the lump in her breast, which she expected would break, is not half so big as it was, and continued decreasing ; that she hath great spirits ; and, from being one of the most miserable of the human species, she now enjoys ease and happiness, and can, without any great pain, do all her usual business, as washing, brewing, baking, and needle-work, except spinning, that motion still giving her great pain : she continues to take half a drachm of dry hemlock twice in a day, but takes the green, when she can get it, in larger quantities. Sir Thomas adds, that she looks very well for a woman of her age.

From the happy success of the hemlock in this instance, it were to be wished it might be tried in some other similar case, and that the weight of the plant taken in one day (whether green or dry) might be particularly ascertained, which was too often in this case taken by guess ; and as the extract

tract recommended by Dr. Stork in his ingenious treatise hath not, upon trial in England, been attended with the same success it had at Vienna; I should prefer the herb itself to any preparation of it.

XLIX. An Account of a remarkable Meteor : In a Letter to the Reverend Thomas Birch, D. D. Secret. of R. S. from Mr. Samuel Dunn.

Reverend Sir,

Chelsea, Dec. 9th, 1763.

Read Dec. 15, 1763. IN the Months of September and October last, on many different days, but always in the afternoon, when the Sun was nearly of the same height above the horizon, I was amused with the appearance of a kind of meteor, which I do not know that it hath been before taken notice of by others. As it appeared under nearly the same circumstances at other times, and therefore may contribute towards the better understanding the theory of a parhelion, I shall give the description of this meteor, as it appeared the 6th of October last, at five o'clock afternoon. A kind of mock Sun appeared of equal altitude with the real Sun about $22\frac{1}{2}$ ° southerly therefrom. A little

little above the mock Sun the Sky was clear, but the phænomenon was in the midst of clouds that were not very dense. The diameter of this phænomenon was nearly like that of the real Sun, and a remarkable red stream of light pointed therefrom as at all other times towards the real Sun, which shined clearly at the same time. As there was no descending rain, nor any other colour of the rain-bow, I take this to have been a meteor not yet registered amongst meteorological observations.

I am,

Reverend Sir,

Your most obedient servant,

Samuel Dunn:

L. An

L. *An Account of a Blow upon the Heart; and of its Effects: By Mark Akenside, M. D. F. R. S. and Physician to Her Majesty.*

Read Dec. 22,
1763.

ON the 11th of September, 1762, Richard Bennet, a lad about fourteen years of age, was brought to a consultation of the physicians and surgeons of St. Thomas's Hospital. His disorder was a palpitation of the heart; so very violent to the touch, that we all concluded it to be an aneurysm, and without remedy. He had a frequent cough. His pulse was quick, weak, and uneven; but not properly intermitting. It was apparent that nothing could be done, farther than by letting blood in small quantities, and by the use of emollient pectoral medicines, to lessen now and then, however inconsiderably, the extreme danger to which he was continually subject. He was taken into the hospital that same day, being Saturday; and treated according to what had been agreed upon. But on the Tuesday morning following, he died, without any previous alarm or alteration.

The origin of his complaint was a blow, which he had received six months before, from the master whom he served, as waiter in a public house. The master had owned that he had pushed him slightly on the left side with his hand. The boy informed us that he himself was then carrying a plate under his arm; and that the blow or push, from his master,

ter, drove the edge of the plate forcibly between two of his ribs. He was immediately very ill from the hurt ; sick, and in great pain. His mother also informed us, that she thought the palpitation was more violent about a fortnight after the accident, than when we examined him. The day after the blow, they took eight ounces of blood from his arm : about three weeks after that, they again opened a vein, but got not much from it : and three weeks from thence, they let him bleed the last time, to the amount of eight ounces. He began to have a cough soon after the hurt, with frequent spittings of blood in very large quantities ; and had nocturnal sweats almost the whole six months, during which he survived the blow. About four months after it, there came, over the umbilical region of the abdomen, a livid appearance like a mortification : but it went off gradually, and at length vanished. He had nothing particular in his habit of body or state of health ; save that, about a year before this accident, he had been crippled with the rheumatism. He was, when we saw him, a good deal reduced ; but had not a hectic nor consumptive look.

On the day of his death, Mr. Cowell opened him ; when, to our great surprize, we found no aneurysm, nor the least extravasation of blood either from the cavities of the heart or the large vessels. But on the left ventricle of the heart, near it's apex, there was a livid spot, almost as large as a half-crown piece, bruised and jelly like ; the part underneath being mortified quite to the cavity of the ventricle. From thence upward, toward the auricle,

auricle, there went several livid specks and traces of inflammation, tending in like manner to gangrene. The heart did also, throughout its whole surface, adhere very closely to the pericardium; and the whole outer surface of the pericardium, as closely, to the lungs. The other viscera were quite sound.

So that the mischief here was properly a contusion of the heart; the edge of the plate having struck it, probably at the instant of its greatest diastole. This produced an inflammation on its surface, followed by a gangrene, and terminating in that double adhesion: by which the whole heart was fast tied up; till on this account, as well as by reason of the mortification, it was no longer able to circulate the blood..

LI. Ratio

**LI. Ratio conficiendi Nitrum in Podolia : Au-
thore — Wolf, M. D. communicated by
Mr. Henry Baker, F. R. S.**

Read Dec. 22, 1763. **N**ITRUM, quod in Europa con-

sumitur, longe maxima parte ex India Orientali adfertur : ceterum fere omne ex Ucrainia, tam Polonica, quam Russica, vel adjacentibus provinciis venit. Obtinetur elixivatione ex humo et cineribus. Humus quidem sola est vegetabilis et animalis ; sed præterea etiam opus est, ut diu sit immota, inculta, deserta. Talis in Ucrainia et Podolia est valde frequens. Nam inculta jacet hæc regio quasi a tertio æræ christianaæ seculo, quo Getæ, antiqui possessores, a Bulgaris extrudebantur, quorum posteri pecorum magis quam agrorum, urbiumque culturæ incubuerunt. Maxime vero ob bella superioris seculi, Turcica, Cossica et Tartarica, ab incolis deserta atque relicta est ; nostrâ tamen vitâ, confluentibus colonis ob præcellentem fertilitatem soli, jam satis colitur.

Amplissima hæc planities, quantum videre licuit, tegitur humo nigra vel subruba, ad paucorum pollicum, vel pedis profunditatem, sub qua jacet terra plus minus alba, cretacea, calcaria, margacea, (vel saxum ex his induratum) conchyliis marinis plurimum generum referta, multis in locis adeo copiose, ut tota non videatur, quam iis solis, constare. Argilla et sabulum minus frequenter occurrunt. Ista humus vero est adeo levis, et in aqua adeo solubilis ut

ut a pauca pluvia statim diffundat, atque a levi vento, vel a sole citissime siccescat, et in pulverem nigrum subtilem, viatorum vestimenta, ad cutem usque penetrantem et denigrantem, attollatur.

Indicia terræ, nitro prægnantis, talia habent coloni: si bene nigra, tactu lœvis, non fabulosa, in farinam subtilem friabilis: si firmosa, pinguis: si saporis frigidæ nitrosi: si diu videatur relicta, immota: maxime vero dives æstinnatur si efflorescentiâ nitrosâ, instar lanuginis albæ, obtecta sit: hinc, ubiunque suspicio est, oppidum quondam fuisse, vel pagum, vel stabulum, vel cœmeterium. Præsertim tamen colles appetunt, in his locis valde frequentes, quos Mogily appellant. Horum figura conica arte factos esse facile prodit: de plurimis etiam certo scimus, in memoriam præliorum, ibi editorum, congestos esse; de reliquis vero ob similitudinem idem arbitramur. Ex his unus, ob insignem magnitudinem, Szeroka Mogila, seu magnus collis, dictus, prope Granoviam situs, perantiquus, forte per 100 annos jam nitro conficiendo inservit. Hujus diameter 300 circiter est passuum, et, quantum ex residuo segmento hyperbolico æstimatur, 300 pedes facile altus erat. Fabula narrat, Reginam quandam, accepto nuncio, de rege ab inimicis profligato, cum novo exercitu approperasse, et errore inimiei, proprium maritum in hoc loco oppressisse: an ossa occisorum sub fundo lateant, ulterior effossio decebit.

Pro fabricatione nitri, locum eligunt vicinum illi, ubi terra nitri ferax satis copiosa, ut saltem per æstatem unam operi continuando sufficiat: rationem tamen etiam habent aquæ et ligni, quo nempe commodius atque minori pretio convehi possint. Utensilia huc

pertinentia uno vocabulo appellant Maydan, et consistunt sequentibus.

1. Ahenum æneum magnum, continens dolia 15, seu amphoras 60, quarum quælibet capit congius 6 (gallons) seu libras 54 aquæ.

2. Dolia lignea 100 superius aperta, et prope fundum pertusa foramine, pro lubitu claudendo: capacitas horum est talis, ut contineant terræ carrum unum, quod redit ad amphoras 4 vel quinque.

3. Cadi duo permagni, amphorarum circiter centum.

4. Alvei 32, seu excipula lignea lata, amphoram unam vel paulo plus continentia, quæ cristallisationi inserviunt.

5. Præterea amphoræ aliquot pro apportanda aqua.

Furnus ex terra effoditur, in quo ahenum ope laterum firmatur, in eadem cum horizonte linea. In peripheria aheni adhuc circulum ex asseribus parvis construunt, ad octo circiter pollicum altitudinem, atque luto superinducunt, ne lixivium, forte nimis ebulliens, marginem aheni transcendat et effundatur. Proximo loco ad ahenum ponunt cados illos duos magnos, cetera circumstant. Pro transfundendo lixivio vel aqua, utuntur canali ligneo portabili.

Jam terram nitrosam effosiam ad furnum vehunt, vel, si is propinquus, eam statim in loco effossionis probe comminuunt spatulis ferreis, lapides et similia auferunt, atque in acervos congerunt, ita, ut laxe sibi invicem incumbat. Si hæc terra nitro valde dives, (quod ex pinguitudine et efflorescentia lanuginosa noscunt) admiscent ei aliam minus diyitem, æquali copia, bene tamen nigram, diu relictam: nimirum, termino chymico, terræ animali admiscent pure vegetabilem. Tandem addunt cinerum partem quintam circiter, vel minus, prout experientia docuerit, et similiter bene subigunt. Alii tum demum cineres addunt,

dum

dum terram in dolia immittunt. Cineres sunt ex fraxino utpote communiori arbore. Si urina ad manus, vel matrix nitri superabundans, has etiam adfundunt. Calcem vivam vero, quantum audivi, non addunt. Sic copiam terræ præparant, incipiente æstate, et per totam æstatem similiter continuant, ne sub continua coctione deficiat. Alii terram, quæ æstate sequenti elixivari debet, per antecedentem æstatem convehunt et præparant. Communis tamen praxis est, terram effossam et præparatam, statim in ipso loco effossonis elutriare; quod ita peragitur.

In quodlibet doliorum 100 supra N°. 2. memoratorum, immittunt terræ præparatæ nitroſæ carrum unum, nempe amphoras 4 circiter. Aquam frigidam (alii fervidam) superaffundunt ad repletionem dolii: cineres, si nondum additi, addunt: et baculo bene circumagitant. Sic relinquunt per 24 horas, nisi quod agitatio cum baculo interdum repetatur. Hoc tempore elapso, lixivium sic enatum, per foramen, prope fundum doliorum, emittunt, et in cados duos magnos N°. 3. memoratos, transfundunt. Terram sic elutriatam ex doliis ejiciunt, novam immittunt, et similiter operantur. Ita quotidie fit, quousque nitri coctio durat.

Pro nitri excoctione opus habent matrice nitri, quæ est lixivium spissum, post nitri crystallisationem relicturn, jam ulterius in crystallos non cogendum: quare hoc lixivium sollicite ex anno novissimo in subsequentem servant. Hoc enim deficiente, per octo sæpe dies, sub continua ebullitione, lixivium recens nitrum coqui et inspissari debet, antequam ad crystallisationem idoneum evadat. Cujus phænomeni ratio in eo sita videtur, quod lixivium recens istum caloris

gradum non assumat, qui pro abigendis partibus pinguis et alcalinis volatilibus requiritur, quæ densitas requisita ipsi conciliatur per matricem nitri, copiosam terram calcariam in acido salis et nitri solutam, continentem. Hoc lixivio vero semel obtento, excoctio cunctius perficitur.

Nimirum hujus matrix nitri dolium unum vel alterum in ahenum infundunt, et lixivium recens nitrosum in cadiis magnis collectum addunt, ad repletionem aheni, ignem subdunt, et sub continuâ ebullitione coquunt, fere per 24 horas. Tunc signis crystallisationis in superficie apparentibus, lixivium hoc excoctum, spissum, ex aheno, in alveos illos planos, 32 sub N°. 4. memoratos transfundunt: ibique iterum per 24 horas relinquunt. Sic crystallisatione facta, matrix nitri, ab hac crystallisatione residua, ex aliis decantatur, et in ahenum reaffunditur. Crystalli nitri eximuntur, et exsiccantur, quæ impuriores sunt, et pro depuratione, in aqua pura iterum solvuntur, per lanam filtrantur, in aheno minori inspissantur, et secunda vice crystallisantur in nitrum purius vendibile. Matrix nitri in ahenum reinfusæ adducta similiter novum lixivium recens nitrosum ex cadiis illis duobus magnis, coquunt per 24 horas et crystallisant. Hac ratione opus per totam æstatem continuat: hieme a gelu impeditur.

Productum diei unius dicunt doba, et ad minimum computatur ponderis unius (kamies, sive 14 oko) quod reddit ad libras communes 42. Sub depuratione oko unum vel 3 libras ab hoc quanto decedunt. Pondus unum nitri venditur hodie in loco confectionis rublis 4 (17 shillings). Verum tempore pacis multo vilius.

Quod si carrum unum, seu amphoras 4 terræ nitroſæ præparatæ cum cineribus, laxe cohærentis, sumamus pro pedibus cubicis quatuor; patet, ex 400 pedibus cubicis hujusmodi terræ obtineri libras 40 circiter nitri, adeoque libra una nitri in 10 pedibus cubicis terræ præparatæ, vel in 7 aut 8 pedibus cubicis terræ compactionis effossa hæret, licet hoc adeo exacte computari non possit.

Terram istam, ex qua nitrum dicta ratione extratum, ex doliis ejectam, in aggeres quatuor circiter pedes altos congerunt, et sic relinquunt per annos septem, quo tempore elapsio, maydan in eodem loco collocant, et ex eadem terra, famili opere æqualem fere nitri copiam elutriant. Sed tentia vice post septem alios annos, non quidem omni nitro caret, sed jam operæ pretium non solvit.

Nullus dubito, hunc nitri parandi modum ex orientalioribus regionibus hic pervenisse, et in India atque China non absimili modo fieri. Qua ratione vero in Europa fiat, autores bene multi describunt. Omnes humum et cineres requirunt, alii etiam urinam, alii etiam calcem vivam. Hanc miscelam omnes aëri exponunt, vel libere, vel sub tecto, vel muris ex luto constructis superinducunt, vel in aggeres altos congerunt, vel in fossas profundas laxe conjiciunt. Omnes etiam, quocunque modo hoc fiat, nitrum obtinent: copia tamen valde diversa, quæ, ut facile videatur, non tam ab operosa et sumptuosa expositione, quam ab ipsa pinguedine humi pendet.

Nitrum puritate multum differt. Naturale primæ crystallisationis nunquam caret sale communi. Non semper est prismaticum, sed etiam invenitur cubicum, æque bonum ac illud, si basis alcalina sit mineralis, ex sale

sale communi, vel aliunde. Figura enim semper ab alcali non ab acido pendet, licet Linnæus bonam partem systematis fossilium huic errori superstruxerit. Si multum terræ calcariæ, et non satis cinerum, sub coccione nitri adhibitum fuerit, crystalli erunt minus firmæ, et solutæ per alcali præcipitantur, quod bono nitro non accidit. Si cineres fuerint ex duriori ligno, nitrum erit magis firmum, et in crystallis bene magnis, quale est Indicum. Si in humo adhibita, terra metallica, uti martialis delitescit, semper ejus aliqua pars, saltem tinctura in nitro relinquitur. Sic Indicum est rubellum, et aquam fortē dat multo magis fumis rubris refertum, quam Polonicum. Ex hoc enim cum vitriolo Anglico destillata aqua fortis est viridis, quæ si a mercurio abstrahitur, relinquit præcipitatum flavum, et per cohobationem, album, bonaque pars mercurii in aqua fortī abstracta latet. Præfertur vero nitrum Polonicum a chemicis omni alio, utpote sincerrimum.

Ut plurimum, nitrum ab Anglis, Hollandis, Polonis et Russis, multo minori pretio emi, quam domi fieri potest. Ratio facile patet quod in his regionibus ligna et cineres quasi gratis habeantur, vectura quoque et opera manualia a colonis servis fiant. In regis Borussiæ dominiis forte plus nitri conficitur, quam in omni reliqua Europa, et tamen vix credo, millefimam partem domesticam fuisse ejus, quod in præsenti bello absumtum. Nempe magis necessaria, magisque proficua nobis est terra, nitro et sale volatili prægnans, pro foecundandis agris, atque confiendo pane, quam ut nitrum destructivum inde elixivetur, vel parum utilis sal ammoniacus inde sublimetur. Talia desertis incultisque terris relinquenda sunt.

Cogitationes

Cogitationes quædam circa originem Nitri.

NITRUM commune ex alcali fixo vegetabili et acido nitroso componitur. De origine prioris non disputatur, cum cineres ad nitri confectionem sumantur, neque sine his bonum nitrum in copia fieri possit. Cum tamen etiam sine additis cineribus paucum nitrum ex humo elutriari possit, valde probabile est, in humo adhuc aliquid alcali fixi, per putrefactionem nondum destructi, latere. Vel etiam per coccionem alcali fixum eadem ratione hic generatur, quâ oritur dum Tartarus cum calce viva, vel creta coquitur. Hoc experimento Kunkel, et post illum alii, demonstrarunt alcali fixum vegetabile sine igne genitum. In humo vero, et terra calcaria, et acidum, tartari acido simile, per calcinationem et destillationem demonstratur.

Sed de acido nitri, res multa difficultate laborat. Omnes chémici hoc acidum ex aëre derivant, ibique genitum dicunt ex acido universalis vitriolico, atque inde per partes humi alcalinas attrahi. Ne dicam: acidi vitriolici universalitatem per omnem atmosphærā, precastio assumi; et nitrum in omni humo generari, licet in tali loco, ubi longe lateque de minera vitriolica nihil videtur: item, in sale alcalico fixo, puro, sincero, per annos in aëre relicto, repetitis experimentis, vix micam salis medii, multo minus vitriolici, observari, modo hoc non fiat in laboratorio, vel alibi, in vicinâ vaporum acidorum. Sed pulcræ Margræfii destillationes aquæ pluvialis et nivalis lucem huic rei affundunt: obtinuit nempe ex libris 225 harum aquarum lentissime inspissatarum, per additionem salis tartari

tartari puri, pauca grana nitri et salis communis, quæ quantitas inassignabilis minor certe erat scrupulo uno: adeoque in illa pluviae quantitate, quæ fere est pedum cubicorum $3\frac{1}{2}$ vix tantum acidi nitrosi continetur, ac in scrupulo uno nitri. Jam observationes meteoricæ, docent, omnem aquam per annum unum de celo, delabentem raro ad duorum pedum altitudinem ascendere. Duxi vero in descriptione confectionis nitri podolici, ex pedibus cubicis 10 terræ nitrosæ præparatæ, ad minimum libram unam nitri elixivari, atque hanc terram semel elutriatam in aggeres congestam, post septem annos, simile nitri quantum largiri. Ponamus, 10 pedes cubicos hujus terræ, contingere aërem in superficie 10 pedum quadratorum, et omne humidum, in hanc superficiem delabens, acidum suum omne nitrosum hic figere, nihil vero nec in auras iterum ascendere, nec per aquas defluentes abripi. Cadunt vero in hanc superficiem per 7 annos, aquæ cœlestis pedes cubici 140 quæ per Margrarium destillata, daret, cum sale tartari, scrupulos 40 nitri, quod longe abest a libra una. Cum vero rationi magis sit consonum, ex aëre non plus nec minus in humum descendere, quam ex humo in aërem ascenderat: attractio etiam acidi per alcali valde sit precaria, cum exinde sequeretur, montes calcarios et cretaceos, ab omni humo denudatos, hoc acido tandem saturari debere, saltem nitro abundare, quod omnino falsum; patet, hanc chemicorum hypothesin stare non posse.

Verum ex omni humo plus minus nitri elixivatur; ex ceteris terris nullum, nisi humo permixtae sint: omnes qui nitrum conficiunt, humum adhibent, neque experimentum scitur, ubi sine humo fieri possit, atque omne nitrum non nisi in superficie terræ ad parvam

parvam profunditatem invenitur, ubi nempe humus est; cum ergo humus non sit, nisi vegetabilia et animalia per putrefactionem destruncta, vix dubitare licet, acidum nitri ex regno vegetabili et animali originem ducere, et quidem per destructionem horum componi, cum in recentibus non inveniatur. Salia enim essentialia, nitrosa dicta, Borraginis, Portulaccæ, Parietariæ, Millepedum, Lumbricorum terrestrium, etc. etc. non nisi per similitudinem quandam sic dicuntur. Ex fæcibus humanis elixivatis, quidem, nitrum obtinuit Hombergius, sed fæces jam ad humum pertinent.

Vegetabilia et animalia recentia, destillatione, dant spiritum plus minus acidum oleosum factentem, ad spiritum tartari accendentem, cum oleo foetido; priora quidem plus acidi; posteriora plus olei: ex carbone vero residuo utroque paucum sal commune elixivari, atque post ulteriore calcinationem etiam alcali fixum elutriari potest, relicta tandem terra calcaria: et quidem vegetabilia plus largiuntur alcali fixi, animalia vero plus salis communis, et plus terræ calcariæ. Humus contra vegetabilis et animalis largiuntur destillatione similes spiritus acidos, similique oleum prioribus, sed longe minori quantitate: præterea vero alcali volatile, quod in recentibus non aderat; et quidem ex vegetabili plus acidi, ex animali vero plus alcali volatile: residuus carbo utriusque, præter sal commune, etiam nitrum, elixivatione præbet, quod in recentibus non aderat; atque post ulteriore calcinationem, alcali fixi nihil suppeditat, quod tamen in recentibus aderat; superstite tandem, ut prius, terra calcaria. Vegetabilis tamen humus plus nitri, animalis vero plus salis communis, continet. Omnis ergo mutatio,

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quæ

quæ vegetabilibus et animalibus per putrefactionem accidit, videtur consistere, in diminutione acidi et olei, in destructione alcali fixi, et in generatione alcali volatilis et acidi nitrofi. Idem fere efficiunt chymici, qui norunt, omne alcali fixum ab addito pauco oleo et acido, repetita destillatione, in alcali volatile mutari. Acidum vero nitri hac ratione ars chemica nondum produxit, licet, ut infra dicetur, ex combinatione acidi salis cum acido vegetabili vel animali, et parte alcali volatilis, omnino similimum quid obtineatur.

Videretur alcali volatile ad nitri confectionem parum conferre, cum, sub coctione, omne in auras dispellatur; tamen, sine hoc, nitrum vel nullum vel paucissimum obtinetur. Hinc nitri coccatores urinam valde expetunt, et humum animalem, divitiorem alcali volatili, solcite conquirunt, talemque præferunt, quæ diu immota jacuit, cum in sæpius mota, hujus alcali volatilis multum per aërem et per pluviam abripiatur. Ob hanc rationem etiam calcem vivam, vel aliam terram calcariam humo admiscent. Hæc enim putrefactionem, et obinde alcali volatilis generationem valde accelerant, ut constat ex pulcris celeb. Pringlii circa septica experimentis, et ex destillatione quorumcumque animalium vel vegetabilium cum calce viva. Ex quibus etiam vera ratio fœcundationis agrorum per terras calcarias patet, ut ad vanam attractionem acidi nitrofi ex aëre non opus sit recurrere. Obinde etiam humum præparatam aëri exponunt, qui putrefactionem similiter promovet. Lanugo alba, tempore nocturno, terram nitrosam obducens, nitrum sapit, et per microscopium crystallos nitri

nitri ostendit, sed a sole oriente cito dissipatur: ut profecto vix dubium relinquatur esse hanc lanuginem nitrum volatile, ex alcali volatili et nitri acido constans, quod acidum sub coctione necessario cum alcali suo volatili in auras dispelleretur, nisi ab additis cineribus vel etiam calce viva retineretur. Videtur ergo acidum nitri in origine sua cum alcali volatili coniunctum esse, et verosimiliter inde etiam phlogiston suum specificum, in detonationem adeo primum habet. Nam aurum ex aqua regis per alcali praecipitatum, non fulminat, nisi alcali volatile vel in confectione aquæ regis, vel in praecipitatione adhibitum fuerit.

Artificialem acidi nitroſi compositionem chymici ſæpius tentarunt, de qua re ſequentia proferre licet. Multi acidum vitriolicum mutari dicunt in nitroſum per additum phlogiston: ſed ſpiritus vitrioli ſulfureus Stahlii, ex vitriolo per retortam fracturalam destillatus, non est ſpiritus nitri: neque ille, qui ex oleo vitrioli per retortam tubulatam, injectis ſenſim carbonibus candentibus, destillatur: neque ille, qui ex oleo vitrioli glaciali leni igne destillatur: neque ille, qui ex arcano duplicato per additum alumem uſtum vel fabulum destillatur: licet multo ſint volatiliores ipſo nitri ſpiritu.

Alii acidum vitriolicum cum alcali volatili combinant, et obtinent ſalem ammoniacum ſecretum Glauberi, cum ſpiritu ſulfureo, qui non eſt nitri. Si fal tartari extemporaneum bene calcinatum in duplo ſpiritus urinæ ſolvatur, et cum parte una et dimidia vitrioli Salisburgensis calcinati miſceatur, et destilletur; reſiduum vero in aqua ſolutum a terra metallica filtretur, evaporetur et in ſpiritu urinæ

næ iterum solvatur, obtinentur sub lenta inspissatione crystalli nitroſæ, quæ ſal commune ſapiunt, metallæ omnia volatilia reddunt, et fusione in occluſo ſolvunt: minime vero nitrum conſtituent. Pietschius ex ſpiritu vitrioli, urina putrefacta et calce viva, verum nitrum produxifſe dicitur, quod tamen a vero, bafi ſaltem alcalina vegetabili, omnino differre debet.

Alii acidum vitriolicum combinant cum acido vegetabili vel animali. Sed oleum vitrioli cum tartaro deſtillatum, dat ſpiritum tartari ſulfureum, nitri nihil: neque ex ſpiritu theriacali et ſpiritu tartari, cum ſpiritu vitrioli et alcali fixo mixtis et deſtillatis: neque ex ſpiritu cornu cervi et tinctura antimonii acri, verum nitrum obtinetur, licet ſimile quid.

Sal commune totum quantum in nitrum mutantum multi fruſtra gloriantur. Alii magni nominis, inter quos Pottius, volunt: ſpiritum ſalis purum per phlogiston purum, in ſpiritum nitri mutari. Sed ſpiritus ſalis volatilis per retortam tubulatam injectione ſuccesſiva carbonum cendantium deſtillatus, non eſt talis: neque ille, qui ex ſale communi pulvere carbonum (vel fuligine) atque fabulo (vel alumine uſto) mixtis, ignitis, tandem per additum oleum vitrioli deſtillatur. Stahlius vult, acidum ſalis purum ſola ſolutione ferri in acidum nitri verum mutari: ſed repetitum experimentum forte non ſemper ſuccedit. Obtinetur quidem ſpiritus cum vaporibus rubris, ſed hi non ſemper nitri præſentiam arguunt: alię plurimæ aquæ gra-datoriæ ex ferro, auro, zinco, partim etiam cupro, paratæ, omniaque menstrua, mercurium rubro co-lore ſublimantia præcipitantia huc pertinerent: qualis

qualis, exempli gratia, ex solutione ferri in spiritu salis fumante (ex sale ammoniaco et oleo vitrioli facto) cum octuplo butyri antimonii martialis destillatur: vel etiam si solutiones metallorum rubrorum cum addito sale ammoniaco secreto destillantur. Licet enim spiritus nitri concentratus fumis rubris ut plurimum videatur, tamen hoc ita proprium ei non est ut abesse non possit. Nam, si talis spiritus abstrahatur moderatiori igne, vel a nitro crudo, vel ab arsenico, vel mercurio, vel alio quocunque metallo, præsertim albo; tincturam hanc suam rubram, licet volatilissimam sui partem, in abstracto corpore relinquit, et subvividis, licet debilior, tamen sincerus spiritus nitri transit. Aurum fugax, quod hac ratione abstractus in argento vel alio metallo relinquit, naturam metallicam horum fumarum, bene demonstrat. Neque chemici aliud quidpiam in via humida querunt, quam ut hanc tincturam rubram ex metallis imperfectioribus ope menstruorum extrahant, et in aurum figant.

Propius ad verum accedunt, qui acidum salis cum vegetibili combinant. Nullibi enim nitrum generatur, ubi non insimul sal commune occurrat. Sic solutiones vitrioli cyprini et salis ammoniaci fixi, confusæ, a præcipitato filtratae, inspissatae ad siccitatem, tunc cum aceto concentrato solutæ, iterum inspissatae, tandem destillatae, dant spiritum fumantem omni fere nota, nitrosum; simile quid obtinetur, si scoriæ reguli martialis chalybeati, fortiter reverberatae, in aceto destillato saepius alternatim solvantur et inspissentur, tandem cum sale ammoniaco fixo et vitriolo calcinato destillentur.

Facile vero videtur, non fumos rubros, non figuram prismaticam, non detonationem cum inflammabilibus, non solutiones metallorum specificas unumquodque

quodque solum, certa nitri signa præbere, sed plura concurrere debere, ut de vero nitro producto dubium non relinquatur.

LII. *An Essay towards solving a Problem in the Doctrine of Chances. By the late Rev. Mr. Bayes, F. R. S. communicated by Mr. Price, in a Letter to John Canton, A. M. F. R. S.*

Dear Sir,

Read Dec. 23, 1763. I Now send you an essay which I have found among the papers of our deceased friend Mr. Bayes, and which, in my opinion, has great merit, and well deserves to be preserved. Experimental philosophy, you will find, is nearly interested in the subject of it; and on this account there seems to be particular reason for thinking that a communication of it to the Royal Society cannot be improper.

He had, you know, the honour of being a member of that illustrious Society, and was much esteemed by many in it as a very able mathematician. In an introduction which he has writ to this Essay, he says, that his design at first in thinking on the subject of it was, to find out a method by which we might judge concerning the probability that an event has to happen, in given circumstances, upon supposition that we know nothing concerning it but that, under the same circum-

circumstances, it has happened a certain number of times, and failed a certain other number of times. He adds, that he soon perceived that it would not be very difficult to do this, provided some rule could be found according to which we ought to estimate the chance that the probability for the happening of an event perfectly unknown, should lie between any two named degrees of probability, antecedently to any experiments made about it ; and that it appeared to him that the rule must be to suppose the chance the same that it should lie between any two equidifferent degrees ; which, if it were allowed, all the rest might be easily calculated in the common method of proceeding in the doctrine of chances. Accordingly, I find among his papers a very ingenious solution of this problem in this way. But he afterwards considered, that the *postulate* on which he had argued might not perhaps be looked upon by all as reasonable ; and therefore he chose to lay down in another form the proposition in which he thought the solution of the problem is contained, and in a *scholium* to subjoin the reasons why he thought so, rather than to take into his mathematical reasoning any thing that might admit dispute. This, you will observe, is the method which he has pursued in this essay.

Every judicious person will be sensible that the problem now mentioned is by no means merely a curious speculation in the doctrine of chances, but necessary to be solved in order to a sure foundation for all our reasonings concerning past facts, and what is likely to be hereafter. Common sense is indeed sufficient to shew us that, from the observation of what has in former instances been the consequence of a certain

cause or action, one may make a judgment what is likely to be the consequence of it another time, and that the larger number of experiments we have to support a conclusion, so much the more reason we have to take it for granted. But it is certain that we cannot determine, at least not to any nicety, in what degree repeated experiments confirm a conclusion, without the particular discussion of the beforementioned problem; which, therefore, is necessary to be considered by any one who would give a clear account of the strength of *analogical* or *inductive reasoning*; concerning, which at present, we seem to know little more than that it does sometimes in fact convince us, and at other times not; and that, as it is the means of acquainting us with many truths, of which otherwise we must have been ignorant; so it is, in all probability, the source of many errors, which perhaps might in some measure be avoided, if the force that this sort of reasoning ought to have with us were more distinctly and clearly understood.

These observations prove that the problem enquired after in this essay is no less important than it is curious. It may be safely added, I fancy, that it is also a problem that has never before been solved. Mr. De Moivre, indeed, the great improver of this part of mathematics, has in his *Laws of chance**, after Bernoulli, and to a greater degree of exactness, given rules to find the probability there is, that if a very great number of trials be made concerning any event,

* See Mr. De Moivre's *Doctrine of Chances*, p. 243, &c. He has omitted the demonstrations of his rules, but these have been since supplied by Mr. Simpson at the conclusion of his treatise on *The Nature and Laws of Chance*.

the

the proportion of the number of times it will happen, to the number of times it will fail in those trials, should differ less than by small assigned limits from the proportion of the probability of its happening to the probability of its failing in one single trial. But I know of no person who has shewn how to deduce the solution of the converse problem to this ; namely, " the number of times an unknown event " has happened and failed being given, to find the " chance that the probability of its happening should " lie somewhere between any two named degrees of " probability." What Mr. De Moivre has done therefore cannot be thought sufficient to make the consideration of this point unnecessary : especially, as the rules he has given are not pretended to be rigorously exact, except on supposition that the number of trials made are infinite ; from whence it is not obvious how large the number of trials must be in order to make them exact enough to be depended on in practice.

Mr. De Moivre calls the problem he has thus solved, the hardest that can be proposed on the subject of chance. His solution he has applied to a very important purpose, and thereby shewn that those a remuch mistaken who have insinuated that the Doctrine of Chances in mathematics is of trivial consequence, and cannot have a place in any serious enquiry *. The purpose I mean is, to shew what reason we have for believing that there are in the constitution of things fixt laws according to which events happen, and that, therefore, the frame of the world must be

* See his *Doctrine of Chances*, p. 252, &c.

the effect of the wisdom and power of an intelligent cause ; and thus to confirm the argument taken from final causes for the existence of the Deity. It will be easy to see that the converse problem solved in this essay is more directly applicable to this purpose ; for it shews us, with distinctness and precision, in every case of any particular order or recurrency of events, what reason there is to think that such recurrency or order is derived from stable causes or regulations innature, and not from any of the irregularities of chance.

The two last rules in this essay are given without the deductions of them. I have chosen to do this because these deductions, taking up a good deal of room, would swell the essay too much ; and also because these rules, though of considerable use, do not answer the purpose for which they are given as perfectly as could be wished. They are however ready to be produced, if a communication of them should be thought proper. I have in some places writ short notes, and to the whole I have added an application of the rules in the essay to some particular cases, in order to convey a clearer idea of the nature of the problem, and to shew how far the solution of it has been carried.

I am sensible that your time is so much taken up that I cannot reasonably expect that you should minutely examine every part of what I now send you. Some of the calculations, particularly in the Appendix, no one can make without a good deal of labour. I have taken so much care about them, that I believe there can be no material error in any of them ; but should there be any such errors, I am the only person who ought to be considered as answerable for them.

Mr.

Mr. Bayes has thought fit to begin his work with a brief demonstration of the general laws of chance. His reason for doing this, as he says in his introduction, was not merely that his reader might not have the trouble of searching elsewhere for the principles on which he has argued, but because he did not know whither to refer him for a clear demonstration of them. He has also made an apology for the peculiar definition he has given of the word *chance* or *probability*. His design herein was to cut off all dispute about the meaning of the word, which in common language is used in different senses by persons of different opinions, and according as it is applied to *past* or *future* facts. But whatever different senses it may have, all (he observes) will allow that an expectation depending on the truth of any *past* fact, or the happening of any *future* event, ought to be estimated so much the more valuable as the fact is more likely to be true, or the event more likely to happen. Instead therefore, of the proper sense of the word *probability*, he has given that which all will allow to be its proper measure in every case where the word is used. But it is time to conclude this letter. Experimental philosophy is indebted to you for several discoveries and improvements; and, therefore, I cannot help thinking that there is a peculiar propriety in directing to you the following essay and appendix. That your enquiries may be rewarded with many further successes, and that you may enjoy every every valuable blessing, is the sincere wish of, Sir,

Newington-Green,
Nov. 10, 1763.

your very humble servant,

Richard Price.

C c c 2

S E C-

P R O B L E M.

Given the number of times in which an unknown event has happened and failed: *Required* the chance that the probability of its happening in a single trial lies somewhere between any two degrees of probability that can be named.

S E C T I O N I.

DEFINITION 1. Several events are *inconsistent*, when if one of them happens, none of the rest can.

2. Two events are *contrary* when one, or other of them must; and both together cannot happen.
3. An event is said to *fail*, when it cannot happen; or, which comes to the same thing, when its contrary has happened.
4. An event is said to be determined when it has either happened or failed.
5. The *probability* of any event is the ratio between the value at which an expectation depending on the happening of the event ought to be computed, and the value of the thing expected upon it's happening.
6. By *chance* I mean the same as probability.
7. Events are independent when the happening of any one of them does neither increase nor abate the probability of the rest.

P R O P. I.

When several events are inconsistent the probability of the happening of one or other of them is the sum of the probabilities of each of them.

Suppose

Suppose there be three such events, and which ever of them happens I am to receive N, and that the probability of the 1st, 2d, and 3d are respectively $\frac{a}{N}$, $\frac{b}{N}$, $\frac{c}{N}$. Then (by the definition of probability) the value of my expectation from the 1st will be a , from the 2d b , and from the 3d c . Wherefore the value of my expectations from all three will be $a + b + c$. But the sum of my expectations from all three is in this case an expectation of receiving N upon the happening of one or other of them. Wherefore (by definition 5) the probability of one or other of them is $\frac{a+b+c}{N}$ or $\frac{a}{N} + \frac{b}{N} + \frac{c}{N}$. The sum of the probabilities of each of them.

Corollary. If it be certain that one or other of the three events must happen, then $a + b + c = N$. For in this case all the expectations together amounting to a certain expectation of receiving N, their values together must be equal to N. And from hence it is plain that the probability of an event added to the probability of its failure (or of its contrary) is the ratio of equality. For these are two inconsistent events, one of which necessarily happens. Wherefore if the probability of an event is $\frac{P}{N}$ that of its failure will be $\frac{N-P}{N}$.

P R O P. 2.

If a person has an expectation depending on the happening of an event, the probability of the event is to the probability of its failure as his loss if it fails to his gain if it happens.

Suppose a person has an expectation of receiving N, depending on an event the probability of which is

is $\frac{P}{N}$. Then (by definition 5) the value of his expectation is P , and therefore if the event fail, he loses that which in value is P ; and if it happens he receives N , but his expectation ceases. His gain therefore is $N-P$. Likewise since the probability of the event is $\frac{P}{N}$, that of its failure (by corollary prop. 1) is $\frac{N-P}{N}$. But $\frac{P}{N}$ is to $\frac{N-P}{N}$ as P is to $N-P$, i. e. the probability of the event is to the probability of its failure, as his loss if it fails to his gain if it happens.

P R O P. 3.

The probability that two subsequent events will both happen is a ratio compounded of the probability of the 1st, and the probability of the 2d on supposition the 1st happens.

Suppose that, if both events happen, I am to receive N , that the probability both will happen is $\frac{P}{N}$, that the 1st will is $\frac{a}{N}$ (and consequently that the 1st will not is $\frac{N-a}{N}$) and that the 2d will happen upon supposition the 1st does is $\frac{b}{N}$. Then (by definition 5) P will be the value of my expectation, which will become b if the 1st happens. Consequently if the 1st happens, my gain by it is $b-P$, and if it fails my loss is P . Wherefore, by the foregoing proposition, $\frac{a}{N}$ is to $\frac{N-a}{N}$, i. e. a is to $N-a$ as P is to $b-P$. Wherefore (componendo inversè) a is to N as P is to b . But the ratio of P to N is compounded of the ratio of P to b , and that of b to N . Wherefore the same

same ratio of P to N is compounded of the ratio of a to N and that of b to N , i. e. the probability that the two subsequent events will both happen is compounded of the probability of the 1st and the probability of the 2d on supposition the 1st happens.

Corollary. Hence if of two subsequent events the probability of the 1st be $\frac{a}{N}$, and the probability of both together be $\frac{P}{N}$, then the probability of the 2d on supposition the 1st happens is $\frac{P}{a}$.

P R O P. 4.

If there be two subsequent events to be determined every day, and each day the probability of the 2d is $\frac{b}{N}$ and the probability of both $\frac{P}{N}$, and I am to receive N if both the events happen the 1st day on which the 2d does; I say, according to these conditions, the probability of my obtaining N is $\frac{P}{b}$. For if not, let the probability of my obtaining N be $\frac{x}{N}$ and let y be to x as $N-b$ to N . Then since $\frac{x}{N}$ is the probability of my obtaining N (by definition 1) x is the value of my expectation. And again, because according to the foregoing conditions the 1st day I have an expectation of obtaining N depending on the happening of both the events together, the probability of which is $\frac{P}{N}$, the value of this expectation is P . Likewise, if this coincident should not happen I have an expectation of being reinstated in my former circumstances, i. e. of receiving that which in value is x depending

pending on the failure of the 2d event the probability of which (by cor. prop. 1) is $\frac{N-b}{N}$ or $\frac{y}{x}$, because y is to x as $N-b$ to N . Wherefore since x is the thing expected and $\frac{y}{x}$ the probability of obtaining it, the value of this expectation is y . But these two last expectations together are evidently the same with my original expectation, the value of which is x , and therefore $P+y=x$. But y is to x as $N-b$ is to N . Wherefore x is to P as N is to b , and $\frac{x}{N}$ (the probability of my obtaining N) is $\frac{P}{b}$.

Cor. Suppose after the expectation given me in the foregoing proposition, and before it is at all known whether the 1st event has happened or not, I should find that the 2d event has happened; from hence I can only infer that the event is determined on which my expectation depended, and have no reason to esteem the value of my expectation either greater or less than it was before. For if I have reason to think it less, it would be reasonable for me to give something to be reinstated in my former circumstances, and this over and over again as often as I should be informed that the 2d event had happened, which is evidently absurd. And the like absurdity plainly follows if you say I ought to set a greater value on my expectation than before, for then it would be reasonable for me to refuse something if offered me upon condition I would relinquish it, and be reinstated in my former circumstances; and this likewise over and over again as often as (nothing being known concerning the 1st event) it should appear that the 2d had happened. Notwithstanding therefore this discovery that the 2d event

event has happened, my expectation ought to be esteemed the same in value as before, i. e. x , and consequently the probability of my obtaining N is (by definition 5) still $\frac{x}{N}$ or $\frac{P}{b}$ *. But after this discovery the probability of my obtaining N is the probability that the 1st of two subsequent events has happened upon the supposition that the 2d has, whose probabilities were as before specified. But the probability that an event has happened is the same as the probability I have to guess right if I guess it has happened. Wherefore the following proposition is evident.

P R O P. 5.

If there be two subsequent events, the probability of the 2d $\frac{b}{N}$ and the probability of both together $\frac{P}{N}$, and it being 1st discovered that the 2d event has happened, from hence I guess that the 1st event has also happened, the probability I am in the right is $\frac{P}{b}$.[†]

P R O P.

* What is here said may perhaps be a little illustrated by considering that all that can be lost by the happening of the 2d event is the chance I should have had of being reinstated in my former circumstances, if the event on which my expectation depended had been determined in the manner expressed in the proposition. But this chance is always as much *against* me as it is *for* me. If the 1st event happens, it is *against* me, and equal to the chance for the 2d event's failing. If the 1st event does not happen, it is *for* me, and equal also to the chance for the 2d event's failing. The loss of it, therefore, can be no disadvantage.

† What is proved by Mr. Bayes in this and the preceding proposition is the same with the answer to the following question. What is the probability that a certain event, when it happens, will

P R O P. 6.

The probability that several independent events shall all happen is a ratio compounded of the probabilities of each.

For from the nature of independent events, the probability that any one happens is not altered by the happening or failing of any of the rest, and consequently the probability that the 2d event happens on supposition the 1st does is the same with its original probability ; but the probability that any two events happen is a ratio compounded of the probability of the 1st event, and the probability of the 2d on supposition the 1st happens by prop. 3. Wherefore the probability that any two independent events both happen is a ratio compounded of the probability of the 1st and the probability of the 2d. And in like manner considering the 1st and 2d event together as one event ; the probability that three independent events all happen is a ratio compounded of the probability that the two 1st both happen and the probability of the 3d. And thus you

be accompanied with another to be determined at the same time ? In this case, as one of the events is given, nothing can be due for the expectation of it ; and, consequently, the value of an expectation depending on the happening of both events must be the same with the value of an expectation depending on the happening of one of them. In other words ; the probability that, when one of two events happens, the other will, is the same with the probability of this other. Call x then the probability of this other, and if $\frac{b}{N}$ be the probability of the given event, and $\frac{p}{N}$ the probability of both, because $\frac{p}{N} = \frac{b}{N} \times x$, $x = \frac{p}{b} =$ the probability mentioned in these propositions.

may

may proceed if there be ever so many such events; from whence the proposition is manifest.

Cor. 1. If there be several independent events, the probability that the 1st happens the 2d fails, the 3d fails and the 4th happens, &c. is a ratio compounded of the probability of the 1st, and the probability of the failure of the 2d, and the probability of the failure of the 3d, and the probability of the 4th, &c. For the failure of an event may always be considered as the happening of its contrary.

Cor. 2. If there be several independent events, and the probability of each one be a , and that of its failing be b , the probability that the 1st happens and the 2d fails, and the 3d fails and the 4th happens, &c. will be $abba$, &c. For, according to the algebraic way of notation, if a denote any ratio and b another, $abba$ denotes the ratio compounded of the ratios a, b, b, a . This corollary therefore is only a particular case of the foregoing.

Definition. If in consequence of certain data there arises a probability that a certain event should happen, its happening or failing, in consequence of these data, I call it's happening or failing in the 1st trial. And if the same data be again repeated, the happening or failing of the event in consequence of them I call its happening or failing in the 2d trial; and so on as often as the same data are repeated. And hence it is manifest that the happening or failing of the same event in so many different trials, is in reality the happening or failing of so many distinct independent events exactly similar to each other.

P R O P. 7.

If the probability of an event be a , and that of its failure be b in each single trial, the probability of its happening p times, and failing q times in $p+q$ trials is $E a^p b^q$ if E be the coefficient of the term in which occurs $a^p b^q$ when the binomial $\underline{a+b}^{p+q}$ is expanded.

For the happening or failing of an event in different trials are so many independent events. Wherefore (by cor. 2. prop. 6.) the probability that the event happens the 1st trial, fails the 2d and 3d, and happens the 4th, fails the 5th, &c. (thus happening and failing till the number of times it happens be p and the number it fails be q) is $abbab$ &c. till the number of a 's be p and the number of b 's be q , that is; 'tis $a^p b^q$. In like manner if you consider the event as happening p times and failing q times in any other particular order, the probability for it is $a^p b^q$; but the number of different orders according to which an event may happen or fail, so as in all to happen p times and fail q , in $p+q$ trials is equal to the number of permutations that $aaaa\ bbbb$ admit of when the number of a 's is p , and the number of b 's is q . And this number is equal to E , the coefficient of the term in which occurs $a^p b^q$ when $\underline{a+b}^{p+q}$ is expanded. The event therefore may happen p times and fail q in $p+q$ trials E different ways and no more, and its happening and failing these several different ways are so many inconsistent events, the probability for each of which is $a^p b^q$, and therefore by prop.

prop. 1. the probability that some way or other it happens p times and fails q times in $p+q$ trials is $E a^p b^q$.

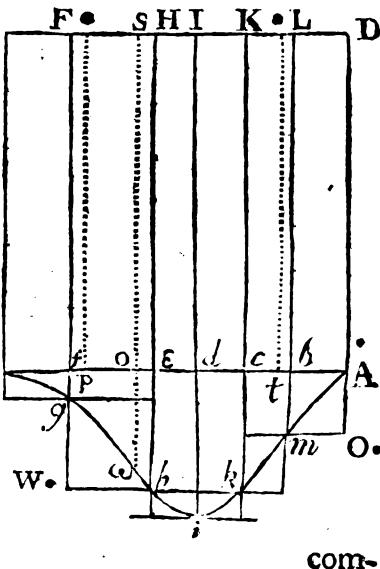
SECTION II.

Postulate. 1. I Suppose the square table or plane ABCD to be so made and levelled, that if either of the balls o or W be thrown upon it, there shall be the same probability that it rests upon any one equal part of the plane as another, and that it must necessarily rest somewhere upon it.

2. I suppose that the ball W shall be 1st thrown, and through the point where it rests a line os shall be drawn parallel to AD, and meeting CD and AB in s and o ; and that afterwards the ball O shall be thrown $p+q$ or n times, and that its resting between AD and os after a single throw be called the happening of the event M in a single trial. These things supposed,

Lem. 1. The probability that the point o will fall between any two points in the line AB is the ratio of the distance between the two points to the whole line AB.

Let any two points be named, as f and b in the line AB, and through them parallel to AD draw fF , bL meeting CD in F and L. Then if the rectangles Cf, Fb, LA are



commensurable to each other, they may each be divided into the same equal parts, which being done, and the ball W thrown, the probability it will rest somewhere upon any number of these equal parts will be the sum of the probabilities it has to rest upon each one of them, because its resting upon any different parts of the plane A C are so many inconsistent events ; and this sum, because the probability it should rest upon any one equal part as another is the same, is the probability it should rest upon any one equal part multiplied by the number of parts. Consequently, the probability there is that the ball W should rest somewhere upon F_b is the probability it has to rest upon one equal part multiplied by the number of equal parts in F_b ; and the probability it rests somewhere upon C_f or L_A , i.e. that it dont rest upon F_b (because it must rest somewhere upon A C) is the probability it rests upon one equal part multiplied by the number of equal parts in C_f, L_A taken together. Wherefore, the probability it rests upon F_b is to the probability it dont as the number of equal parts in F_b is to the number of equal parts in C_f, L_A together, or as F_b to C_f, L_A together, or as f_b to B_f, A_b together. Wherefore the probability it rest upon F_b is to the probability it dont as f_b to B_f, A_b together. And (*componendo inverse*) the probability it rests upon F_b is to the probability it rests upon F_b added to the probability it dont, as f_b to A_B , or as the ratio of f_b to A_B to the ratio of A_B to A_B . But the probability of any event added to the probability of its failure is the ratio of equality ; wherefore, the probability it rest upon F_b is to the ratio of equality as the ratio of f_b to A_B to the ratio of A_B to A_B , or the ratio of equality ; and therefore the probability it rest upon F_b

Fb is the ratio of fb to AB . But *ex hypothesi* according as the ball W falls upon Fb or not the point o will lie between f and b or not, and therefore the probability the point o will lie between f and b is the ratio of fb to AB .

Again; if the rectangles Cf , Fb , LA are not commensurable, yet the last mentioned probability can be neither greater nor less than the ratio of fb to AB ; for, if it be less, let it be the ratio of fc to AB , and upon the line fb take the points p and t , so that pt shall be greater than fc , and the three lines Bp , pt , tA commensurable (which it is evident may be always done by dividing AB into equal parts less than half cb , and taking p and t the nearest points of division to f and c that lie upon fb). Then because Bp , pt , tA are commensurable, so are the rectangles Cp , Dt , and that upon pt completing the square AB . Wherefore, by what has been said, the probability that the point o will lie between p and t is the ratio of pt to AB . But if it lies between p and t it must lie between f and b . Wherefore, the probability it should lie between f and b cannot be less than the ratio of pt to AB , and therefore must be greater than the ratio of fc to AB (since pt is greater than fc). And after the same manner you may prove that the forementioned probability cannot be greater than the ratio of fb to AB , it must therefore be the same.

Lem. 2. The ball W having been thrown, and the line os drawn, the probability of the event M in a single trial is the ratio of Ao to AB .

For, in the same manner as in the foregoing lemma, the probability that the ball o being thrown shall rest

rest somewhere upon D_o or between AD and so is the ratio of Ao to AB . But the resting of the ball o between AD and so after a single throw is the happening of the event M in a single trial. Wherefore the lemma is manifest.

P R O P. 8.

If upon BA you erect the figure $BgbikmA$ whose property is this, that (the base BA being divided into any two parts, as Ab , and Bb and at the point of division b a perpendicular being erected and terminated by the figure in m ; and y, x, r representing respectively the ratio of bm , Ab , and Bb to AB , and E being the coefficient of the term in which occurs $a^p b^q$ when the binomial $\overline{a+b}^{p+q}$ is expanded) $y = E x^p r^q$. I say that before the ball W is thrown, the probability the point o should fall between f and b , any two points named in the line AB , and withall that the event M should happen p times and fail q in $p + q$ trials, is the ratio of $fgbikmb$, the part of the figure $BgbikmA$ intercepted between the perpendiculars fg , bm raised upon the line AB , to CA the square upon AB .

D E M O N S T R A T I O N.

For if not; 1st let it be the ratio of D a figure greater than $fgbikmb$ to CA , and through the points e, d, c draw perpendiculars to fb meeting the curve $AmigB$ in b, i, k ; the point d being so placed that di shall be the longest of the perpendiculars

culars terminated by the line fb , and the curve $A m i g B$; and the points e, d, c being so many and so placed that the rectangles, bk, ci, ei, fb taken together shall differ less from $fgbikmb$ than D does; all which may be easily done by the help of the equation of the curve, and the difference between D and the figure $fgbikmb$ given. Then since di is the longest of the perpendicular ordinates that insist upon fb , the rest will gradually decrease as they are farther and farther from it on each side, as appears from the construction of the figure, and consequently eb is greater than gf or any other ordinate that insists upon ef .

Now if A_o were equal to A_e , then by lem. 2. the probability of the event M in a single trial would be the ratio of A_e to AB , and consequently by cor. Prop. 1. the probability of its failure would be the ratio of B_e to AB . Wherefore, if x and r be the two forementioned ratios respectively, by Prop. 7. the probability of the event M happening p times and failing q in $p+q$ trials would be $E x^p r^q$. But x and r being respectively the ratios of A_e to AB and B_e to AB , if y is the ratio of eb to AB , then, by construction of the figure $A i B$, $y = E x^p r^q$. Wherefore, if A_o were equal to A_e the probability of the event M happening p times and failing q in $p+q$ trials would be y , or the ratio of eb to AB . And if A_o were equal to A_f , or were any mean between A_e and A_f , the last mentioned probability for the same reasons would be the ratio of fg or some other of the ordinates insisting upon ef , to AB . But eb is the greatest of all the ordinates that insist upon ef . Wherefore, upon supposition the point should lie

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any where between f and e , the probability that the event M happens p times and fails q in $p+q$ trials can't be greater than the ratio of eb to A B. There then being these two subsequent events, the 1st that the point o will lie between e and f , the 2d that the event M will happen p times and fail q in $p+q$ trials, and the probability of the 1st (by lemma 1st) is the ratio of ef to A B, and upon supposition the 1st happens, by what has been now proved, the probability of the 2d cannot be greater than the ratio of eb to A B, it evidently follows (from Prop. 3.) that the probability both together will happen cannot be greater than the ratio compounded of that of ef to A B and that of eb to A B, which compound ratio is the ratio of fb to C A. Wherefore, the probability that the point o will lie between f and e , and the event M happen p times and fail q , is not greater than the ratio of fb to C A. And in like manner the probability the point o will lie between e and d , and the event M happen and fail as before, cannot be greater than the ratio of ei to C A. And again, the probability the point o will lie between d and c , and the event M happen and fail as before, cannot be greater than the ratio of ci to C A. And lastly, the probability that the point o will lie between c and b , and the event M happen and fail as before, cannot be greater than the ratio of bk to C A. Add now all these several probabilities together, and their sum (by Prop. 1.) will be the probability that the point will lie somewhere between f and b , and the event M happen p times and fail q in $p+q$ trials. Add likewise the correspondent ratios together, and their sum will be the ratio of the sum of the antecedents to

to their common consequent, i. e. the ratio of fb , ei , ci , bk together to CA ; which ratio is less than that of D to CA , because D is greater than fb , ei , ci , bk together. And therefore, the probability that the point o will lie between f and b , and withal that the event M will happen p times and fail q in $p+q$ trials, is less than the ratio of D to CA ; but it was supposed the same which is absurd. And in like manner, by inscribing rectangles within the figure, as eg , db , dk , cm , you may prove that the last mentioned probability is greater than the ratio of any figure less than $fgbikmb$ to CA .

Wherefore, that probability must be the ratio of $fg\ b\ i\ k\ m\ b$ to C A.

Cor. Before the ball W is thrown the probability that the point o will lie somewhere between A and B, or somewhere upon the line A B, and withal that the event M will happen p times, and fail q in $p + q$ trials is the ratio of the whole figure A : B to C A. But it is certain that the point o will lie somewhere upon A B. Wherefore, before the ball W is thrown the probability the event M will happen p times and fail q in $p + q$ trials is the ratio of A : B to C A.

P R O P. 9.

If before any thing is discovered concerning the place of the point o , it should appear that the event M had happened p times and failed q in $p + q$ trials, and from hence I guess that the point o lies between any two points in the line A B, as f and b , and consequently that the probability of the event M in a single trial was somewhere between the ratio of $A b$ to $A B$ and that of $A f$ to $A B$: the probability I am in

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the right is the ratio of that part of the figure A*i*B described as before which is intercepted between perpendiculars erected upon A B at the points *f* and *b*, to the whole figure A*i*B.

For, there being these two subsequent events, the first that the point *o* will lie between *f* and *b*; the second that the event M should happen *p* times and fail *q* in *p* + *q* trials; and (by cor. prop. 8.) the original probability of the second is the ratio of A*i*B to C A, and (by prop. 8.) the probability of both is the ratio of *fgbimb* to C A; wherefore (by prop. 5) it being first discovered that the second has happened, and from hence I guess that the first has happened also, the probability I am in the right is the ratio of *fgbimb* to A*i*B, the point which was to be proved.

Cor. The same things supposed, if I guess that the probability of the event M lies somewhere between *o* and the ratio of A*b* to A B, my chance to be in the right is the ratio of A*b**m* to A*i*B.

S C H O L I U M .

From the preceding proposition it is plain, that in the case of such an event as I there call M, from the number of times it happens and fails in a certain number of trials, without knowing any thing more concerning it, one may give a guess whereabouts it's probability is, and, by the usual methods computing the magnitudes of the areas there mentioned, see the chance that the guess is right. And that the same rule is the proper one to be used in the case of an event concerning the probability of which we

we absolutely know nothing antecedently to any trials made concerning it, seems to appear from the following consideration ; viz. that concerning such an event I have no reason to think that, in a certain number of trials, it should rather happen any one possible number of times than another. For, on this account, I may justly reason concerning it as if its probability had been at first unfixed, and then determined in such a manner as to give me no reason to think that, in a certain number of trials, it should rather happen any one possible number of times than another. But this is exactly the case of the event M. For before the ball W is thrown, which determines its probability in a single trial, (by cor. prop. 8.) the probability it has to happen p times and fail q in $p + q$ or n trials is the ratio of A : B to C A, which ratio is the same when $p + q$ or n is given, whatever number p is ; as will appear by computing the magnitude of A : B by the method * of fluxions. And consequently before the place of the point o is discovered or the number of times the event M has happened in n trials, I can have no reason to think it should rather happen one possible number of times than another.

In what follows therefore I shall take for granted that the rule given concerning the event M in prop. 9. is also the rule to be used in relation to any event concerning the probability of which nothing

* It will be proved presently in art. 4. by computing in the method here mentioned that A : B contracted in the ratio of E to 1 is to C A as 1 to $\underline{n+1} \times E$: from whence it plainly follows that, antecedently to this contraction, A : B must be to C A in the ratio of 1 to $n+1$, which is a constant ratio when n is given, whatever p is.

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at all is known antecedently to any trials made or observed concerning it. And such an event I shall call an unknown event.

Cor. Hence, by supposing the ordinates in the figure AiB to be contracted in the ratio of E to one, which makes no alteration in the proportion of the parts of the figure intercepted between them, and applying what is said of the event M to an unknown event, we have the following proposition, which gives the rules for finding the probability of an event from the number of times it actually happens and fails.

P R O P. 10.

If a figure be described upon any base AH (Vid. Fig.) having for it's equation $y = x^p r^q$; where y , x , r are respectively the ratios of an ordinate of the figure insisting on the base at right angles, of the segment of the base intercepted between the ordinate and A the beginning of the base, and of the other segment of the base lying between the ordinate and the point H , to the base as their common consequent. I say then that if an unknown event has happened p times and failed q in $p + q$ trials, and in the base AH taking any two points as f and t you erect the ordinates fc , tF at right angles with it, the chance that the probability of the event lies somewhere between the ratio of Af to AH and that of At to AH , is the ratio of $tFCf$, that part of the before-described figure which is intercepted between the two ordinates, to $ACFH$ the whole figure insisting on the base AH .

This is evident from prop. 9. and the remarks made in the foregoing scholium and corollary.

Now, in order to reduce the foregoing rule to practice, we must find the value of the area of the figure described and the several parts of it separated, by ordinates perpendicular to its base. For

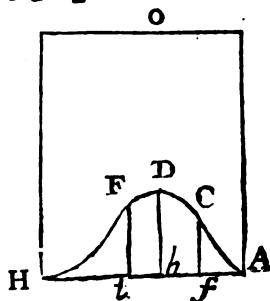
which purpose, suppose $AH = 1$ and HO the square upon AH likewise = 1, and Cf will be = y , and $Af = x$, and $Hf = r$, because y , x and r denote the ratios of Cf , Af , and Hf respectively to AH . And by the equation of the curve $y = x^p r^q$ and (because $Af + fH = AH$) $r + x = 1$. Wherefore

$$y = x^p \times 1 - x^q = x^p - qx^{p+1} + q \times \frac{q-1}{2} \times x^{p+2} - q$$

$\times \frac{q-1}{2} \times \frac{q-2}{3} \times x^{p+3} + \text{etc.}$ Now the abscisse being x and the ordinate x^p the correspondent area is $\frac{x^{p+1}}{p+1}$

(by prop. 10. cas. 1. Quadrat. Newt.)* and the ordinate being qx^{p+1} the area is $\frac{qx^{p+2}}{p+2}$; and in like man-

\ddots



* Tis very evident here, without having recourse to Sir Isaac Newton, that the fluxion of the area ACf being $y\dot{x} = x^p \dot{x} -$

$\frac{p+1}{2} q x^{p+1} \dot{x} + q \times \frac{q-1}{2} \times x^{p+2} \dot{x}$ &c. the fluent or area itself is $\frac{x^{p+2}}{p+2} - \frac{q+1}{p+1}$

$= q \times \frac{x^{p+2}}{p+2} + \frac{q \times q-1}{2} \times \frac{x^{p+3}}{p+3}$, &c.

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ner of the rest. Wherefore, the abscisse being x and the ordinate y or $x - qx^p + \text{&c.}$ the correspondent

area is $\frac{x^{p+1}}{p+1} - \frac{qx^p}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+2}}{p+3} - q \times \frac{q-1}{2} \times \frac{x^{p+3}}{p+4} + \text{&c.}$ Wherefore, if $x = Af = \frac{Af}{AH}$

and $y = Cf = \frac{Cf}{AH}$, then $A Cf = \frac{A Cf}{HO} = x^{\frac{p+1}{p+1}} - \frac{q \times x^p}{p+2} + q \times \frac{q-1}{2} \times \frac{x^{p+2}}{p+3} - \text{&c.}$

From which equation, if q be a small number, it is easy to find the value of the ratio of $A Cf$ to HO . and in like manner as that was found out, it will appear that the ratio of HCf to HO is $r^{\frac{q+1}{q+1}} - p \times$

$\frac{q+2}{q+2} + p \times \frac{p-1}{2} \times r^{\frac{q+3}{q+3}} - p \times \frac{p-1}{2} \times \frac{p-2}{3} \times r^{\frac{q+4}{q+4}}$ &c.

which series will consist of few terms and therefore is to be used when p is small.

2. The same things supposed as before, the ratio of $A Cf$ to HO is $\frac{x^{p+1}}{p+1} - \frac{r^q}{p+1} + \frac{q \times x^p}{p+1} \times \frac{r^{q-1}}{p+2} + \frac{q \times}{p+1} \frac{q-1}{p+2} \times \frac{x^{p+2}}{p+3} \times \frac{r^{q-2}}{p+4} + \frac{q \times}{p+1} \frac{q-1}{p+2} \times \frac{x^{p+3}}{p+4} \times \frac{r^{q-3}}{p+4} + \text{&c.}$

&c. + $x \frac{n+1}{n+1} \times \frac{q}{p+1} \times \frac{q-1}{p+2} \times \text{&c.} \times \frac{1}{n}$ where $n =$

$\frac{p+1}{p+q}$. For this series is the same with $x \frac{p+1}{p+1} - q x$
 $x \frac{p+2}{p+2}$ &c. set down in Art. 1st. as the value of the
 $\frac{p+2}{p+2}$

ratio of A C f to H O ; as will easily be seen by putting in the former instead of r its value $1-x$, and expanding the terms and ordering them according to the powers of x . Or, more readily, by comparing the fluxions of the two series, and in the former instead of r substituting $-x^*$.

* The fluxion of the first series is $x \frac{p}{r} \dot{x} + q x \frac{p+1}{r^{p-1}} \dot{r} +$
 $q x \frac{p+1}{r^p} \dot{x} + q \times \frac{q-1}{p+1} \times x \frac{p+2}{r^{p+1}} \dot{r} + q \times \frac{q-1}{p+2} \times x \frac{p+2}{r^{p+2}} \dot{x}$
 $+ q \times \frac{q-1}{p+3} \times x \frac{p+3}{r^{q-3}} \dot{r}$ &c. or, substituting $-\dot{x}$ for \dot{r} ,
 $x \frac{p}{r} \dot{x} - q x \frac{p+1}{r^{q-1}} \dot{x} + q x \frac{p+1}{r^{q-1}} \dot{x} - q \times \frac{q-1}{p+1} \times$
 $x \frac{p+2}{r^{q-2}} \dot{x} + q \times \frac{q-1}{p+2} \times x \frac{p+2}{r^{q-2}} \dot{x}$ &c. which, as all the
 terms after the first destroy one another, is equal to $x^p r^q \dot{x} =$
 $x^p \times 1-x^q \dot{x} = x^p \dot{x} \times 1-qx+q \times \underline{q-1} x^2$ &c. = $x^p \dot{x} -$
 $q \times \dot{x} + q \times \frac{q-1}{p+1} x^{\frac{p+2}{2}}$ &c. = the fluxion of the latter series
 or of $x \frac{p+1}{p+1} - q \times x \frac{p+2}{p+2}$ &c. The two series therefore are
 the same.

3. In like manner, the ratio of HCF to HO is

$$\frac{r}{q+1} \times \frac{x^p}{x^p} + p \times r \frac{x^{p-1}}{q+1} + p \times p-1 \times r \frac{x^{p-2}}{q+2} + \dots$$

&c.

4. If E be the coefficient of that term of the binomial $a+b^{p+q}$ expanded in which occurs $a^p b^q$, the ratio of the whole figure ACFH to HO is

$$\frac{1}{n+1} \times \frac{1}{E}$$
, n being $= p+q$. For, when $Af = \text{AH}$,
 $x=1$, $r=0$. Wherefore, all the terms of the series set down in Art. 2. as expressing the ratio of ACF to HO will vanish except the last, and that becomes $\frac{1}{n+1} \times \frac{q}{p+1} \times \frac{q-1}{p+2} \times \text{etc.} \times \frac{1}{n}$. But E being the coefficient of that term in the binomial $\underline{a+b}^n$ expanded in which occurs $a^p b^q$ is equal to

$$\frac{p+1}{q} \times \frac{p+2}{q-1} \times \text{etc.} \times \frac{n}{1}$$
. And, because Af is supposed to become $= \text{AH}$, $\text{ACF} = \text{ACH}$. From whence this article is plain.

5. The ratio of ACF to the whole figure ACFH is (by Art. 1. and 4.) $\frac{1}{n+1} \times E \times \frac{x^{p+1}}{x^{p+1}} - q \times \frac{x^{p+2}}{x^{p+2}} + q \times \frac{q-1}{x^{p+3}} \times \frac{x^{p+3}}{x^{p+3}}$ &c. and if, as x expresses the ratio of Af to AH , X should express the ratio of Af to AH ; the ratio of Af to ACFH would be $\frac{1}{n+1} \times E \times \frac{X^{p+1}}{X^{p+1}} - q \frac{X^{p+2}}{X^{p+2}} + q \times \frac{q-1}{X^{p+3}} \times \frac{X^{p+3}}{X^{p+3}}$ &c. and consequently the ratio of ACF to ACFH is $\frac{1}{n+1} \times E \times \frac{d}{d}$ into the difference between

between the two series. Compare this with prop. 10, and we shall have the following practical rule.

R U L E I.

If nothing is known concerning an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I guess that the probability of its happening in a single trial lies somewhere between any two degrees of probability as X and x , the chance I am in the right in my guess is $\frac{n+1}{n+2}$

$\times E \times x^d$ into the difference between the series $X^{\frac{p+1}{p+2}}$
 $- q X^{\frac{p+2}{p+2}} + q \times \frac{q-1}{2} \times X^{\frac{p+3}{p+3}}$ — &c. and the
series $x^{\frac{p+1}{p+1}} - q x^{\frac{p+2}{p+2}} + q \times \frac{q-1}{2} \times x^{\frac{p+3}{p+3}}$ — &c. E

being the coefficient of $a^p b^q$ when $a+b$ is expanded.

This is the proper rule to be used when q is a small number; but if q is large and p small, change every where in the series here set down p into q and q into p and x into r or $1-x$, and X into $R = 1-X$; which will not make any alteration in the difference between the two serieses.

Thus far Mr. Bayes's essay.

With respect to the rule here given, it is further to be observed, that when both p and q are very large numbers, it will not be possible to apply it to practice on account of the multitude of terms which the serieses in it will contain. Mr. Bayes, therefore, by

... F f f z 325 ah

an investigation which it would be too tedious to give here, has deduced from this rule another, which is as follows.

R U L E 2.

If nothing is known concerning an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I guess that the probability of its happening in a single trial lies between $\frac{p}{n} + z$ and $\frac{p}{n} - z$; if $m^2 = \frac{n^2}{pq}$ $a = \frac{p}{n}$, $b = \frac{q}{n}$, E the coefficient of the term in which occurs $a^p b^q$ when $\overline{a+b}^n$ is expanded, and $\Sigma = \frac{n+1}{n} \times \frac{\sqrt{2pq}}{\sqrt{n}} \times E a^p b^q \times$ by the series $mz = \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5} - \frac{n-2 \times n-4}{2n \times 3n} \times \frac{m^7 z^7}{7} + \frac{n-2}{2n} \times \frac{n-4}{3n} \times \frac{n-6}{4n} \times \frac{m^9 z^9}{9}$ &c. my chance to be in the right is greater than $\frac{2 \Sigma}{1 + 2 E a^p b^q + \frac{2 E a^p b^q}{n}}$ and less than $\frac{2 \Sigma}{1 - 2 E a^p b^q - \frac{2 E a^p b^q}{n}}$. And if $p = q$ my chance is 2Σ exactly.

* In Mr. Bayes's manuscript this chance is made to be greater than $\frac{2 \Sigma}{1 + 2 E a^p b^q}$ and less than $\frac{2 \Sigma}{1 - 2 E a^p b^q}$. The third term in the two divisors, as I have given them, being omitted. But this being evidently owing to a small oversight in the deduction of this rule, which I have reason to think Mr. Bayes had himself discovered, I have ventured to correct his copy, and to give the rule as I am satisfied it ought to be given.

In

In order to render this rule fit for use in all cases it is only necessary to know how to find within sufficient nearness the value of $E a^p b^q$ and also of the series $m z - \frac{m^3 z^3}{3} \text{ &c.}^*$. With respect to the former Mr. Bayes has proved that, supposing K to signify the ratio of the quadrantal arc to its radius, $E a^p b^q$ will be equal to $\frac{\sqrt{n}}{2 \sqrt{K p q}} \times$ by the ratio whose hyperbo-

lic logarithm is $\frac{1}{12} \times \frac{1}{n} - \frac{1}{p} - \frac{1}{q} - \frac{1}{360} \times \frac{1}{n^3} - \frac{1}{p^3}$
 $\frac{1}{q^3} + \frac{1}{1260} \times \frac{1}{n^5} - \frac{1}{p^5} - \frac{1}{q^5} - \frac{1}{1680} \times \frac{1}{n^7} - \frac{1}{p^7} -$
 $\frac{1}{q^7} + \frac{1}{1188} \times \frac{1}{n^9} - \frac{1}{p^9} - \frac{1}{q^9} \text{ &c. where the numerical coefficients may be found in the following manner. Call them A, B, C, D, E, &c. Then } A = \frac{1}{2 \cdot 2 \cdot 3} = \frac{1}{3 \cdot 4}.$ $B = \frac{1}{2 \cdot 4 \cdot 5} - \frac{A}{3}.$ $C = \frac{1}{2 \cdot 6 \cdot 7} -$
 $\frac{10B + A}{5}.$ $D = \frac{1}{2 \cdot 8 \cdot 9} - \frac{35C + 21B + A}{7}.$ $E = \frac{1}{2 \cdot 10 \cdot 11} -$
 $\frac{126C + 84D + 36B + A}{9}.$ $F = \frac{1}{2 \cdot 12 \cdot 13} -$

* A very few terms of this series will generally give the hyperbolic logarithm to a sufficient degree of exactness. A similar series has been given by Mr. De Moivre, Mr. Simpson and other eminent mathematicians in an expression for the sum of the logarithms of the numbers 1, 2, 3, 4, 5 to x , which sum they have asserted to be equal to $\frac{1}{2} \log. c + x + \frac{1}{2} \times \log. x - x + \frac{1}{2} \frac{1}{x} - \frac{1}{2} \frac{1}{x^3} + \frac{1}{2} \frac{1}{x^5} \text{ &c. } c$ denoting the circumference of a circle whose radius is unity. But Mr. Bayes, in a preceding paper in this volume, has demonstrated that, though this expression will very nearly approach to the value of this sum when only a proper number of the first terms is taken, the whole series cannot express any quantity at all, because, let x be what it will, there will be always a part of the series where it will begin to diverge. This observation, though it does not much affect the use of this series, seems well worth the notice of mathematicians.

462 D + 330 C + 165 E + 55 B + A &c. where the coefficients of B, C, D, E, F, &c. in the values of D, E, F, &c. are the 2, 3, 4, &c. highest coefficients in $a+b^2$, $a+b^3$, $a+b^4$, &c. expanded; affixing in every particular value the least of these coefficients to B, the next in magnitude to the furthest letter from B, the next to C, the next to the furthest but one, the next to D, the next to the furthest but two, and so on *.

With respect to the value of the series $mz - \frac{m^3 z^3}{3} + \frac{n-2}{2n} \times \frac{m^5 z^5}{5}$ &c. he has observed that it may be calculated directly when mz is less than 1, or even not greater than $\sqrt{3}$: but when mz is much larger it becomes impracticable to do this; in which case he shews a way of easily finding two values of it very nearly equal between which its true value must lie.

The theorem he gives for this purpose is as follows.

Let K, as before, stand for the ratio of the quadrant arc to its radius, and H for the ratio whose hyperbolic logarithm is $\frac{2^2-1}{2n} - \frac{2^4-1}{360n^3} + \frac{2^6-1}{1260n^5} - \frac{2^8-1}{1680n^7}$ &c. Then the series $mz - \frac{m^3 z^3}{3}$ &c. will be

$$\text{greater or less than the series } \frac{Hn}{n+1} \times \frac{\sqrt{K}}{\sqrt{2}} - \frac{n}{n+2} \times \frac{1 - \frac{2m^2 z^2}{n}}{\frac{2mz}{n}} \left(\frac{n}{2} + 1 \right) + \frac{\frac{n^2}{n+2} \times \frac{1 - \frac{n}{n+4 \times 4m^3 z^3}}{n+4 \times 4m^3 z^3}}{\frac{n}{n+2}} \left(\frac{n}{2} + 2 \right) + \dots$$

* This method of finding these coefficients I have deduced from the demonstration of the third lemma at the end of Mr. Simpson's Treatise on the Nature and Laws of Chance.

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$$\frac{3n^3}{n+2} \times \frac{\left| 1 - \frac{m^2 z^2}{n} \right|^{\frac{n}{2}} + 3}{n+4 \times n+6 \times 8 m^5 z^5} + \frac{3 \times 5 \times n^4}{n+2} \times \frac{\left| 1 - \frac{2 m^2 z^2}{n} \right|^{\frac{n}{2}} + 4}{n+4 \times n+6 \times n+8 \times 16 z^7 m^7}$$

— &c. continued to any number of terms, according as the last term has a positive or a negative sign before it.

From substituting these values of E at b and mz — $\frac{m^3 z^3}{3} + \frac{n-2}{2} \times \frac{m^5 z^5}{5}$ &c. in the 2d rule arises a 3d rule, which is the rule to be used when mz is of some considerable magnitude.

R U L E 3.

If nothing is known of an event but that it has happened p times and failed q in $p+q$ or n trials, and from hence I judge that the probability of it's happening in a single trial lies between $\frac{p}{n} + z$ and

$\frac{p}{n} - z$ my chance to be right is greater than

$$\frac{\sqrt{Kpq} \times b}{2\sqrt{Kpq+bn^{\frac{1}{2}}+bn^{-\frac{1}{2}}}} \times 2H - \frac{\sqrt{2}}{\sqrt{K}} \times \frac{n+1}{n+2} \times \frac{1}{mz}$$

$$\times 1 - \frac{2 m^2 z^2}{n} \left| \frac{n}{2} \right|^{\frac{n}{2}} + 1 \text{ and less than } \frac{\sqrt{Kpq} \times b}{2\sqrt{Kpq-bn^{\frac{1}{2}}-bn^{-\frac{1}{2}}}}$$

$$\text{multiplied by the 3 terms } 2H - \frac{\sqrt{2}}{\sqrt{K}} \times \frac{n+1}{n+2} \times$$

$$\times \frac{1}{mz} \times \left| 1 - \frac{2 m^2 z^2}{n} \right|^{\frac{n}{2}} + 1 + \frac{\sqrt{2}}{\sqrt{K}} \times \frac{n}{n+2} \times$$

$$\frac{n+1}{n+4} \times \frac{1}{2m^3 z^3} \times \left| 1 - \frac{2 m^2 z^2}{n} \right|^{\frac{n}{2}} + 2 \text{ where } m^2, K, b \\ \text{and } H \text{ stand for the quantities already explained.}$$

An

An APPENDIX.

CONTAINING

An Application of the foregoing Rules to some particular Cases.

TH E first rule gives a direct and perfect solution in all cases; and the two following rules are only particular methods of approximating to the solution given in the first rule, when the labour of applying it becomes too great.

The first rule may be used in all cases where either p or q are nothing or not large. The second rule may be used in all cases where mz is less than $\sqrt{3}$; and the 3d in all cases where $m^2 z^2$ is greater than 1 and less than $\frac{n}{2}$, if n is an even number and very large. If n is not large this last rule cannot be much wanted, because, m decreasing continually as n is diminished, the value of z may in this case be taken large, (and therefore a considerable interval had between $\frac{p}{n} - z$ and $\frac{p}{n} + z$) and yet the operation be carried on by the 2d rule; or mz not exceed $\sqrt{3}$.

But in order to shew distinctly and fully the nature of the present problem, and how far Mr. Bayes has carried the solution of it; I shall give the result of this solution in a few cases, beginning with the lowest and most simple.

Let

Let us then first suppose, of such an event as that called M in the essay, or an event about the probability of which, antecedently to trials, we know nothing, that it has happened *once*, and that it is enquired what conclusion we may draw from hence with respect to the probability of its happening on a *second trial*.

The answer is that there would be an odds of three to one for somewhat more than an even chance that it would happen on a second trial.

For in this case; and in all others where q is nothing, the expression $\frac{n+1}{p+1} \times \frac{X^{p+1}}{x^{p+1}} - x^{p+1}$ or $X^{p+1} - x^{p+1}$ gives the solution, as will appear from considering the first rule. Put therefore in this expression $p+1 = 2$, $X = 1$ and $x = \frac{1}{2}$ and it will be $1 - \frac{1}{2}^2$ or $\frac{3}{4}$; which shews the chance there is that the probability of an event that has happened once lies somewhere between 1 and $\frac{1}{2}$; or (which is the same) the odds that it is somewhat more than an even chance that it will happen on a second trial *.

In the same manner it will appear that if the event has happened twice, the odds now mentioned will be seven to one; if thrice, fifteen to one; and in general, if the event has happened p times, there will be an odds of $2^{p+1} - 1$ to one, for *more* than an equal chance that it will happen on further trials.

Again, suppose all I know of an event to be that it has happened ten times without failing, and the

* There can, I suppose, be no reason for observing that on this subject unity is always made to stand for certainty, and $\frac{1}{2}$ for an even chance.

enquiry to be what reason we shall have to think we are right if we guess that the probability of it's happening in a single trial lies somewhere between $\frac{1}{17}$ and $\frac{2}{3}$, or that the ratio of the causes of it's happening to those of it's failure is some ratio between that of sixteen to one and two to one.

Here $p+1=11$, $X=\frac{16}{17}$ and $x=\frac{2}{3}$ and $X^{p+1}-x^{p+1}=\frac{16}{17}^{11}-\frac{2}{3}^{11}= .5013$ &c. The answer therefore is, that we shall have very nearly an equal chance for being right.

In this manner we may determine in any case what conclusion we ought to draw from a given number of experiments which are unopposed by contrary experiments. Every one fees in general that there is reason to expect an event with more or less confidence according to the greater or less number of times in which, under given circumstances, it has happened without failing; but we here see exactly what this reason is, on what principles it is founded, and how we ought to regulate our expectations.

But it will be proper to dwell longer on this head.

Suppose a solid or die of whose number of sides and constitution we know nothing; and that we are to judge of these from experiments made in throwing it.

In this case, it should be observed, that it would be in the highest degree improbable that the solid should, in the first trial, turn any one side which could be assigned before hand; because it would be known that some side it must turn, and that there was an infinity of other sides, or sides otherwise marked, which it was equally likely that it should turn. The first throw

throw only shews that *it has* the side then thrown, without giving any reason to think that it has it any one number of times rather than any other. It will appear, therefore, that *after* the first throw and not before, we should be in the circumstances required by the conditions of the present problem, and that the whole effect of this throw would be to bring us into these circumstances. That is: the turning the side first thrown in any subsequent single trial would be an event about the probability or improbability of which we could form no judgment, and of which we should know no more than that it lay somewhere between nothing and certainty. With the second trial then our calculations must begin; and if in that trial the supposed solid turns again the same side, there will arise the probability of three to one that it has more of that sort of sides than of *all* others; or (which comes to the same) that there is somewhat in its constitution disposing it to turn that side oftener: And this probability will increase, in the manner already explained, with the number of times in which that side has been thrown without failing. It should not, however, be imagined that any number of such experiments can give sufficient reason for thinking that it would *never* turn any other side. For, suppose it has turned the same side in every trial a million of times. In these circumstances there would be an improbability that it had *less* than 1.400,000 more of these sides than all others; but there would also be an improbability that it had *above* 1.600,000 times more. The chance for the latter is expressed by $\frac{1}{1.600.000}$ raised to the millioneth power subtracted from unity, which is equal to .4647 &c. and

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the

the chance for the former is equal to $\frac{1}{1400000}$; raised to the same power, or to .4895; which, being both less than an equal chance, proves what I have said. But though it would be thus improbable that it had *above* 1.600,000 times more or *less* than 1.400,000 times *more* of these sides than of all others, it by no means follows that we have any reason for judging that the true proportion in this case lies somewhere between that of 1.600,000 to one and 1.400,000 to one. For he that will take the pains to make the calculation will find that there is nearly the probability expressed by .527, or but little more than an equal chance, that it lies somewhere between that of 600,000 to one and three millions to one. It may deserve to be added, that it is more probable that this proportion lies somewhere between that of 900,000 to 1 and 1.900,000 to 1 than between any other two proportions whose antecedents are to one another as 900,000 to 1.900,000, and consequents unity.

I have made these observations chiefly because they are all strictly applicable to the events and appearances of nature. Antecedently to all experience, it would be improbable as infinite to one, that any particular event, before-hand imagined, should follow the application of any one natural object to another; because there would be an equal chance for any one of an infinity of other events. But if we had once seen any particular effects, as the burning of wood on putting it into fire, or the falling of a stone on detaching it from all contiguous objects, then the conclusions to be drawn from any number of subsequent events of the same kind would be to be determined in the same manner with the conclusions just mentioned relating to the constitution of the solid I have supposed.

supposed. —— In other words. The first experiment supposed to be ever made on any natural object would only inform us of one event that may follow a particular change in the circumstances of those objects ; but it would not suggest to us any ideas of uniformity in nature, or give us the least reason to apprehend that it was, in that instance or in any other, regular rather than irregular in its operations. But if the same event has followed without interruption in any one or more subsequent experiments, then some degree of uniformity will be observed ; reason will be given to expect the same success in further experiments, and the calculations directed by the solution of this problem may be made.

One example here it will not be amiss to give.

Let us imagine to ourselves the case of a person just brought forth into this world and left to collect from his observation of the order and course of events what powers and causes take place in it. The Sun would, probably, be the first object that would engage his attention ; but after losing it the first night he would be entirely ignorant whether he should ever see it again. He would therefore be in the condition of a person making a first experiment about an event entirely unknown to him. But let him see a second appearance or one *return* of the Sun, and an expectation would be raised in him of a second return, and he might know that there was an odds of 3 to 1 for *some* probability of this. This odds would increase, as before represented, with the number of returns to which he was witness. But no finite number of returns would be sufficient to produce absolute or physical certainty. For let it be supposed that he has seen it return at regular and stated intervals a million of times. The conclusions

this would warrant would be such as follow ——
 There would be the odds of the millioneth power
 of 2, to one, that it was likely that it would return again
 at the end of the usual interval. There would be the
 probability expressed by .5352, that the odds for this
 was not greater than 1.600,000 to 1; And the pro-
 bability expressed by .5105, that it was not less than
 1.400,000 to 1.

It should be carefully remembered that these de-
 ductions suppose a previous total ignorance of nature.
 After having observed for some time the course of
 events it would be found that the operations of nature
 are in general regular, and that the powers and laws
 which prevail in it are stable and permanent. The
 consideration of this will cause one or a few exper-
 iments often to produce a much stronger expectation of
 success in further experiments than would otherwise
 have been reasonable; just as the frequent observation
 that things of a sort are disposed together in any place
 would lead us to conclude, upon discovering there
 any object of a particular sort, that there are laid up
 with it many others of the same sort. It is obvious
 that this, so far from contradicting the foregoing de-
 ductions, is only one particular case to which they are
 to be applied.

What has been said seems sufficient to shew us
 what conclusions to draw from *uniform* experience.
 It demonstrates, particularly, that instead of proving
 that events will *always* happen agreeably to it, there
 will be always reason against this conclusion. In other
 words, where the course of nature has been the most
 constant, we can have only reason to reckon upon a
 recurrency of events proportioned to the degree of
 this

this constancy; but we can have no reason for thin king that there are no causes in nature which will ever interfere with the operations of the causes from which this constancy is derived, or no circumstances of the world in which it will fail. And if this is true, supposing our only *data* derived from experience, we shall find additional reason for thinking thus if we apply other principles, or have recourse to such considerations as reason, independently of experience, can suggest.

But I have gone further than I intended here; and it is time to turn our thoughts to another branch of this subject: I mean, to cases where an experiment has sometimes succeeded and sometimes failed.

Here, again, in order to be as plain and explicit as possible, it will be proper to put the following case, which is the easiest and simplest I can think of.

Let us then imagine a person present at the drawing of a lottery, who knows nothing of its scheme or of the proportion of *Blanks* to *Prizes* in it. Let it further be supposed, that he is obliged to infer this from the number of *blanks* he hears drawn compared with the number of *prizes*; and that it is enquired what conclusions in these circumstances he may reasonably make.

Let him first hear *ten* blanks drawn and *one* prize, and let it be enquired what chance he will have for being right if he guesses that the proportion of *blanks* to *prizes* in the lottery lies somewhere between the proportions of *9* to *1* and *11* to *1*.

Here taking $X = \frac{1}{11}$, $x = \frac{9}{10}$, $p = 10$, $q = 1$, $n = 11$, $E = 11$, the required chance, according to the first rule,

rule, is $\frac{n+1}{p+1} \times E$ into the difference between
 $\frac{X}{p+1} - \frac{qX}{p+2}$ and $\frac{x}{p+1} - \frac{qx}{p+2} = 12 \times 11$

$$\frac{\overline{X}}{\overline{p+1}} - \frac{\overline{qX}}{\overline{p+2}} - \frac{\overline{x}}{\overline{p+1}} - \frac{\overline{qx}}{\overline{p+2}}$$

$$\frac{\overline{11}}{\overline{12}} - \frac{\overline{11}}{\overline{12}} - \frac{\overline{9}}{\overline{10}} - \frac{\overline{9}}{\overline{10}} = .07699$$

&c. There would therefore be an odds of about 923 to 76, or nearly 12 to 1 *against* his being right. Had he guessed only in general that there were less than 9 blanks to a prize, there would have been a probability of his being right equal to .6589, or the odds of 65 to 34.

Again, suppose that he has heard 20 blanks drawn and 2 prizes; what chance will he have for being right if he makes the same guess?

Here X and x being the same, we have $n=22$, $p=20$, $q=2$, $E=231$, and the required chance equal to $\frac{n+1}{p+1} \times E \times \frac{X}{p+1} - \frac{qX}{p+2} + q \times \frac{q-1}{2} \times \frac{X}{p+3}$
 $= \frac{\overline{x}}{\overline{p+1}} - \frac{\overline{qx}}{\overline{p+2}} + q \times \frac{q-1}{2} \times \frac{\overline{x}}{\overline{p+3}} = .10843$ &c.

He will, therefore, have a better chance for being right than in the former instance, the odds against him now being 892 to 108 or about 9 to 1. But should he only guess in general, as before, that there were less than 9 blanks to a prize, his chance for being right will be worse; for instead of .6589 or an odds of near two to one, it will be .584, or an odds of 584 to 415.

Suppose,

Suppose, further, that he has heard 40 *blanks* drawn and 4 *prizes*; what will the before-mentioned chances be?

The answer here is .1525, for the former of these chances; and .527, for the latter. There will, therefore, now be an odds of only $5\frac{1}{2}$ to 1 against the proportion of blanks to prizes lying between 9 to 1 and 11 to 1; and but little more than an equal chance that it is less than 9 to 1.

Once more. Suppose he has heard 100 *blanks* drawn and 10 *prizes*.

The answer here may still be found by the first rule; and the chance for a proportion of blanks to prizes *less* than 9 to 1 will be .44109, and for a proportion *greater* than 11 to 1 .3082. It would therefore be likely that there were not *fewer* than 9 or *more* than 11 blanks to a prize. But at the same time it will remain unlikely * that the true proportion should lie between 9 to 1 and 11 to 1, the chance for this being .2506 &c. There will therefore be still an odds of near 3 to 1 against this.

From these calculations it appears that, in the circumstances I have supposed, the chance for being right in guessing the proportion of *blanks* to *prizes* to be nearly the same with that of the number of *blanks*

* I suppose no attentive person will find any difficulty in this. It is only saying that, supposing the interval between nothing and certainty divided into a hundred equal chances, there will be 44 of them for a less proportion of blanks to prizes than 9 to 1, 31 for a greater than 11 to 1, and 25 for some proportion between 9 to 1 and 11 to 1; in which it is obvious that, though one of these suppositions must be true, yet, having each of them more chances against them than for them, they are all separately unlikely.

drawn in a given time to the number of prizes drawn, is continually increasing as these numbers increase; and that therefore, when they are considerably large, this conclusion may be looked upon as morally certain. By parity of reason, it follows universally, with respect to every event about which a great number of experiments has been made, that the causes of its happening bear the same proportion to the causes of its failing, with the number of happenings to the number of failures; and that, if an event whose causes are supposed to be known, happens oftener or seldom than is agreeable to this conclusion, there will be reason to believe that there are some unknown causes which disturb the operations of the known ones. With respect, therefore, particularly to the course of events in nature, it appears, that there is demonstrative evidence to prove that they are derived from permanent causes, or laws originally established in the constitution of nature in order to produce that order of events which we observe, and not from any of the powers of chance*. This is just as evident as it would be, in the case I have insisted on, that the reason of drawing 10 times more *blanks* than *prizes* in millions of trials, was, that there were in the wheel about so many more *blanks* than *prizes*.

But to proceed a little further in the demonstration of this point.

We have seen that supposing a person, ignorant of the whole scheme of a lottery, should be led to conjecture, from hearing 100 *blanks* and 10 *prizes* drawn,

* See Mr. De Moivre's *Doctrine of Chances*, pag. 250.

that

that the proportion of *blanks* to *prizes* in the lottery was somewhere between 9 to 1 and 11 to 1, the chance for his being right would be .2505 &c. Let now enquire what this chance would be in some higher cases.

Let it be supposed that *blanks* have been drawn 1000 times, and prizes 100 times in 1100 trials.

In this case the powers of X and x rise so high, and the number of terms in the two serieses $\frac{X^{p+1}}{p+1}$ — $\frac{q X^{p+1}}{p+2}$ &c. and $\frac{x^{p+1}}{p+1} — \frac{q x^{p+2}}{p+2}$ &c. become

so numerous that it would require immense labour to obtain the answer by the first rule. 'Tis necessary, therefore, to have recourse to the second rule. But in order to make use of it, the interval between X and x must be a little altered. $\frac{1}{11} - \frac{1}{10}$ is $\frac{1}{110}$, and therefore the interval between $\frac{1}{11} - \frac{1}{10}$ and $\frac{1}{11} + \frac{1}{10}$ will be nearly the same with the interval between $\frac{1}{10}$ and $\frac{1}{9}$, only somewhat larger. If then we make the question to be; what chance there would be (supposing no more known than that blanks have been drawn 1000 times and prizes 100 times in 1100 trials) that the probability of drawing a blank in a single trial would lie somewhere between $\frac{1}{11} - \frac{1}{10}$ and $\frac{1}{11} + \frac{1}{10}$ we shall have a question of the same kind with the preceding questions, and deviate but little from the limits assigned in them.

The answer, according to the second rule, is that

this chance is greater than $\frac{2 \sum}{1 - 2 E a^p b^q + 2 E a^p b^q}$

H h h 2

and

and less than $\frac{2 \Sigma}{1 - 2 E a^p b^q - 2 E a^p b^q}$, E being $\frac{n+1}{n}$

$$\times \frac{\sqrt{2pq}}{\sqrt{n}} \times E a^p p^q \times mz - \frac{m^3 z^3}{3} + \frac{n-2}{2^n} \times \frac{m^5 z^5}{5} \text{ &c.}$$

By making here $1000 = p$ $100 = q$ $1100 = n$

$$z = \frac{1}{10} = x, m = \frac{\sqrt{n^3}}{\sqrt{pq}} = 1.048808, E a^p b^q = \frac{b}{2} \times \frac{\sqrt{n}}{\sqrt{Kpq}}, b$$

being the ratio whose hyperbolic logarithm is $\frac{1}{1-\frac{1}{2}}$ \times

$$\frac{1}{n} - \frac{1}{p} - \frac{1}{q} - \frac{1}{360} \times \frac{1}{n^3} - \frac{1}{p^3} - \frac{1}{q^3} + \frac{1}{1260} \times \frac{1}{n^5} - \frac{1}{p^5} - \frac{1}{q^5} \text{ &c.}$$

and K the ratio of the quadrantal arc to radius; the former of these expressions will be found to be .7953, and the latter .9405 &c. The chance enquired after, therefore, is greater than .7953, and less than .9405. That is; there will be an odds for being right in guessing that the proportion of blanks to prizes lies *nearly* between 9 to 1 and 11 to 1, (or *exactly* between 9 to 1 and 1111 to 99) which is greater than 4 to 1, and less than 16 to 1.

Suppose, again, that no more is known than that *blanks* have been drawn 10,000 times and *prizes* 1000 times in 11000 trials; what will the chance now mentioned be?

Here the second as well as the first rule becomes useless, the value of mz being so great as to render it scarcely possible to calculate directly the series $mz - \frac{m^3 z^3}{3} + \frac{n-2}{2^n} \times \frac{m^5 z^5}{5} - \text{ &c.}$ The third rule, therefore, must be used; and the information it gives us is, that the required chance is greater than .97421, or more than an odds of 40 to 1.

By

By calculations similar to these may be determined universally, what expectations are warranted by any experiments, according to the different number of times in which they have succeeded and failed; or what should be thought of the probability that any particular cause in nature, with which we have any acquaintance, will or will not, in any single trial, produce an effect that has been conjoined with it.

Most persons, probably, might expect that the chances in the specimen I have given would have been greater than I have found them. But this only shews how liable we are to error when we judge on this subject independently of calculation. One thing, however, should be remembered here; and that is, the narrowness of the interval between $\frac{9}{10}$ and $\frac{1}{2}$, or between $\frac{9}{10} + \frac{1}{5}$ and $\frac{9}{10} - \frac{1}{5}$. Had this interval been taken a little larger, there would have been a considerable difference in the results of the calculations. Thus had it been taken double, or $z = \frac{1}{3}$, it would have been found in the fourth instance that instead of odds against there were odds for being right in judging that the probability of drawing a blank in a single trial lies between $\frac{9}{10} + \frac{1}{3}$ and $\frac{9}{10} - \frac{1}{3}$.

The foregoing calculations further shew us the uses and defects of the rules laid down in the essay. 'Tis evident that the two last rules do not give us the required chances within such narrow limits as could be wished. But here again it should be considered, that these limits become narrower and narrower as q is taken larger in respect of p ; and when p and q are equal, the exact solution is given in all cases by the second rule. These two rules therefore afford a direction

a direction to our judgment that may be of considerable use till some person shall discover a better approximation to the value of the two series's in the first rule †.

But what most of all recommends the solution in this *Essay* is, that it is compleat in those cases where information is most wanted, and where Mr. De Moivre's solution of the inverse problem can give little or no direction; I mean, in all cases where either p or q are of no considerable magnitude. In other cases, or when both p and q are very considerable, it is not difficult to perceive the truth of what has been here demonstrated, or that there is reason to believe in general that the chances for the happening of an event are to the chances for its failure in the same *ratio* with that of p to q . But we shall be greatly deceived if we judge in this manner when either p or q are small. And tho' in such cases the *Data* are not sufficient to discover the exact probability of an event, yet it is very agreeable to be able to find the limits between which it is reasonable to think it must lie, and also to be able to determine the precise degree of assent which is due to any conclusions or assertions relating to them.

† Since this was written I have found out a method of considerably improving the approximation in the 2d and 3d rules by,

$\frac{2}{n} \sum$

demonstrating that the expression $1 + 2 \sum E a^p b^q + \frac{2}{n} \sum E a^p b^q$ comes

almost as near to the true value wanted as there is reason to desire, only always somewhat less. It seems necessary to hint this here; though the proof of it cannot be given.

LIII. *An Account of the Sea Pen, or Pennatula Phosphorea of Linnaeus; likewise a Description of a new Species of Sea Pen, found on the Coast of South-Carolina, with Observations on Sea-Pens in general. In a Letter to the Honourable Coote Molesworth, Esq; M. D. and F. R. S. from John Ellis, Esq; F. R. S. and Member of the Royal Academy at Upsal.*

Dear Sir,

Read Dec. 22, 1763. I Should make some apology for deferring so long the account I promised you of the Animal you were so kind to send me in February 1762, which was taken in a trawl in 72 fathoms water near the harbour of Brest in France; but a new species coming to my hand occasioned this delay. This curious sea-production, I find, by your letter, you took for a new kind of coralline, and not without reason, when upon examining it (as it was not long taken out of the sea) there were still remaining several of the suckers like heads of Polypes disposed along its fickle-shaped Pinnulæ. But when you hear of more of its properties, you will agree with me, that it belongs to another class of Animals; I shall mention only one at present, till I come to describe it more particularly, and that is, that it floats or swims about freely in the sea; whereas Corals, Corallines, Alcyonia, and

all that order of beings, adhere firmly by their bases to submarine substances.

This Animal was well known to the ancients by the name of the Sea-Pen ; many of the old authors took it for a Fucus or Sea-Plant.

This species of yours has been found in the Ocean from the coast of Norway to the most remote parts of the Mediterranean Sea, and not only dragged up in trawls from great depths of the sea, but often found floating near the surface.

Dr. Shaw, in his History of Algiers, remarks that they afford so great a light in the night to the fishermen, that they can plainly discover the fish swimming about in various depths of the sea. From this extraordinary property Doctor Linnæus calls this species of Sea-Pen, *Pennatula Phosphorea*, and remarks, after giving the synonyms of other authors, *Habitat in Oceano fundum illuminans*.

In order to attempt a description of it ; the outward appearance of this Animal is not unlike one of the quill feathers of a bird's wing, but they are found of different sizes from 4 to 8 inches in length; this of yours is about 4 inches long ; the lower half of it, is naked round and white, not unlike the quill part of a writing pen ; the upper part represents that of the feathered part of the pen, and is of a reddish colour, but faded by soaking it often in water in order to examine it more minutely. This upper half (which arises from the quill and is feathered on both sides) is a little compressed and becomes smaller and smaller till it ends in a point at the top ; along the back of this, in the same manner as in the inner side of a common writing pen, there is a groove in the middle from the

the quill to the extremity : from each side of this upper part of the stem proceed little parallel feather-like fins ; these begin at the top of the quill part, very small on each side at first, but lengthen as they advance towards the middle ; from hence they shorten gradually on each side, till they end at a point at the top ; their terminations preserving on each side the figure of the segment of a circle. I come now to consider more minutely those Pinnulæ, or feather-like fins, that project on each side and form the upper part of this animal. These are evidently designed by nature to move the animal backward or forward in the sea, consequently to do the office of fins, while at the same time, by the appearance of the suckers or mouths furnished with filaments or claws, they were certainly intended to provide food for its support ; for notwithstanding what Dr. Linnæus has said in regard to its mouth in his system of nature, viz. *Os baseos commune rotundum*, I could not, with the help of the best glasses, discover that the point of the base was penetrated in the least, so that I am clearly of opinion, that this animal, like the Hydra Arctica or Greenland Polype, which I have described in my Essay on Corallines, nourishes and supports itself by these suckers or Polype-like figures ; that by these, both kinds take in their food, and have no other visible means of discharging the exuviaæ of the animals they feed upon, than by the same way which they take them in ; and that, from attentively considering the structure and manner of living of both these animals, I shall make no doubt in classing them in the same genus of Pennatula, though they vary

very much in their exterior form and size, and consequently are of very different species.

The stem of the suckers of this animal is of a cylindrical form; from the upper part proceed 8 fine white filaments or claws to catch their food: when they retreat on the alarm of danger they draw themselves into their cases, which are formed like the denticles of the Corallines; but here each denticle is furnished with spiculae, which close together round the entrance of the denticle, and protect this tender part from external injuries.

Some time after I had made my remarks on this very extraordinary animal, the Royal Society did me the honour to recommend to me, for my opinion, some very curious observations lately published by Dr. Bohadsch of Prague, a book of great merit, which shews that the author has taken a good deal of pains, in examining very minutely into those animals called by the old authors Zoophytes: but as many of them have not the least resemblance to vegetables, I shall beg leave to pass over such, and only confine myself to this class of the Penna marina, which he seems to have been happy in observing; and therefore shall take the liberty to add such of his observations, as the opportunity of his seeing this animal alive in sea-water afforded him, without which it would have been impossible for me to have had the pleasure of gratifying you, and the rest of the Royal Society, so fully on the subject.

Some of the most curious remarks of Doctor Bohadsch on the anatomy of this animal, as also on the appearance of it while alive in sea-water, are as follows.

“ When the trunk is opened lengthways, a saltish liquor flows out of it, so viscid as to hang down an inch; the whole trunk of the stem is hollow, its outward coriaceous membrane is more than a line thick, and forms a strong covering to it: between this and another thinner membrane of the pinnated part of the trunk are innumerable little yellowish eggs, floating in a whitish liquor, about the size of a white poppy seed; these are best seen, when the trunk is cut across: This thin membrane lines the whole inside of the trunk, in which we observe nothing but a kind of yellowish bone, which takes up three parts of the cavity.

“ This bone in some of these animals is above $2\frac{1}{2}$ inches long and about half a line thick; in the middle part of it, it is four square or quadrangular; towards each end of it, it grows round and very taper: that end is smallest, which is nearest the top of the pinnated trunk. The whole bone is covered with a yellowish clear skin, which at each end changes into a ligament; one of which is inserted in the top of the pinnated trunk, the other in the top of the naked trunk; by the help of this upper ligament, the end of this little bone is either contracted into a very narrow arch, or disposed into a straight line, according to the motion of the trunk.

“ The fins likewise are composed of two skins; the outward one strong and leathery and covered over with an infinite number of crimson streaks, the inner skin is thin and clear: The cylindrical

“ part of the suckers are in the same manner, only
 “ with this difference, their outward skins may be
 “ softer.

“ Both the fins and suckers are hollow, so that
 “ the cavity of the suckers may communicate with
 “ the fins, as their cavity does with the trunk.

“ We now come to the appearance which this
 “ animal makes when alive in sea-water.

“ The trunk then was contracted circularly at
 “ the bottom of the naked part of the stem, and
 “ by this contraction formed a zone of the most
 “ intense purple, which moved upwards and
 “ downwards successively: When it moved up-
 “ wards through the length of the pinnated trunk,
 “ it there became paler, and at length terminated
 “ at the top: the motion being scarce finished, a
 “ like zone appeared at the end of the naked trunk,
 “ which finished its motion in the same manner
 “ as the former.

“ When this zone becomes very much con-
 “ stricted on every side, the trunk above it swells
 “ and acquires the form of an onion; and then
 “ it appears, as if a compressed globe moved along
 “ through the whole space of the trunk; this con-
 “ striction of the trunk gives that fine red colour
 “ to the zone; for when the skin of the trunk
 “ is outwardly full of purple papillæ, the interme-
 “ diate spaces are of a whitish colour. In this
 “ constriction then of the skin the intermediate
 “ spaces are obliterated and the papillæ are
 “ brought nearer together; consequently only the
 “ purple colour presents itself to the eyes and ap-
 “ pears more bright.

“ More-

“ Moreover the end or apex of the naked trunk
 “ is sometimes curved like a hook, and sometimes
 “ extended in a right line; both these motions
 “ then must be directed by the little bone in the
 “ inside, and from this motion of this little in-
 “ ternal bone, that sinus or cavity at the lower end
 “ of the trunk (thought by authors heretofore to
 “ be the mouth) seems plainly to be formed; for
 “ sometimes it is deeper, sometimes shallower;
 “ it is deeper while the moveable globe appears
 “ in the middle of the pinnated part of the trunk,
 “ and shallower when it is in the bottom of the
 “ naked trunk, at which time the bone is most
 “ extended.

“ The fins or pinnulæ have four different motions;
 “ they are moved both towards the naked stem,
 “ and towards the pinnated stem; sometimes they
 “ are drawn in very much to the belly, a little af-
 “ ter they are inclined to the back; further, the
 “ fleshy filaments or claws move in all directions,
 “ and the cylindrical part with the filaments is
 “ either extended out or drawn in and hid in
 “ the fins.” Doctor Bohadsch concludes this
 chapter by observing, that there are some varieties
 to be met with in these red Sea-Pens: some, he says,
 are paler and inclining to a rose colour, others of an
 intense deep red: in the first kind he remarks that
 there are fewer denticles or tentacula (from whence
 the suckers proceed) in the fins, and that these are
 placed in one row within half a line of one another;
 but in the latter, he says, the tentacula are placed in
 a double row and as near as they can be together:
 this is the Pennatula of which I have just now given
 you his account, and which he saw alive in sea-
 water.

water. The other seems to be the same with yours, and is, no doubt on it, Linnæus's *Pennatula Phosphorea*, so that he concludes them to be two distinct species, and calls them by the following names, viz.

Penna (Rubra) pinnis falciformibus, tentaculis in pinnarum facie concava densissime dispositis.

Penna (Rosea) pinnis falciformibus, tentaculis in pinnarum facie concava laxe dispositis.

In the three following chapters Dr. Bohnsch describes three other kinds of Sea-Pens. One he calls *Penna Grisea* or the Grey Sea-Pen with crenated fins; this is figured and described from a dry specimen in Seba's Museum, Tom. III.

The next is a very singular one without fins, having a square bony stem 2 feet 10 inches long, covered with a skin, and furnished on 3 sides with tentacula or suckers: but this was unfortunately broken off at the bottom before he received it: he says, the fishermen call it *Penna del Pescbe de Pavone*, or the Feather of the Peacock-fish. To these he has added the Alcyonium called, *Manus marina*; he calls it *Penna ramosa pinnis carens, tentaculis in ramis dispositis*, and in another place, *Penna Exos*. In order to give you and the rest of the Royal Society some idea of these extraordinary Animals, I have copied his figures, and also the figures of the three last species of Linnæus's *Pennatula*, viz. his *Pennatula (Filosa)* *Pennatula (Sagitta)* and *Pennatula (Mirabilis)* from the authors which he refers us to, and have added an exact delineation of our *Alcyonium (Manus marina)* or Dead mans hand, with some microscopial drawings of different sections of it, to shew that although the substance of it is fleshy, yet that it approaches much nearer to the Madre-

Madrepora Corals, than to any known species of the genus of Animals called Pennatula. — At the same time I allow his remark to be very just, where he observes that the *Hydra Arctica* or Great Greenland Polype, which I have described in the Philosophical Transactions, and in my Essay on Corallines, is certainly a species of Pennatula; but he will find, from both the drawing and description, which I have given of it, that it is not fixt by its base, but floats freely about in the sea; whereas this Alcyonium as well as his (which differs in colour from ours) are always found fixt by their base to some solid submarine body, and consequently cannot be admitted among the Pennatulæ.

I must now conclude this letter with a short account of a new discovered species of Pennatula, which my ingenious friend John Greg Esq; of Charles Town in South-Carolina, discovered on that coast and presented to me some time ago. This beautiful purple animal is of a compressed kidney shape. The body is about an inch long, and half an inch across the narrowest part, it has a small roundish tail of an inch long proceeding from the middle of the body, its tail is full of rings from one end to the other like an Earth-Worm, and along the middle of the upper and under part of it there is a small groove which runs from one end to the other. I examined carefully the point of the tail and could find no perforation in it, which is agreeable to what I have observed in the rest of this genus.

The upper part of the body is convex and near a quarter of an inch thick; the whole surface of it is covered over with minute yellow starry openings, through

through which are protruded little suckers like polypes each furnished with 6 tentacles or filaments, like what we observe on some of the Corals, and seem to be the proper mouths of the animal. The under part of the body is quite flat, this surface is full of the ramifications of fleshy fibres, which, proceeding from the insertion of the tail, as their common center, branch themselves out, so as to communicate with the starry openings on the exterior edge and upper surface of this uncommon animal: for a clearer idea I must refer you to the figure of this, as well as that of your own Pennula, and am,

Dear Sir,

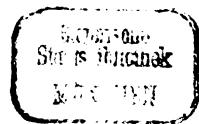
Your much obliged

London, Dec. 15, 1763.

Humble Servant,

John Ellis.

P. S.



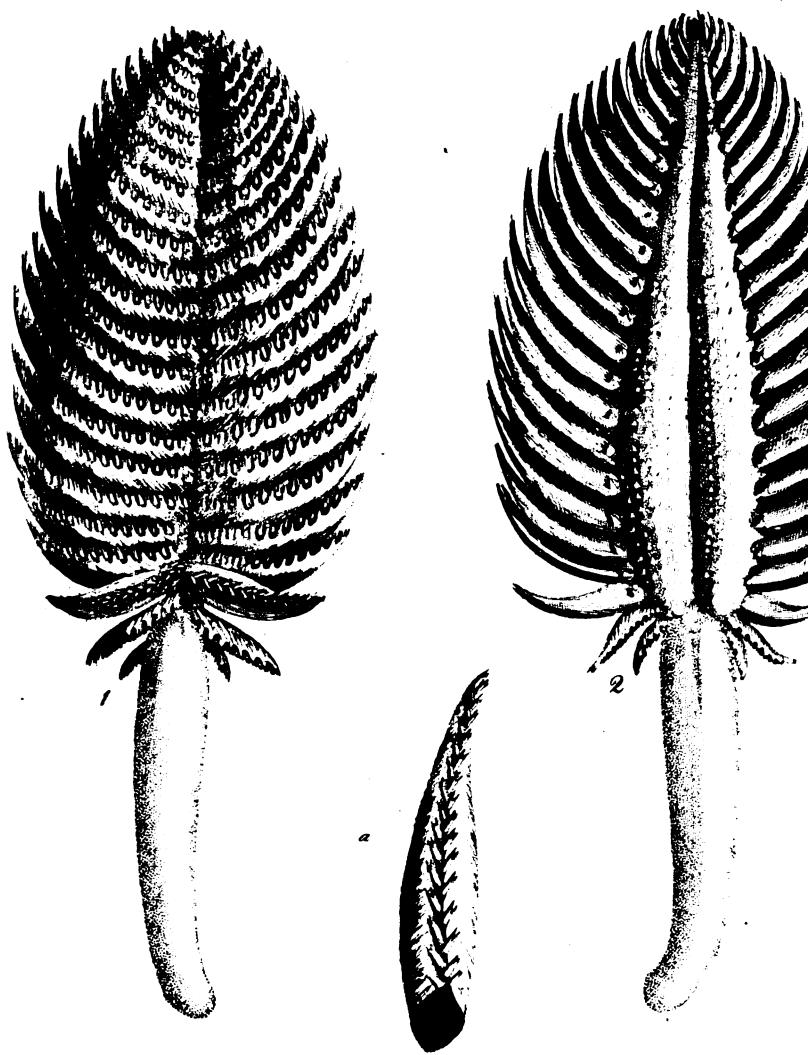


FIG 1. The front of the red Sea Pen.

2. The back of the same.

a. One of the Fins shewing the Order in which
the Denticles are placed.

3. The Finger-shaped Sea Pen.

b. The Fins show that the
extend, or contract this Pen.

P. S. Just when the two plates XIX and XX of the Pennatula were finished, and sent to the Printer, I received three kinds of Sea-Pens, finely preserved in spirits, from my learned friend Thomas Pennant Esq; of Bychton in Flintshire, which he informs me were sent him from the Mediterranean-Sea. One of them is intirely new to me, and, I believe, not yet described ; the other two, which are the Red and the Gray Sea-Pens of Dr. Bohadisch, are so very indifferently designed by the Doctor's painter, and which I have copied in Plate XX, that I thought a better drawing would give you a clearer idea of these strange animals, and be more agreeable to the Royal Society in general.

An Explanation of the Plates.

Plate XIX.

FIG. 1. The back part of the Red-Sea-Pen, or *Pennatula Phosphorea* of Linnæus. This was found on the coast of France, they are frequently met with on the coasts of Norway and Sweden.

2. The front of the same.
 3. and 4. Both sides of the same magnified.
 5. One of the fins more highly magnified, to shew the Polype-like suckers by which it takes in its nourishment.
 6. The kidney-shap'd purple Sea-Pen from South-Carolina in its natural size; this upper part is full

Kk k of

of starry openings, which send out small suckers like polypes by which it feeds.

7. The under part of the same, with its ramifying fibres, that lead from the insertion of the stem as from a center to the circumference, and correspond with all the starry openings on the edge and back of it.
- 8 and 9. Both sides of this animal magnified.
10. A part of the exterior edge higher magnified, to shew the form of the starry openings and suckers, which consist of 6 rays and claws.

Plate XX.

The four following Sea-Pens were found by Dr. Bohadsch, in the Sea, near Naples.

- Fig. 1. Represents the forepart of the Red-Sea-Pen with many rows of suckers on its fins.
2. The back part, the middle of which is covered with the appearance of small papillæ.
3. One of the fins magnified.
4. One of the suckers separated.
5. The bone taken from the internal part of the pinnated trunk ; this is fastened to ligaments at both ends which are likewise inserted in both ends of the animal. When the ligament at the base is contracted, it forms that sinus at *aa*, that has been taken for a mouth by most authors.
6. The Grey-Sea-Pen.
7. One of its crenated fins. *N. B.* There is a figure of this Sea-Pen taken from a dried specimen in the third tome of Seba's Museum.
8. The

8. The Sea-Pen called by the Italian Fishermen *Penna del pesce Pavone*, or the Pen of Peacock Fish ; this appears to be broken off, and is described to be yet 2 feet ten inches long, the square bony part of this is not so hard as that in the Red-Sea-Pen.
9. This last of Dr. Bohadsch's four Sea-Pens is the *Alcyonium* called by authors *Manus marina*, or vulgarly Dead-man's Hand : he calls it *Penna Exos* and the branched Sea-Pen without fins having suckers placed on its branches : but it by no means belongs to this class of animals, which float freely about in the sea ; whereas this adheres to Rocks, Shells, or other marine substances. I have introduced our *Alcyonium Manus marina* or Dead-man's Hand, which is found in great plenty all round the coasts of the British Islands, to shew its internal structure, and how near it comes to the Madrepora Coral, which appears by its growth and form to be produced by animals of the same shape.
10. A piece of the *Alcyonium Manus marina*, cut perpendicular through the middle, to shew that it is formed of tubes, which branch out into others, each ending on the surface in a starry opening of 8 rays ; in each of these openings is a polype-like figure or sucker with eight claws, fastened to the inside of the tube at its lower part by 8 fine tender filaments, by which it can raise or sink itself at pleasure in its tube : all these tubes that compose this *Alcyonium* are connected together by minute reticulated fibres ; these inclose a kind of stiff gelatinous substance, which seems

seems to be the flesh of this compound animal, and these fibres with their inclosed contents to be the muscles; for by the exertion of these it assists in opening or closing the stars on the surface, while the suckers or polype-like figures are pushing themselves out in search of food, or when they are retreating to secure themselves from danger.

11. Is the magnified part of an upright section of this Alcyonium represented in its natural size at *b*. Here the polype like suckers appear in different attitudes; one has extended itself through the starry opening, and is sending forth it's spawn or eggs; at the base of the next sucker you may observe some of the tender filaments by which it is fixt to the bottom of the tube; the sucker next to this is contracted and its starry opening is closed over it; the cell or star next to this is cut in half to shew the manner that the sucker is placed in it.
12. Represents one of these suckers taken out of its cell.
13. Is a cross or horizontal section of a piece of this Alcyonium, the natural size is expressed at *c**.
14. The Madrepora coral is introduced here to shew how near it approaches to this Alcyonium in its external appearance and in the ramification of its tubes.

The

* The reticulated fleshy part of this Alcyonium approaches very near to the nature of sponges; for sponges, when first taken out of the sea, are filled with a gelatinous or mucous matter of a strong

The other 3 figures in this plate are introduced to shew the form of the Pennatula referred to by Dr. Linnæus, in his Syst. Nat. 10 Ed. p. 819.

15. Is the *Pennatula Filosa*, and is figured in Boccone's Recherches, pl. 287, pag. 287. This animal infests the Xiphias or Sword-fish in the Mediterranean-Sea by sucking their blood, and is called by Boccone, *Hirudo cauda utrinque pennata*.
16. Is the *Pennatula Sagitta*; it is described in Linnæi Amœnit's Vol. v. Chin. Lagerstr. p. 14. f. 3. and said to infest the *Lophius Histrio* or Sea-Bat, in the Chinese Sea.
17. Is the *Pennatula Mirabilis*. This is called the *Polypus Mirabilis*, and is described in the Museum of Adolphus Frederick King of Sweden, p. 96. t 19. f. 4.

strong fishy smell : Yet I much doubt whether Sponges have such polype-like suckers as the Corals, Alcyonia, and Pennatulæ, or are even produced by Worms, as the late ingenious Dr. Peysonel informs us ; for in the title to the second part of his manuscript on this subject, which he dedicates to the Royal Society, he says, that Sponges, as well as Corals, Madreporas, &c. are produced by animals that are of a particular species of Urtica marina or Purpura ; but I am inclined to believe he took this for granted from the similitude they bear to Corals, Alcyonia, &c. rather than from actual experiment. I rather take those holes, which I have observed in them, to be so many mouths upon the surface of the animal ; and I am the more inclined to believe it, from a remark I made with Dr. Solander at the Sea-Coast of Sussex in the summer 1762, on the *Spongia Medullam Panis referens*, while it was in a glass vessel of Sea-Water ; where we observed, that the Mamillæ that were on the surface opened and shut, but that no sucker or animal-like figure appeared to come out.

Plate

Explanation of Plate XXI.

Fig. 1. Represents the front of the Red-Sea-Pen a little larger than life, as are the figures of the two following Sea-Pens.

- a. One of the fins shewing, the alternate order in which the denticles incline like the teeth in a saw.
2. The back of the Red-Sea-Pen, with the rachis or middle part between the fins covered over with a rough skin like shagreen.
3. The finger-shaped Sea-Pen, or *Cynomorion*, called so from its likeness to the shape of the *Fungus Melitensis*.
The upper part of this animal is covered over with circular cells, one of which is represented at Fig. 4, from whence proceed Polype-like suckers, having eight pennated arms or claws, one of which suckers is figured at 5.
The Rugæ or Furrows in the swelling part at 6, shew that this animal can extend and contract this part, perhaps to raise or fall itself in the sea.
6. The front of the thorny Sea-Pen, called by Dr. Bohadsch *Penna grisea*.
7. The back part of it.
8. Shews the front of one of the suckers magnified.
9. —— the back view of the same.
10. One of the lower fins a little magnified, which shews the position of the suckers, and the

the insertion of the spines; These spines are combined of many fine spiculæ, which unite and form one spine.. When these spines open at top, each forms a star of small spiculæ, which nature seems to have pointed out as a protection for the mouths or suckers underneath, which have no other covering to defend them, whereas in the Red-Sea-Pen there is a circle of spiculæ to each sucker..

LIV. A Letter from Mr. B. Wilson, F. R. S.
 and Member of the Royal Academy of
 Sciences at Upſal, to Mr. Æpinus, Pro-
 fessor of Natural Philosophy in the Imperial
 Academy of Sciences at St. Petersburg, and
 Member of the Academies of Berlin, Stock-
 holm, and Erfurth.

Read Dec. 23, 1763,
 and
 March 1-64. I Have not had the favour of hear-
 ing from you since I sent you
 some experiments upon *Gems* ſimilar to those pro-
 duced by the *Tourmalin*, which induced me to con-
 clude, that the electric current moves always along
 the grain thereof. It was the more agreeable to
 communicate these new experiments to you, be-
 cause from the least hints the greatest discoveries
 have been made; and what may we not expect
 from that curious observer of nature, who first dis-
 covered these extraordinary qualities in the *Tour-
 malin*, which have ſince excited the attention of ſo
 many Philosophers.

Your treatise upon this ſtone, published in 1762,
 seems to be the ſame you formerly mentioned in a
 letter to me. The remarks at the end of that work
 interest me in a particular manner, as they contain
 objections to ſeveral parts of the letter to Dr. *He-
 berden*. I am obliged therefore to ſay ſomething
 in the defence of my own experiments and deduc-
 tions, which I hope will merit your attention, and
 remove your difficulties.

In

In repeating such of the experiments with the *Tourmalin* as were most proper to answer your objections, I accidentally observed an appearance which has given rise to some new experiments, very simple, and of great consequence in electric researches. These discoveries you shall be acquainted with, before I conclude this letter, that I may have the pleasure of hearing your sentiments concerning them, the sooner.

I am glad to find we * agree in admitting what are called the two species of electricity, one whereof consists in the augmentation of the electric fluid, and the other in it's diminution. But still, notwithstanding the experiment made with the *bent tube* in order to determine that interesting question, you seem to doubt which of these is the *plus*, and which the *minus* electricity.

You say † I have proved the opposition of the two species, by the knobs of light appearing at the upper

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* Mr. Wilson reconnoît de même que moi, l'existence de deux espèces d'électricité, dont l'une consiste dans l'augmentation, et l'autre dans la diminution, de la quantité naturelle du fluide électrique. C'est une question qui doit intercesser chaque physicien, de demêler, quelle de deux électricités, ou la vitrée, ou la résineuse de Mr. du Fay, est la positive, et quelle en est la négative? Quant à moi j'ai déclaré, il y a long tems, dans mon *Sermo de similitudine electricitatis et magnetismi*, que je ne connois aucune expérience, propre à décider cette question. Je suis maintenant encore du même sentiment, car la belle expérience de mylord Charles Cavendish, rapportée ici et perfectionnée par Mr. Wilson, me semble laisser pareillement la chose indécise.

† Quant à l'opposition des deux espèces d'électricité, je conviens qu'elle est fort clairement prouvée par l'expérience de Mr. Wilson, et par ce phénomène, que la lumière électrique est beaucoup plus vive qu'ailleurs dans le vuide, aux surfaces supérieures des colonnes du mercure, (jusqu'à y former comme des boutons

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de

per surface of the quicksilver, when one electrifies with glass ; and at the under surface, when one electrifies with wax, or sulphur ; but that, in your opinion, is all which can be concluded from it. You then declare, that was you disposed to argue against me, it would be in this manner. " It is easy to

de lumière) quand on électrise avec un tuyau de verre ; et que ces boutons se trouvent au contraire aux surfaces inférieures du vif argent quand on se sert d'un bâton de cire d'Espagne ou de soufre. Mais selon moi c'est tout ce qu'on en peut conclure. Voila comme je raisonn erois, si j'avois envie de disputer contre Mr. Wilson. Il est facile de concevoir, que, quand un fluide (et sur tout un fluide élastique, ou dont les parties se repoussent mutuellement) sort d'un corps, le fluide, dis-je, doit être plus dense proche de la surface d'où il sort, que là, où il trouve plus de liberté de se répandre. La surface donc, proche de laquelle la lumière électrique est la plus vive, doit être celle, de laquelle sort le fluide, qui cause ces apparences lumineuses.

Ces raisonnemens, n'ont ils pas autant de probabilité, que ceux de Mr. Wilson ? Mais ne prouvent-ils pas directement le contraire de ce que Mr. Wilson a avancé ? Ne prouvent-ils pas, dis-je, que l'électricité résineuse est la même que la positive, et la vitrée la même que la négative ?

Pour dire le vrai, le raisonnement dont je me suis servi, me semble presque avoir plus de vraisemblance, que celui de Mr. Wilson. Il est incontestable, que la matière électrique entre très librement, et sans éprouver une résistance considérable dans les metaux et autres corps non électriques, comme je le prouverai clairement tout à l'heure. Le raisonnement de Mr. Wilson semble, par consequent, être fondé sur une hypothèse gratuitement admise, savoir, que la matière électrique s'accumule à la surface du vif argent, parce qu'elle n'y peut entrer librement.

Je ne pretends pourtant pas prouver par tout cela, qu'effectivement l'électricité résineuse soit la même que la positive, et la vitrée au contraire la même que la négative. Mon unique but est de faire comprendre, que cette hypothèse est au moins aussi probable, que celle, pour laquelle Mr. Wilson s'est déclaré.

“ conceive that when an elastic fluid issues from a body (as from the quicksilver in the bent tube) it will be denser at the surface from whence it issues, than it is where it finds more liberty to expand itself. And therefore the surface near which the electric light is the brightest, should be that from whence the fluid issues, which causes those luminous appearances.”

This reasoning appears intirely new to me, and I am at a loss to comprehend why an elastic fluid confined within a tube, whose sides are supposed parallel, will be denser at the surface of a body from whence it issues, than in any other part; since its expansive force must in that case be limitted by the sides thereof. As you have not given any particular experiment to prove what you assert in this case, you will therefore give me leave to differ from you in opinion. If, as you say, there is really more liberty for an elastic fluid to expand in any other part of the exhausted tube, than at the surface itself, you must produce some evidence in favour of that opinion, before it can be admitted. On the other hand, I should have thought, if all *resistance* is supposed to be removed from within the tube, the liberty, for the fluid to expand itself, will be equal in every part; reckoning from the surface of one column of quicksilver through the whole void, to the surface of the other column of quicksilver.

As I am for supporting this opinion, let us examine it more particularly, and attend only to the appearances which glass affords in certain circumstances: because when the direction of the fluid,

caused by glass, is once traced, that which is caused by wax, amber, or resin, follows of course. The electric fluid, when it is emitted from any smooth surface of metal without edges, or angles, appears, in certain circumstances, to issue from all parts of that surface equally. This fact, I apprehend, is so well established that it needs no further proof.

Now the column of quicksilver being confined by the sides of the glass, which are supposed parallel, the top of the quicksilver will answer to the smooth surface described. The electric fluid therefore that is to pass from it, into the void space, which is of the same diameter with the column of quicksilver, will move forward within the hollow of the tube to the next column of quicksilver. And since no resistance is supposed to be within any part of the vacuum, there can be no cause for any accumulation: consequently when the fluid is suffered to pass along the tube, the appearance ought to be the same at the surfaces, that it is in every part of the void space. But by my experiment there is a greater quantity of light seen at the second surface of the quicksilver, than in any other part, (when polished glass electrifies the first column) and that this light which appears so dense, extends itself about one tenth of an inch from the surface. Whereas the light extending all along the intermediate hollow of the tube, appears to be much thinner, and rarer, and of an uniform density. I conclude therefore that this luminous accumulation at the second surface is caused by a *resistance* exerted at, or near, the *surface* of the quicksilver: when the electric fluid, issuing from the

the glass that electrifies it, is pushing forward to enter the second column of quicksilver.

At the time I related this experiment with the bent tube in the letter to Dr. *Heberden*, I omitted certain phænomena, which attended the experiment, greatly favouring the doctrine here advanced. If when glass is electrified, and applyed to the first column, we suffer the electric fluid to pass along the tube in small quantities only, and at short intervals, little luminous streams will be seen to move from the first to the 2d column of quicksilver, and consequently from the glass. The like appearances happen, but in a contrary direction, when resin or amber is made use of, and applyed to the same column. Glass therefore electrifies *plus*; or fills bodies with more of this fluid than belongs to them naturally: and resin, &c. *vice versa*.

When you say, your reasoning appears to have as much probability as mine, I believe you do not include your observation, that the electric fluid enters with great freedom, and without *any considerable resistance*, into metals and other non-electric bodies. Because the words *any considerable resistance*, imply *some resistance*, which is all that is contended for: and a very small *resistance* will occasion very extraordinary appearances, as I shall be able to shew by and by. There is no occasion to trouble you with any further arguments to prove this *resistance*; of which yourself seem to entertain no doubt; and the *accumulation* caused by the *resistance* is evident.

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In your second remark, respecting the *impermeability of glass*, you say you agree with Mr. Franklin * as to the

* Je me tourne vers un autre objet, sur lequel, il me semble, que Mr. Wilson a eu envie de savoir mon sentiment. C'est la question de l'imperméabilité du verre. On peut bien savoir par mon livre : *Tentamen Theoriae Electricitatis et Magnetismi*, ce que j'en pense, et que bien que je tombe d'accord avec Mr. Franklin, de l'existence de cette imperméabilité, je différe pourtant beaucoup de lui, par rapport à plusieurs autres points. Il ne seroit donc pas à la vérité nécessaire, d'exposer ici de nouveau mon sentiment, néanmoins je me charge de ce travail, pour ne laisser rien à desirer à ceux qui liront ce recueil.

Qu'on suspende un fil de fer ou d'archal, quelque long qu'il soit, à des fils de soye, et qu'on en électrise un bout par le moyen d'un tuyau de verre, ou d'un bâton de cire d'espagne. Dans moins d'un clin d'oeil non seulement le bout qu'on électrise, devient électrique, mais aussi le fil entier le sera d'un bout à l'autre, et le sera partout également. Qu'on touche après l'un des bouts, et l'électricité fera détruite dans tout le fil, d'un bout à l'autre, avec la même vitesse, qu'elle avoit été produite.

Qu'on suspende au contraire de la même façon un tube de verre bien sec, ou un cylindre de cire d'espagné ou de souffre, et qu'on le traite de la même manière. Le succès en sera tout à fait différent. Ce n'est pas le cylindre entier, qui devient alors électrique dans un instant, mais seulement une partie de la longueur de quelques pouces, ou d'un pied tout au plus, acquiert cette force, et il faut travailler fort long temps, si on veut amener les choses, au point d'en rendre électrique une partie d'une longueur un peu considérable. Qu'on touche après la partie électrisée du tuyau, et l'électricité ne sera détruite, ni dans un instant, ni dans le tuyau entier, comme dans l'expérience précédente. Au contraire, encors que l'endroit touché perde son électricité, il n'en sera de même, des parties tout proches, qui conserveront plutôt leur électricité pendant fort long temps.

J'en tire la conclusion : que la matière électrique, traverse les métaux ou d'autres corps non électriques et se distribue en eux fort facilement et fort rapidement, mais qu'au contraire elle passe par le verre, la cire d'espagne, et d'autres corps électriques par eux mêmes beaucoup plus difficilement et plus lentement. Cette règle ne doit pas.

the existence of this impermeability, though you differ from him in many other points: and refer to your Essay upon *the Theory of magnetism and electricity*. You then relate two experiments, one with a wire, and the other with a glass tube, or cylinder of wax: and observe that the first may be easily electrified, and the latter also, though with great difficulty, to any considerable length. You then draw this conclusion from the two experiments, that the electric matter pervades metals and other non-electric bodies, and expands itself in them with the greatest ease and rapidity. But that on the contrary *it passes through glass, wax, and other electric bodies, more slowly and with much greater difficulty.*

This conclusion, instead of establishing the impermeability of glass, most evidently affirms the contrary: for though, according to your observation, the fluid passes *more slowly and with much greater difficulty through glass than iron*; your admitting *that it does pass at all through the glass*, ends the dispute, as to the point of permeability: and at the same time establishes my doctrine of resistance; at least in glass, and resinous substances.

In regard to my experiments upon the *Tourmalin*, you say * that the first and second correspond with your discoveries.

pas être prise pour une hypothèse. C'est une loi, prise immédiatement et d'une manière incontestable, de l'expérience.

Cette propriété des corps électriques par eux mêmes, est selon moi la même que celle, que Francklin a appellée d'un autre nom, *imperméabilité*. Au moins, je ne fais confister l'imperméabilité en rien autre chose, qu'en cela.

* Je viens aux expériences de Mr. Wilson. La première et la seconde s'accordent tout à fait avec mes découvertes. Il n'en

discoveries ; but that those from the 3d to the 8th do not agree with your notions and experience : and then

est pas de même, de celles qui suivent, depuis la troisième jusqu'à la huitième, qui demandent d'être discutées avec plus de soin.

J'ai établi comme une loi constante, de l'électricité de la Tourmaline : *Que cette pierre est toujours dans l'état contraire (c'est à dire, que son côté positif, est négativement électrique, et le côté négatif l'est positivement) quand un de ses côtés, quel qu'il soit, est plus chaud que l'autre ; et qu'elle ne retourne dans son état naturel, qu'après que la chaleur s'est distribuée uniformément en elle.* Par les expériences de Mr. Wilson au contraire, il faut établir une règle tout à fait différente, savoir : *Que la Tourmaline, quand elle chauffée inégalement, des deux côtés, a l'espèce d'électricité, qui est naturelle au côté le plus chaud (c'est à dire, que la Tourmaline est positivement électrique des deux côtés, quand c'est le côté positif, qui est le plus chaud ; et qu'elle l'est négativement, quand c'est le côté négatif, qui est le plus chaud) mais qu'elle retourne dans son état naturel, quand la chaleur s'est répandue uniformément.* Cette règle ne s'accorde en aucune façon avec celle que j'avois avancée.

Encore que je fusse tout à fait convaincu de la justesse de mes assertions, et de leur parfaite conformité avec l'expérience, je ne pouvois pourtant que fort difficilement me résoudre, à douter de l'exacitude des expériences faites par un aussi habile Observateur, que Mr. Wilson. J'ai donc mieux aimé supposer, qu'il y avoit, dans la manière, dont Mr. Wilson et moi avions procédé dans ces expériences, quelque différence, qui pût être la cause de la différence qui se trouve dans nos résultats. Je croyois à la vérité de trouver facilement une circonstance sur laquelle on pût fonder quelque soupçon. C'est que moi j'avois toujours posé la Tourmaline, ou sur un charbon ardent, ou sur une plaque de métal ou de verre échauffée, de façon, que l'un des côtés de la pierre, avoit toujours touché à quelque corps. Mr. Wilson au contraire, en échauffant la Tourmaline, y a procédé tellement que la pierre n'a jamais touché à quelque corps. Voilà une circonstance, qui semble assez importante, pour avoir pu causer une différence, sensible. C'étoit à l'expérience d'en décider.

C'est

then declare that you have established this, as a constant law, namely, that the *Tourmalin* is always in an inverted state; that is, its plus side is electrified minus and the minus side plus, whenever either of the sides is hotter than the other: and that it does not return to its natural state, until the heat is distributed uniformly therein. My experiments on the contrary, you observe, lead one to lay down a rule intirely different. viz. That the *Tourmalin*, when its sides are unequally heated, exhibits the species of electricity which is natural to the hotter side, that is, the *Tourmalin* is plus on both sides, when the plus side is the hotter; and minus on both sides, when the minus side is so: but that it returns to its natural state also, when the heat is uniformly distributed. To account for these different opinions, you think it more agreeable to suppose some difference in our methods of making the experiments, than to question the facts I have declared. Accordingly you observe one circumstance which would naturally give rise to such a suspicion. And then tell us, that you always placed the *Tourmalin* upon burning coals, or upon a plate of metal, or heated glass: so that one side of the stone was always in contact with some (*non electric*) body. That I, on the contrary, in heating

C'est elle qui me force et m'autorise à declarer, que la regle, que j'ai avancée, est la seule, qui lui soit conforme. J'ai échauffé la *Tourmaline* de la même manière que Mr. Wilson, et j'ai pris garde, qu'elle ne touçhat à rien, mais pour le resultat, je l'ai, non obstant cela, toujours trouvé conforme à ma regle, et pas une seule fois à celle de Mr. Wilson.

Je le repete encore; il m'est extremement difficile, de supposer, que Mr. Wilson soit tombé ici dans une méprise, néanmoins je suis convaincu, que de ma part, il n'y a assurement, aucune faute. Ainsi je ne fais qu'en juger.

the *Tourmalin*, never suffered it to be in contact with any (*non electric*) body.

Here, say you, is a circumstance that seems of consequence enough to cause a sensible difference; and then you appeal to the experiment which is to decide it: adding at the same time, *it is experiment that obliges and authorizes you to declare, that the rule advanced by you, is the only one that agrees therewith*. Now your experiment tells us no more, than that you heated the *Tourmalin* in my manner, taking care (as you express it) that it was in contact with *nothing*. But notwithstanding this, that you have always found the event agreeable to your rule, and not one single time to mine.

From your description of this experiment, your exact method of making it does not appear. I am inclined to believe some material circumstance has been omitted in the method; or that our apparatus's are essentially different; (though you seem to have had a regard to some of the necessary requisites for making the experiment properly;) otherwise, I cannot apprehend why you were not able to succeed: because the experiment always answers with me. Perhaps the *difference in the size of our Tourmalins* may have contributed towards causing the different effects. The *Tourmalins* I employed in the experiments with the flame, are *above five times larger than yours*: and must therefore require a longer time in heating. Now the degree of heat given to my *large Tourmalins*, was much below that of boiling water; and therefore I suspect, that the *small Tourmalins*, you employed, were made *better*; whereas they ought to have

have been at the most, but *about blood warm.* For since a difference of heat, between the two sides, is absolutely necessary, it is of importance that this difference be increased as much, and as suddenly, as the nature of the circumstances will admit.

If you follow this rule, I apprehend you will be more likely to succeed: but that you may be the better enabled to make my experiments answer, it may not be amiss to relate them again in the manner they were lately repeated, and frequently, with two different Tourmalins, before several members of the Royal-Society of London, who are well acquainted with enquiries of this kind.

E X P E R I M E N T.

After the convex side of the Tourmalin (*b*, fig. 1. Tab. XXII.) has been held for a short time, about one tenth of an inch, from the flame of a candle, both sides thereof are electrified plus: and continue so for half a minute or more. And in a short time after, the same Tourmalin, without being heated a fresh, or disturbed by any other cause than that of the air surrounding, returns to its natural state; as you have called it; that is, the plain side changes to minus, and the convex side remains plus. These appearances continue whilst the Tourmalin is cooling.

Upon heating the Tourmalin again, as before, excepting that the plain side was now next the flame, both sides thereof were electrified minus: and continued so, for half a minute or more; and in a short

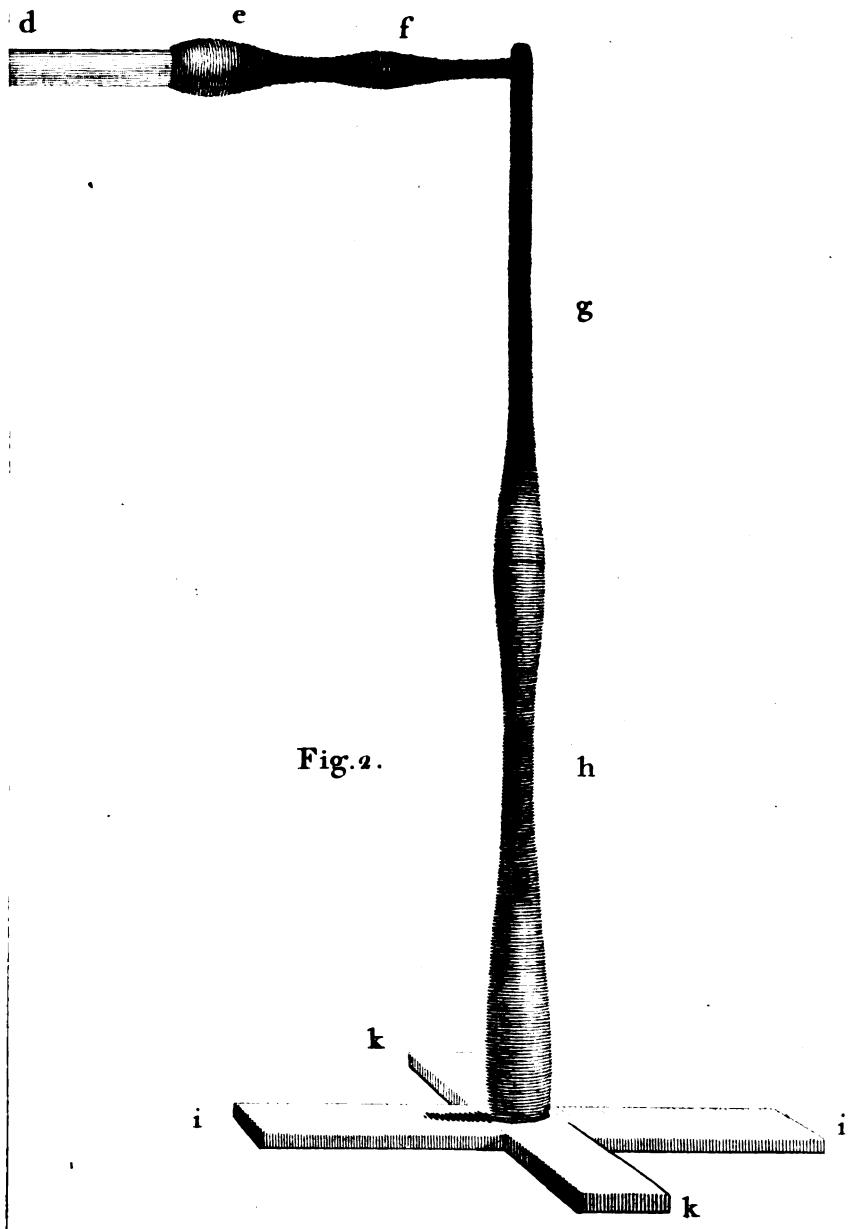
M m m 2 time

time after, the same *Tourmalin*, without being heated afresh, or disturbed by any other cause but that of the air surrounding, returned to its natural state again; that is, the *convex side* changed to *plus*, and the *plain side* remained *minus*.

One of the *Tourmalins* that were employed in these experiments, (*marked b*) belongs to Dr. *Heberden*, and is the same I formerly made use of: but the largest of them was put into my possession by the favour of Dr. *Morton* Secretary to the Royal-Society, of *London*, who received it, with several others, from Mr. *Loton F. R. S.* late governor of the Island of *Cylon*. This stone weighed about one hundred and eighty grains: but the form thereof not being the most favourable for experiments, I had permission to fashion it into such shape as would answer the purpose best. For this end, alterations were made therein; and at, one time, the electric poles (if they may be so called) were pretty near in the longest direction of the stone, and at another time, in the shortest. These trials terminated in the form and size, nearly, as represented by the letter *a*, fig. 2: and the direction of the poles is now in the shortest line that can be drawn between any of the opposite sides.

I have to observe, that when this *Tourmalin* is held between the eye and the light, and viewed in the direction though which the electric fluid is found to pass, it appears of a *darker* colour considerably, than when it is viewed at right angles to the former direction. This appearance obtains in many other *Tourmalins*, especially if they happen to be as conveniently shaped.

In





In regard to the other experiments with the *heated iron*, and *Tourmalin*, which you have only hinted at, it is enough to recommend the utmost care, to avoid the least degree of friction; and the iron itself must be so managed, that there may be *as easy an electric communication with the air, from it, as the flame of a candle is found to have.*

Give me leave, in this place, to make some observations on the *apparatus* you have made use of in these experiments, as it does not in my opinion, seem the best calculated for such nice purposes; at least in our climate, which is certainly more moist than yours.

The instrument first described, * consists of a *balance* 12 inches long, fixed upon a small stand; from one arm of which, that moves upon a hinge, is suspended, by a silk string, a small cork ball: but there is

* Le premier de ces instrumens est représenté par la Fig. I. Sur un pied quarré A B. je fis dresser un bâton C D. environ d'un pied de hauteur. Ce bâton avoit au haut une charniere près de D. au moyen de laquelle le lévier à bras égaux F G. étoit un peu mobile. Chaque bras de ce lévier F D. et D G. avoit la longueur de 7. pouces à peu près. J'employai cet instrument pour pouvoir suspendre commodément le pendule H I. Le lévier F G. étoit un peu mobile, afin de pouvoir au besoin hausser, ou baisser un peu le pendule. Pour faire le pendule H I. je pris un fil de soie cruë, j'y attachai par le bas un petit morceau de liège arrondi de la grosseur à peu près d'une lintille. La longueur du fil H I. étoit de 5 à 6. pouces environ, et j'attachai le pendule avec de de la cire à un des bras du lévier F G. Sur un autre petit pied quarré M N, qui est représenté dans la Fig. II. je mis dans le milieu un tube mince de verre O P, à peu près de 6. pouces de longueur, dont le haut se terminoit dans un hémisphère O R, de deux lignes environ. Il est très-facile de faire ce tube en soufflant une petite boule de verre, comme on fait en faisant un Thermomètre, et en brisant ensuite cette boule jusqu'à la moitié. Je me sers dans les expériences de cet instrument pour y poser la *Tourmaline*, afin qu'elle puisse agir librement.

nq

no mention of what the *stand* or *ballance* is made; though a difference in the materials will, I apprehend, make some difference in the experiment. And if the cork is, as you say, the size of a pea, it will be a considerable objection with me: and the more so, if it is to be moistened with water. For the force employed to move it, in some of my experiments, is too inconsiderable. And I should imagine the *ballance* itself could not be very nicely adjusted; because such an increase in the weight of the cork, by moistening it (as you say) from time to time, ought to make it not only preponderate, but unsteady; as an evaporation of the moisture is constantly making some alteration in the weight. My *apparatus* contrived for the same purposes, hath already been described, except that part of it which respects the size of the pith balls, and each of these are about the *thirtieth part of a pea*. This smallness, nevertheless, does not require any moisture to retain the electric fluid, as the balls communicate with the finest flaxen threads, and the threads with a slender piece of wood, about one inch long, and the greater part one tenth of an inch thick; with the angles rounded off, and polished. This is fixed upon the end of a cylinder of amber, properly supported. But that there may be no mistake in the construction thereof, I have given a drawing and description of the one I use at present, see fig. 2. When I at any time want to examine the state of the *Tourmalin*, it is brought slowly towards the electrified balls in an horizontal direction.

Your *glass stand* seems liable to an objection, unless the air be extreamly dry: because the mere breathing, if not properly guarded against, in very delicate experi-

experiments, is sufficient to cause an unfavourable alteration; by the moisture condensing upon it.

We are not told of what materials your pincers* were made, but I suppose of some electric substance; otherwise in removing the *Tourmalin* from time to time, they may interrupt the experiment, by conducting away the fluid. And even supposing them made of an electric substance, the unavoidable friction may possibly disturb the experiment. I have therefore always preferred a different method, which will appear presently.

I shall now comply with the promise I made in the beginning of this letter, to acquaint you with some simple and interesting experiments upon the *Tourmalin*: most of them were made during the frost in Nov. last, in consequence of an appearance which I then observed accidentally.

With me it has been always found most convenient to fix the *Tourmalin* at the end of a long stick of the hardest kind of Sealing-Wax, and when I am not using it, to put the other end into the top of a candlestick, or other suitable stand; that the stone may be the less exposed to any kind of friction (See fig. I.) And it is a rule with me never to take hold of the Sealing-wax, or even to touch it, but by that end (d) the farthest from the *Tourmalin*. One day on

* J'emploie encore de petites pincettes A B C, pour ne pas toucher la *Tourmaline* avec les doigts, et je la prends toujours par les côtés, comme il est indiqué dans la Fig. III. afinque la pierre soit touchée le moins qu'il est possible par des corps non électriques. Il faut encore avoir un Tube de verre et un bâton de cire d'Espagne tout prêts, pour qu'on puisse examiner de la manière que nous décrivons plus bas l'espèce d'Electricité produite par la *Tourmaline*.

removing

removing the *Tourmalin* into another room, to repeat the experiments we have here been treating of, I observed it was electrified; though no cause for its being so then appeared: the friction arising from the air, in such circumstances, being not sufficient to produce that effect. And I must here take notice, that many months before this fact was ascertained, I frequently suspected the like appearance: but it happened at those times that the effects were so uncertain, and appeared so accidental, that I did not think they deserved attention †.

In tracing out the cause of this appearance, it seemed most necessary to observe the changes in the air with respect to warmth: for, it is well known, those changes cause manifest alterations in the *expansion* and *contraction* of bodies.

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

In a room with a south aspect, and where no fire had been for sometime, *Farb. Therm.* stood at 42. The *Tourmalin* was in the same room, and had continued there some hours, undisturbed, without shewing any signs of electricity. On removing the *Tourmalin* into a warm room carefully, and the *Therm.* along with it, in less than 3 minutes (the *Therm.* having risen to 47) the convex side of the stone shewed

† *N. B.* The experiments upon the *Tourmalin*, by *Mr. Epinus*, which respect the heating and cooling of its sides *equally*, when occasioned by *violent and artificial means*, are, it is apprehended, very different from the ten following experiments; tho' they also respect an *equal* heating and cooling of the stone; because the *degree of heat* employed, is not only extreamly different, but the *means of obtaining it*, is so likewise. The one being *natural* and the other *artificial*.

a minus

a minus, and the *plain side a plus*, electricity. These signs increased for a time, and then decreased, till they entirely disappeared. When this happened, the *Tourmalin* appeared to be of the same temper, in respect to warmth, with the room; and the *Therm.* was raised to 58.

The same degree of warmth, or nearly so, was continued in the room for thirty minutes and more, without causing any alteration; for the *Tourmalin* afforded no electric appearance whatsoever.

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

I then removed the *Tourmalin* with the *Therm.* into the *cold room* and with equal care*. The stone, some little time after, shewed signs of electricity again: but then, those signs were *contrary* to what had been observed before. For, in this case, the *convex side was plus*, and the *plain side minus*, in nearly the same time; and these signs increased also for a time, and then decreased, till they entirely vanished. When that happened, the *stone* appeared also of the same temper, in respect to coldness, with the room. And the *Therm.* was fallen to about 42°. During half an hour, or more, after that time, the *Tourmalin* shewed no electric signs whatsoever, nor did the *Therm.* fall any lower.

* It is indifferent which side of the *Tourmalin* is moved foremost, provided it be done *slowly*.

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

Not content with this last discovery, I removed the *Tourmalin* and *Therm.* into the *open air.* In about three minutes, the *Therm.* having fallen from 42 to 39, the *Tourmalin* was electrified again, in the same manner as in the last experiment. But when the electric signs disappeared, the *Tourmalin* and the air were in this case also of an equal temper: at which time the *Therm.* had fallen to 34. The *stone* was continued in the open air for half an hour or more, but no further electric signs appeared.

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

On returning into the room where the first experiment was made, the electric signs were stronger than in any of the preceding trials, and contrary to the two last; for the convex side was *minus*, and the plain side *plus*, which agrees with the appearances in the first experiment.

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

When the *Therm.* shewed the state of the outward air to be considerably less warm, than what answers to the degree at which water is fixed, the same changes happened, by carrying the *Tourmalin* from it, into a room, where the *Quicksilver* stood at 34: and afterwards, from thence, back again into the open air.

Sir

Sir Isaac Newton carried two Thermometers, properly prepared, out of a cold place into a warm one, in order to shew that the warmth was conveyed through the *vacuum*, by the vibrations of a much subtler medium than the air : and had these last experiments upon the *Tourmalin* at that time been known to him, he must have been agreeably surprised to find them tending so strongly to establish the existence of that *subtile medium**. This doctrine receives a further confirmation from the experiments that follow.

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

About the middle of this month, December, the wind being full south, and the air loaded with a thick fogg, which you know is the worst of weather for electric experiments, the *Tourmalin* afforded the same appearances as before, by removing it from one room to another ; and even into the open vapourous air ; notwithstanding the unfavourable season : but then, the appearances were weaker.

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

In the most wet season, and during frequent heavy showers of rain, I repeated the first, second, third, and fourth experiments. And though the electric power was not very strong, yet they always succeeded so well as to ascertain the facts.

* See Newton, Opt. page, 323.

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

After being acquainted with the preceding experiments, you will not wonder that the *Tourmalin* afforded the same appearances on removing it, in the open dry air, from the sun-shine into the shade ; and again, from the shade into the sun-shine.

If these small differences, in the degrees of warmth, are capable of causing such appearances ; well may the greater differences ; and such more particularly as Mr. *Brown* and *yourself* have experienced in freezing of Quick-silver : and therefore *I cannot now agree with you in calling that the natural state of the Tourmalin, which arises from the heat given it by boiling water.*

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

It appears by the preceding experiments, that when the *Tourmalin* was of the same temper with the air in the different rooms, there were no electric signs to be observed. From which we may understand, if the heat of the air should be increased, even beyond that of boiling water, a *Tourmalin* exposed therein for a time, would afford no electric signs ; that is, whilst the *stone* continues of the same temper with the air. I have lately caused the heat of the air to be increased, in a convenient room, beyond the degree of vital heat, even to 108 : and then placed two *Tourmalins*, in the same room, very near the *Thermometer*, without being able to observe any electric effects ; that is, after they had remained therein a short time.

E X-

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

Upon a very nice examination, and during some favourable opportunities, *I have observed the Tourmalin to be feebly electrified, when the Therm. varied, up or down only one degree.*

The smallness of the force here required to cause those manifest effects, and even them by *natural means* only, is a new discovery, and, perhaps, deserves the attention of philosophers.

In my first and second letters upon the *Tourmalin*, there are experiments that give us assurances of a *flux and reflux* of the electric fluid, or *aether*, at different times, even without artificial means to occasion it. And I did not scruple to advance that doctrine, as appears from a passage in the opticks which I quoted in the second letter, somewhat favouring the same opinion. This you see has happened to be a right conjecture; for these last experiments, are I apprehend, so clear and satisfactory, that there is no room left for a doubt about it. And I do hope they will lead to some useful discoveries. For these forces, however small they may appear, are probably sufficient to answer very great purposes in nature.

Upon considering the effects which heat and cold occasion in the *Tourmalin*, it may not be improper here to observe, that all bodies we are acquainted with, are *dilated* by heat, and *contracted* by cold: and when they acquire the same temper with the air, whether it be hot or cold, the same state of dilatation, or contraction, continues unaltered.

The

The *Tourmalin* we find, when uniformly disturbed on all sides, by changing the temperature of the air ; is not only electrified, but shews two opposite and contrary effects. That is, in passing from a great, to a less, degree of warmth, it is electrified in one manner ; and in passing from a less to a greater degree of warmth, it is electrified in another manner ; which evidently shews, that there is some *power*, belonging to the stone, which is differently affected by such contraction and dilatation. The same thing appears from other different effects it affords, when its two sides are *equally* warm. But the *Tourmalin*, affording no electric appearance whatever, when the whole mass is of the same temper with the air, agrees with the observation that *all bodies cease in that state, to contract or dilate* : and is a manifest indication, that the *fluid*, which causes these electric appearances, is in such circumstances, in *equilibrio* ; and must ever remain so, unless disturbed by violence.

The importance of this last observation bespeaks your attention, as it greatly tends to throw more light upon this curious subject.

From the experiment with the bent tube, mentioned in the former part of this letter, it was proved, that there is a *resistance* exerted at, or near, the surface of the Quicksilver where the light is accumulated. This *resistance*, which I apprehend is essential to all bodies, merits a further illustration ; because the electric phænomena in general greatly depend upon it.

When a *bladder* is well blown up, and secured properly, it will yeild or give way, and change its form, in that part against which any given pressure is exerted.

exerted. And upon removing the pressure, the bladder will immediately recover its first form.

This yielding or giving way of the form, and then afterwards recovering it, proves an elastic substance existing within the bladder, and between the two sides where the pressure is employed.

In like manner when two glass prisms, or the object glasses of two long Telescopes press upon each other with their own weight only, philosophers know, by the phænomena of light, that they do not touch : and that there must be something between the glasses to keep them at a distance. They also know, by the like phænomena, that more pressing is required to bring them nearer to a contact ; and that when the pressure is removed, they immediately recover their first distance.

Now this yeilding or giving way, and the recovery of the distance between the glasses, proves the existence of some *elastic substance* between them respectively. Since we find the effects of applying, and removing, the pressure, exactly similar to the case with the bladder.

Hence it is evident that the same *elastic substance* or *medium* causes prisms, and convex glasses, when pressed against each other, to exhibit several rings of different colours ; by having its density varied : and that it occasions all bodies to act upon light at a distance, by reflecting, refracting, and inflecting it ; and light [to act upon bodies, at a distance, by causing a motion of their parts, and heating them.

This is the medium then which gives rise to the resistance found in electric experiments.

For

For when a quantity of the electric fluid is forced into the *apparatus*, which supports the two balls, we should from its elastic principle, expect it to pass out again immediately: whereas the fact is, that it passes out by slow degrees; and takes a considerable time in evacuating the *apparatus* effectually. Some *power* therefore must hinder the fluid, at least, in some measure, from escaping: and that *power* must be exerted at, or near, the surface of the body.

To say it is detained by an attraction of the body, will not answer the purpose: for the *power* which is supposed to draw the fluid into it, must certainly be sufficient to hinder it from passing out. Now by the experiment, the fluid does pass out, though slowly: this *power* therefore, which resists its passing out, can be no other than what arises from the *medium* we have proved to be spread upon the surfaces of bodies.

The evidence in favour of this doctrine is greatly strengthened by the following experiments, the three first of which, are well known to electric enquirers.

When glass is properly electrified, and held over the wooden part of the *apparatus* (c) at the distance of six or eight inches, and there *continued* for a time, the balls are separated to a considerable distance.

But upon taking away the glass, the separation is at an end, and there are no electric signs remaining in the balls.

These appearances therefore argue, that no part of the electric fluid, appertaining to the excited glass, passed from it into the wood. And that the cause, which obstructed its passage, is a *resistance*, exerted at or near, the surface of the wood: because we know, from a variety of experiments, that the *repulsive power*

of this fluid acts at great distances, and in gross bodies particularly ; without the fluid being able to enter them. There can then be no doubt that the separation of the balls, in the present circumstances, entirely depends upon this *repulsive power* ; which drives the natural quantity of the fluid belonging to the wood, or part of it at least, towards the balls. And though the *repulsive power* is sufficient to force the fluid from the wood into the balls, and there occasion the effects of a *plus* electricity, (as is found upon tryal;) * yet the experiment shews, it is not sufficient, in the same circumstances, to force it out of them, as they cease to be electrified on removing the *power*.

If this is not the case, and the fluid from the glass is supposed to enter to the wood ; I would ask, why the balls do not retain the fluid, or at least some part of it, and continue separated when the glass is taken away ? It would be unphilosophical to say, the glass actually suffered a quantity of the electric fluid to pass from it, into the wood and balls ; and then, on removing the glass, that it took it away again ; or attracted it back : because when the same glass is brought near enough to the wood, the balls will be electrified, and separated so effectually, as to *continue* in that state of separation, after the glass is removed : which proves clearly, that the *repulsive power* is not only great enough to overcome the *resistance* of the balls ; but even to force out some part of the fluid contained therein : therefore in this case the balls are electrified *minus*. And this *minus* may be increased,

* For the proof of this, see the Essay by Dr. Hoadly and myself, page 13.

by bringing the power gradually nearer, and then removing it quickly.

We may then very justly conclude, that this separation of the balls, is occasioned by the expansive power of the electric fluid, or *aëther*, crowding from without, and through the air, to enter the balls, and restore the *equilibrium*. And in like manner that the *plus* electricity causes a separation of the balls in consequence of the same electric fluid, or *aëther*, crowding from *within* to get out of the balls, and passing through a like quantity of air, in order to restore the *equilibrium*; *for the same medium which appertains to the surfaces of bodies, must resist the exit and entrance equally*: and therefore the one case will be always the converse of the other.

We have seen that on bringing the electrified glass near enough to the wood, the balls are electrified *minus*. If now the circumstances are changed, by bringing the same glass *considerably nearer* to the wood, and much quicker, the balls are electrified *plus*; and continue so for a considerable time after the glass is removed *: which is the strongest confirmation that this effect entirely depends upon the *resistance* of the wood being overcome, and the electric fluid entering the apparatus, by the nearer approach of the glass.

There are then two different methods of causing a *plus* appearance in the balls. The one, we find, depends upon an actual entrance of the fluid, from the glass, into the wood, &c. And the other upon a quantity of the fluid, originally in the wood, being forced

* The glass in this experiment must not only be brought quickly towards the wood, but it must likewise be removed from it as suddenly.

from it into the balls, by the *repulsive power* of the fluid appertaining to the excited glass.

In these experiments we also learn, that when the glass is held nearer to the wood, than in the *minus*, and farther from it than in the *plus* appearance of the first experiment, it does not produce any electric signs whatever in the balls. Which shews a kind of *balance* subsisting between the *power* of the glass, and the *resistance* of the wood, &c. For, if we deviate the least on either side from this intermediate distance, electric effects, of the one kind or other, immediately take place.

I shall produce another experiment in favour of these forces, and of the *balance* obtaining between them, in certain circumstances. In the experiment I am about to mention, it is necessary, first to electrify the wood and balls; by properly rubbing that end of the sealing-wax, amber, or glass, to which they are affixed.

You know then, that, if we touch the balls with the hand, we immediately unelectrify them, and the wood; but not the amber: and that these balls with the wood, will continue unelectrified. But if I blow ever so gently *against* that part of the *amber* which is electrified, the balls will be separated to a considerable distance, and continue in that state. On the other hand, if the *amber* has not been rubbed, no electricity can be produced by the same force of blowing, or even by a blast six or eight times greater: but if the blast is considerably increased, the *amber* will be electrified*. By which it appears that in the first case, the *electric power* in the *amber* receives such an *additional force* from

* See the letter to Dr. Heberden, page 332.

the slight *friction* of the *breath*, as enables it to destroy the *ballance* and overcome the *resistance at the surface of the wood*, in that part, where it is joined to the amber.

The *Leyden* experiment depends also upon a certain *ballance*, which obtains between the *mediums* at the opposite surfaces of the glass, by the *power of repulsion**: but this enquiry, being of a very extensive nature, would lead me too far for the business of a letter; I must therefore refer you to the works quoted last. In selecting the experiments above related to prove the *resistance*, I have purposely confined myself to a few, and those such as appeared to be the most simple and most worthy of attention. You will therefore do me the honor to examine, and consider carefully these experiments and observations, as I think they have sufficiently established a *resistance appertaining to bodies*, independent of the gross matter they contain: and that it arises from the same *elastic medium* which we before proved to exist between the convex glasses.

London Dec. 20, 1763.

I am, &c.

B. Wilson.

* The consideration of this experiment was particularly attended to, in a former work, by Dr. Hoadly and myself. See also the Phil. Trans. Vol. LI. Part II. and Pages 898, 899.

Expla-

Explanation of Fig. 1. TAB. XXII.

- a, b,* Two *Tourmalins* fixed upon
- c. c.* Sticks of the hardest kind of sealing-wax.
- d, d,* Two handles of wood, in which the other ends of the sealing-wax are secured.
- f, f,* A stand of wood, with holes to rest the handles therein, when the *Tourmalins* are unemployed.

Explanation of Fig. 2. TAB. XXIII.

- a, a,* Two very small balls of the pith of Elder, suspended by
- b, b,* The finest flaxen threads about six inches long. The balls and threads together weigh about the fiftieth part of a grain.
These threads are neatly fastened in a small hole on the under side of the thin end, and so as to touch the wood
- c,* Which is mahogany. Every part of this small piece of wood is well polished, and neatly joined to
- d,* the cylinder of amber; which is about four inches long, and near half an inch thick. It is finely polished also, and the other end slides into
- e,* which is part of the arm: *e* is joined to *f* by a screw; and the other end of
- f,* slides into the upper part of the stand *g*. One end of the upright *h* screws on to *g*, and the other end, into the cross pieces *i, i*, and *kk*; which are let into each other by a mortass, and thus.

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thus secured. The whole *apparatus* takes to pieces easily, for the conveniency of packing up in a case six inches and half long.

The stand is made of Cocoa-tree, without angles, or edges, and well polished.

The size of my *apparatus* is about twice as large as the drawing before you.

LV. A

LV. *A Discourse on the Parallax of the Sun. By the Rev. Thomas Hornsby, M. A. Savilian Professor of Astronomy in the University of Oxford, and F. R. S.*

Read Dec. 23, 1763. THE quantity of the Sun's parallax is of such importance both to the theory and practical part of astronomy, that every method of determining it hath been employed by the astronomers of every age. Mr. Flamsteed informs us, in the 92d and 96th numbers of the Philosophical Transactions, that from some observations made upon the planet Mars, he had found the Sun's parallax not to exceed 10 seconds; and Dr. Halley, in a memoir written expressly with a view to ascertain the exact quantity of it, supposes it not to be greater than $12 \frac{1}{2}$.

When we consider the imperfect state of astronomy at the time when Mr. Horrox lived, we cannot sufficiently admire the wonderful genius of that young gentleman, who at the age of 24 could collect from his own observations, that the parallax of the Sun did not exceed 14 seconds; while many celebrated astronomers, whose tables were then in the greatest repute, had assigned a parallax of more than two minutes to the Sun, which Kepler had supposed could not be less than 59 seconds, and which Hevelius, who published the admirable treatise of Mr. Horrox, intitled, *Venus in Sole visa*, fixed at 41 seconds.

In

In the year 1719, Dr. Pound and his nephew, that illustrious astronomer, Mr. Bradley, did, when Mars was in opposition to the Sun, demonstrate (to use the words of Dr. Halley, Phil. Trans. N°. 366, p. 114.) the extreme minuteness of the Sun's parallax, and that it was not more than $12''$, nor less than $9''$, upon many repeated trials. At the same time and by the same kind of observations Mr. Maraldi determined this parallax to be $10'$, the result of his observations agreeing exactly with those deduced from the correspondent observations by Mr. Richer at Cayenne and by Mr. Cassini at Paris in the year 1672.

The voyage which the Abbé de la Caille undertook, to perfect a catalogue of some of the principal fixt stars, furnished the astronomers with the means of determining the Sun's parallax by corresponding altitudes of the planets Mars and Venus, to be observed on each side of the equator, with all the accuracy of which that method is capable. The astronomers here in Europe were invited to determine the distances of the planets from particular stars on stated days, while the Abbé himself proposed to make the corresponding observations on the southernmost part of Africa at the Cape of Good Hope. By the differences of the altitudes of the northern limb of Mars and of such stars as were nearly in the same parallel observed on the same day at the Cape with a sextant of 6 f. radius; at Greenwich by Dr. Bradley with a mural quadrant of 8 f.; at Bologna in Italy by M. Zanotti with a similar instrument of 5f.; at the Royal Observatory at Paris by Messieurs Cassini de Thury and Gentil with a moveable quadrant of 6 f.; and in Sweden by Messieurs Wargentin, Strömer and Schem-

Schemmark, with telescopes of 7 and 8 f. armed with micrometers, it was found, when every reduction is made, that, according to each observation, the dates of which are given below, the horizontal parallax of the Sun, when at its mean distance from the earth, was as is represented in the following table.

| Greenwich. | Bologna. | Paris. | Stockholm. | Upsal. | Hernofand. |
|---------------------|------------------|-----------------|------------------|-----------------|-----------------|
| 1751. | 1751. | 1751. | 1751. | 1751. | 1751. |
| Aug. - - 30 9, 677 | Aug. 31 9, 753 | Sept. 13 9, 134 | Sept. 10, 466 | Sept. 21 9, 438 | Sept. 25 9, 933 |
| Sept. - - 13 9, 324 | Sept. 1 9, 895 | 14 9, 715 | 25 10, 504 | 24 12, 255 | 27 10, 618 |
| 14 9, 096 | 13 9, 971 | + 24 11, 912 | Oct. + 3 12, 864 | 25 9, 715 | |
| Oct. - - 3 10, 161 | 14 10, 238 | Oct. 8 9, 895 | 5 10, 085 | Oct. 6 9, 134 | |
| 4 10, 504 | Oct. + 7 11, 075 | | 6 9, 735 | | |
| 7 9, 515 | | | | | |
| + 9 10, 961 | | | | | |
| Mean of all 9, 891 | 10, 186 | 10, 164 | 10, 734 | 10, 235 | 10, 875 |
| Mean rej. + 9, 712 | 9, 964 | 9, 581 | 10, 202 | 9, 421 | |

By taking a mean of all the observations, it follows that the Sun's mean horizontal parallax is $10''$, 2; and if we reject the observations which differ most in excess from the rest, the mean will give 9,842 for the Sun's mean horizontal parallax.

Besides these 27 determinations, the Abbé de la Caille compared 41 observations, the mean of which is given in the following table.

| N° of Obs. | Observations. | Instruments. | Places. | Par. |
|--|--------------------------------------|----------------------|------------|----------|
| 7 | The late M. Caffini and M. Maraldi | Quadrant 2 f. rad. | Thury | " 8, 982 |
| 6 | Mr. Delisle — at the Hotel de Clugny | Mur. circle 2 f. | Paris † | 11, 532 |
| 3 | Father Beraud | Refr. tel. 7 f. | Lyon | 9, 020 |
| 6 | M. M. Garipuy and d'Arquier | Ditto. | Toulouse | 8, 944 |
| 12 | M. Sabatelli and Father Careni | Quad. 4f. diag. div. | Naples | 9, 933 |
| 7 | M. Bosc | Tel. of 6 and 8 f. | Wittemberg | 10, 999 |
| Mean of all observ. according to A. Caille | | | | 10, 210 |
| Mean of Results (rejecting the 2d) | | | | 9, 575 |

V O L. LIII.

P p p

Few

Few observations of Venus near the inferior conjunction with the Sun on Oct. 31. 1751 were made, on account of the unfavourable weather here in Europe. By an observation made at Greenwich on Oct. 25, the mean horizontal parallax was $9'',8$; but according to the observation made at Paris on the same day at the Royal Observatory, that parallax was $11'',4$. On Oct. 27, by an observation made at Paris, the Sun's mean horizontal parallax was $9'',85$; but by an observation at Bologna on the same day it was found to be $10'',4$ — By the observation at Paris on Nov. 17, the Sun's mean parallax was $10'',5$. By a mean of all the observations of Venus, the Sun's mean parallax is $10'',38$; and if we reject the Paris observation on Oct. 25, that parallax is $10'',13^*$.

We see then that, according to these observations, the Sun's mean horizontal parallax is not less than $8'',94$. If we take a mean of the whole, that quantity is $10,09$: But if we reject the observations that differ most in excess, the Sun's mean horizontal parallax will be found to be $9'',92$ a determination in which every astronomer might readily acquiesce, when he considers the accuracy of the observers and the nice agreement of almost all the observations.

And such was the state of the Sun's parallax as deduced from the latest and best observations, when the approaching transit of Venus in 1761 engaged the attention of the curious of all nations. Dr. Halley, in Philosophical Transactions, N°. 348, had proposed a method of determining the Sun's parallax

* See the Abbé de la Caille's Introduction to his Ephémérides Célestes from 1765 to 1774.

by

by procuring observations to be made upon this transit in such places where the difference of time between the ingress and egress would be the greatest possible; namely near the mouth of the Ganges, where the Sun would be vertical at the middle of the transit, and at Port-Nelson in Hudson's-Bay, where the planet would enter upon the Sun's disk about the time of Sun-set, and leave it soon after Sun-rising; for in the former place, says Dr. Halley, the planet would be equally distant from noon both at ingress and egress, and the apparent motion of Venus upon the Sun would be accelerated by almost double the quantity of the horizontal parallax of Venus from the Sun: because Venus is at that time retrograde, and moves in a direction contrary to that of the eye of an observer upon the earth's surface. Whereas in Hudson's-Bay, under an opposite meridian, the eye of an observer will be carried, while the Sun seems to move under the pole from setting to rising, in a direction contrary to the motion of the observer's eye at the Ganges; that is, in the direction of the planet's retrograde motion from east to west.—From these considerations, and supposing with Dr. Halley the axis of the planet's path to be inclined to the axis of the equator in an angle of $2^{\circ}.18'$ only, the interval between the two contacts would have been $15'.10''$ longer in Hudson's Bay than at the mouth of the Ganges.

But upon examination the case is found to be somewhat different. The axis of the equator on the 6th of June 1761 made an angle of $6^{\circ}10'$ with the axis of the ecliptic on one side, and the axis of the planet's path an angle of $8^{\circ}.30'.10''$ on the other; the axis of the planet's path therefore made an angle with the equator of $14^{\circ}.40'.10''$.—The planet's latitude was $5\frac{1}{2}$ minutes greater both from observation and

the Doctor's own tables, than he had supposed in his calculation made from the Rodolphine tables corrected: and therefore the planet's egress could not have been observed at Port Nelson. Having made a computation for a place in North America situated $5^{\text{h}}. 30'$ to the west of Greenwich, and in the 60th degree of latitude; and also for a place to the east of Ganges, and $6^{\text{h}}. 30'$ to the east of Greenwich in the latitude of $22^{\circ}. 42'$ north, that the places might be nearly situated in the same circumstances with the mouth of the Ganges and Port Nelson, I find that the interval between the two contacts would be but $4'. 56''$ longer in America than in the East Indies, supposing the Sun's parallax $12''.5$, and the inclination of Venus's path $14^{\circ}. 40'$ to the equator.

And here perhaps it may not be altogether unnecessary to enquire how far the mistake which Dr. Halley committed, by using the difference of the two angles instead of their sum, would influence the times of the transit as seen at Ganges and Port Nelson. For this purpose I made use of the same elements which Dr. Halley has given in his paper, and calculated the angle of the vertical with the orbit of Venus at the two internal contacts at both places, supposing the orbit to be inclined first only $2^{\circ}. 18'$ to the equator, agreeably to Dr. Halley's supposition, and also $14^{\circ}. 40'$. and I found that the duration would be $15'. 13''$ longer at Hudson's-Bay than at the Ganges upon the first supposition; and $14'. 44''$, if the circles be duly inclined to each other; the difference being only 29 seconds. It has already been found by calculation, supposing the latitude of Venus to be about $9 \frac{1}{4}$ minutes, that the difference of duration at the two places would have been only $4'. 56''$. It may fairly

fairly therefore be concluded that the transposition of the circles contributed very little towards giving so different a result, the reason of which need not here be mentioned; and Dr. Halley seems to have been led into the mistake entirely from supposing the latitude of Venus to be about $4'. 0''$ according to the tables, which he then used, constructed upon the principle that the nodes of that planet were fixed.— Having determined that the difference of duration at the two places above mentioned would be $15'. 10''$ (differing only $3''$ from the method I used which is independent of projection) the Doctor proceeds to shew, that if Venus had no latitude at the time of the middle of the transit, the difference would be $18'. 40''$; and if the planet should pass $4'. 0''$ to the north of the Sun's center, that difference would be $21'. 40''$, and would become still greater, if the planet's north latitude should be farther increased. And such would have been the event, had the motion of the nodes been progressive. But, agreeably to the principles of universal attraction, their motion is really retrograde, and this Dr. Halley says he himself suspected, *ut ob nuperas quasdam observationes suspicio est.* And therefore it is somewhat surprising that he did not determine by calculation what would have been the difference in the whole duration between the two places, if Venus should pass more to the southward of the Sun's center, than he had supposed. He would then immediately have perceived that the two stations were not so advantageously placed, as the solution of the problem required.

Observers were therefore to be sent to other places; in order to determine the Sun's parallax agreeably to the method proposed by Dr. Halley. The city of Tobolski

Tobolski in Siberia is so situated, that the interval between the two contacts was perhaps as short as could possibly be observed on any part of the earth's surface; to this place was sent the Abbé Chappe d'Auteroches, one of the French astronomers. Near Hudson's Bay and in 60° of latitude the duration would have been 5 minutes longer, supposing the Sun's parallax = $9''$. At Bencoolen, where it was first proposed to send Mess. Mason and Dixon, the difference would have been about $4\frac{1}{2}$ minutes. At the island of Rodrigues, where Mr. Pingré could only observe the last internal contact, the difference would have been about $7\frac{1}{2}$ minutes. On the southern coast of New-Holland, it would have been somewhat more than 10 minutes. And in the great Indian Ocean, under $115\frac{1}{4}$ of absolute longitude from the Isle of Ferro and in 57° of south latitude, where the beginning of the transit would happen soon after Sun-rising, and the end just before Sun-set, the difference would amount to $13\frac{1}{4}$ minutes. The greatest difference between the interval of the two internal contacts, as determined by actual observation on the 6th of June, was $2'.49'',75$, a quantity hardly sufficient to determine the Sun's parallax agreeably to the method proposed by Dr. Halley.

I have however made the necessary calculations, and compared the duration of the transit observed at several places with the duration as observed at Tobolski. The parallax resulting from each observation is contained in the following table, in which the 3d column contains the observed duration, the 4th the difference of each observed duration; the next contains that difference as deduced by computation upon a supposition that the Sun's parallax is $9''$. In the last

last column is given the horizontal parallax on the day of the transit, resulting from a comparison of the 4th and 5th columns.

| Places. | Observers. | Observed duration. | Difference of observ'd dur. | Difference by calculation. | Sun's Parallax. |
|--|---------------|--------------------|-----------------------------|----------------------------|-----------------|
| Tobolski | Abbe Chappe | 5 48 53,25 | " | " | " |
| Cajaneburg | Mr. Planmann | 5 49 54 | 1 00, 75 | 1 00, 88 | 8, 980 |
| Tornea° | Hellant | 5 50 09 | 1 15, 75 | 1 05, 27 | 10, 444 |
| Tornea° | Lagerborn | 5 50 21 | 1 27, 75 | 1 05, 27 | +12, 098 |
| Upsal | Bergman | 5 50 26 | 1 32, 75 | 1 33, 78 | 8, 901 |
| Upsal | Mallet | 5 50 07 | 1 13, 75 | 1 33, 78 | +7, 077 |
| Upsal | Strömer | 5 50 02 | 1 08, 75 | 1 33, 78 | +6, 597 |
| Hernosand | Gister | 5 50 26 | 1 32, 75 | 1 25, 76 | 9, 733 |
| Abo | Justander | 5 50 09 | 1 15, 75 | 1 20, 68 | 8, 450 |
| Stockholm | Wargentin | 5 50 45 | 1 51, 75 | 1 34, 21 | 10, 675 |
| Stockholm | Klingenstiern | 5 50 42 | 1 48, 75 | 1 34, 21 | 10, 389 |
| Calcutta | Magee | 5 50 31 | 1 37, 75 | 1 37, 02 | 9, 067 |
| Madras | Hirrit | 5 51 43 | 1 2 49, 75 | 2 39, 50 | 9, 577 |
| Mean of the whole | | | | | 9, 332 |
| Mean, rejecting 2 observations at Upsal and 1 at Tornea- | | | | | 9, 579 |

The duration at Cajaneburg was the shortest except at Tobolski; with which if we compare the duration observed at Madras the parallax is 9'',948: and by taking a mean of the parallax deduced from a comparison of the observation at Madras with those of Tobolski and Cajaneburg, the parallax is 9'',762.

The observations at the above places agree as well together as can be expected from such small differences in the duration, which must in some measure be influenced by the necessary and unavoidable errors in observation.

If

If the quantity of the sun's diameter, and the least distance of the centers were very exactly known, tha Sun's parallax might safely be determined by comparing the duration of the transit as observed at different places with the duration as supposed to be seen from the earth's center. According to this method, supposing the least distance of the centers to be $9'.29''\frac{1}{4}$, which is a mean between the Greenwich Shirburn and Paris observations, and the difference of the semidiameters of the Sun and Venus= $916'',4$, the duration as observed at Tobolski was more than 10 minutes shorter than if seen without parallax ; at Tornea^o, at Stockholm, at Cajaneburg, at Astracan, and indeed in almost every part of Europe and Asia, the duration was considerably shortened ; and if a number of good observations made in several of those parts were procured, the Quantity of the Sun's, parallax might be well enough ascertained, as the difference in duration for a difference of one second in the Sun's parallax will be found very considerable.

Tho' this method should not be practised, unless the necessary requisites for the computation be known with some degree of precision, I have ventured to compare the durations observed chiefly in the northern parts of Europe, and some in Asia, with the duration, as seen from the earth's center,= $5^h.59'.19''$, 10 mean time, or $5^h.59'.16''$, 64 apparent time, and calculated from the elements above mentioned.

Places

| Places. | Observed durations. | Duration without parallax. | Difference of durations. | Differ. for 1" of parallax. | Sun's parallax. |
|--------------|---------------------|----------------------------|--------------------------|-----------------------------|-----------------|
| Tobolski - | 5 48 53 25 | 5 59 16, 64 | 10 23, 39 | 64, 09 | " 9, 726 |
| Cajaneburg - | 5 49 54 | | 9 22, 64 | 57, 32 | 9, 815 |
| Tornea° - | 5 50 09 | | 9 17, 64 | 56, 83 | 9, 636 |
| Tornea° - | 5 50 21 | | 8 55, 64 | 56, 83 | 9, 425 |
| Abo - | 5 50 09 | | 9 07, 64 | 55, 12 | 9, 935 |
| Upsal - | 5 50 26 | | 8 50, 64 | 53, 67 | 9, 888 |
| Upsal - | 5 50 07 | | 9 09, 64 | 53, 67 | 10, 241 |
| Upsal - | 5 50 02 | | 9 15, 64 | 53, 67 | 10, 352 |
| Hernosand - | 5 50 26 | | 8 50, 64 | 53, 65 | 9, 890 |
| Stockholm - | 5 50 42 | | 8 34, 64 | 53, 62 | 9, 579 |
| Stockholm - | 5 50 45 | | 8 31, 64 | 53, 62 | 9, 523 |
| Calcutta - | 5 50 36 | | 8 40, 64 | 53, 31 | 9, 766 |
| Madras - | 5 51 43 | | 7 33, 64 | 46, 36 | 9, 785 |

N. B. The 4th column contains the difference between the observed and calculated duration; in the 5th is given the difference in the duration for a difference of 1" in the Sun's parallax, and the 6th column is obtained by dividing the 4th by the 5th.

The mean of all the results is 9", 812: and if we reject two of the observations at Upsal, which differ most in excess, the Sun's parallax is 9", 724, agreeing very nearly with the quantity resulting from a comparison of some of the observed durations with the shortest observed at Tobolski and Cajaneburg.

We may also proceed to find the Sun's parallax by means of the least distance of the centers as observed in two or more places where the effect of parallax was contrary; or if the least distance of the centers was only determined at one place, it may be found by calculation at any other place, where the total duration was observed. But in this and the last case the elements of calculation are required with so rigorous

exactness, that perhaps these methods are only to be called in to illustrate and confirm the others.

Mr. Pingré confined himself principally to the determination of the least distance of the centers. At $21^{\text{h}}.43'.11''$ he found the distance between the nearest limbs of Venus and the Sun to be the greatest $= 5'.57'',2.$ or $5'.57'',4$ when corrected by refraction. This distance being subtracted from $15'.19'',5$ the difference of the Semidiameters, leaves $9'.22'',1$ for the least apparent distance of the centers. But as that observation was made rather too late, when the distance of the centers was greater than it ought to be, he found by calculation that it should be diminished by $0'',22.$ The true apparent least distance of the centers by actual observation was therefore $9'.21'',88.$ In order to be more secure of this result, Mr. Pingré compared a large number of observed distances, both at the beginning and towards the middle of the transit, with the distance determined by internal contact, and after excluding every doubtful observation, he found the least apparent distance of the centers to be $9'.21'',69.$ By comparing this distance with the distance deduced from the total duration as observed at any place (the method of finding which he has given at large in his memoir inserted in the Memoirs of the academy of sciences for 1761) and by knowing from calculation what influence a parallax of $10''$ for instance would have upon those distances, he found the Sun's parallax as in the following table.

Places

| Places. | Observed dura- tions. | L. distance of centers from the durati- ons. | L. distance deduced from calculation. | Sun's pa- rallax. |
|--------------------|--------------------------|---|---|----------------------|
| Tobolski - - - - | 5 48 53, 25 | 9 51, 53 | 9 52, 24 | 10, 125 |
| Stockholm - - - - | 5 50 43, 5 | 9 54, 85 | 9 55, 83 | 10, 03 |
| Upsal - - - - | 5 50 26 | 9 55, 62 | 9 55, 95 | 10, 23 |
| Cajaneburg - - - - | 5 49 54 | 9 55, 61 | 9 55, 61 | 10, 00 |
| Tornea° - - - - | 5 50 09 | 9 55, 28 | 9 56, 08 | 10, 09 |

By taking a mean of these determinations, we find the Sun's parallax to be $10'',\text{1}$. In the above calculations the Sun's semi-diameter was supposed = $15''.48'',\text{5}$, and that of Venus $29''$. Observers, says Mr. Pingré, have found the former to be about $2''$ less, and the latter on the contrary half a second larger. By calculating upon the supposition of a difference of $2''$ in the difference of the semidiameters of the Sun and Venus, the least distance of the centers at Tobolski, Stockholm, Upsal, Tornea°, and Caja-neburg, ought to be $3'',\text{12}$ less, and at Rodrigues $2'',\text{56}$ or $2'',\text{60}$, and the Sun's horizontal parallax ought also to be $0'',\text{17}$ less. If then this correction be admitted, which is warranted by the best observations, the Sun's horizontal parallax will be $9'',\text{92}$.

There is still another method by which we are enabled to determine the Sun's parallax, by comparing the observations made in different places where the effect of parallax upon the planet is considerable at the times of the two contacts. It was more convenient to make use of the 2d internal contact for this purpose, and the observers were very advantageously stationed at St. Helena and the Cape of Good

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Hope:

Hope: for by comparing the observations made there with those at Tornea^o, Tobolski, and in some of the Eastern parts of Asia, the difference of the times of the contacts when reduced to the same meridian will be found to be very considerable, amounting to more than $9\frac{1}{4}$ minutes at the two first places above mentioned, and being greater, as the places are farther situated to the North-East. But if this method be used, it is absolutely necessary that the longitudes of the places should be determined with the utmost accuracy, since an error of a few seconds would have a considerable influence upon the result, and would increase or diminish the quantity of the Sun's parallax, in proportion. The unfavourable state of the heavens at the time of the internal contact prevented the Rev. Mr. Maskelyne from making an observation at the Isle of St. Helena; which is the more to be lamented as his observation would have confirmed or corrected the observation at the Cape, if necessary; since the effect of parallax at both places would have been very nearly the same. The observers at the Cape were more fortunate, and differed only 4" in their observation of the internal contact. — But before we proceed to deduce the quantity of the Sun's parallax, by comparing as well the observation made at Greenwich as those at other places, with the observation at the Cape, it will be necessary to lay before the reader the authorities upon which the longitude of each place has been determined.

The longitude of the Cape of Good-Hope was not even nearly known till the Abbé de la Caille went thither in the year 1751. By a comparison of 9 eclipses of Jupiter's satellites as well immersions as emersions observed at the Cape with the corresponding

ing observations made at Paris, the Cape was found by the Abbé de la Caille himself to be $1^{\text{h}}.4'.14''$ to the East of Paris, or $1^{\text{h}}.13'.31''$ to the East of Greenwich. Mess. Mason and Dixon observed many eclipses of Jupiter's satellites at the Cape, but the weather was not so favourable here in England. However by comparing four observations made in Surrey-Street and one at Greenwich with those made at the Cape, the difference of longitude at a mean is found to be $1^{\text{h}}.13'.28''$, which I have used in the following computations.

The internal contact, as reduced from siderial to apparent time by Mr. Mason, happened at $21^{\text{h}}.39'.52''$.— But upon examination it will be found to have happened later: for whether we make use of the Sun's mean R. ascension from the best solar tables extant, or the Sun's apparent R. ascension reduced to the meridian of the place as determined by actual observation on the day of the transit, the true apparent time of the contact will be found to have happened at $21^{\text{h}}.39'.54$ — or at $21^{\text{h}}.39'.54 \frac{1}{2}$ if the time by the star Antares be used, whose situation was more favourable to an observer in 34° . of South latitude. I shall therefore suppose the internal contact to have happened at $21^{\text{h}}.39'.52''$ by taking a mean of the two observations*.

The Royal Observatory at Paris was supposed by Sir Isaac Newton, in his Principia, to be $9'.20''$ to the East of Greenwich. And the editor of Dr. Hal-

* Mr. Mason, before he left England, acknowledged, in a letter to me, that he had committed a mistake in his calculation, by forgetting to apply to the Sun's place the equation of præcession, which on the day of the transit amounted to — $15'',6$.

ley's

Icy's tables has followed that determination, which has also been generally used by the English Astronomers. — The French Astronomers have till very lately imagined the difference of meridians to be $9'.10''$. as deduced from a single observation of an eclipse of Jupiter's first satellite made by Mr. Cassini when in London, with a telescope of similar size and construction with that used at Paris when the same eclipse was observed. — In the year 1734 Mr. Maraldi published a comparison of 33 eclipses observed at Greenwich by Mr. Flamsteed, and at Paris by the French Astronomers, 19 of which are immersions, and the rest emersions. The longitudes resulting from each correspondent observation differ widely from each other, the two observatories being $11'.27''$ distant by an immersion of the 2d satellite, and only $7' 43''$ by an emersion of the first. But if we take a mean of the whole, the difference of longitude will be $9'.24''$; and if we exclude the observation of the 2d satellite above mentioned, which must be very faulty, the difference of meridians will be $9'.22''$, a result which in all probability is but a very few seconds from the truth. It may be observed that the immersions all give the difference of longitude too great, and almost all the emersions too little; a circumstance owing either to the badness of the air here in England, or to an inequality in the goodness of the telescopes, or perhaps to both; for whatever was the advantage in observing the immersions, was ballanced by the emersions: for which reason whenever the eclipses of Jupiter's satellites are used, the longitude should, if possible, be deduced both from immersions and emersions.

As the observations of transits of Mercury may be very useful in settling the longitudes of places which are

are not far distant, I have examined the several observations that I can meet with made at Paris, and either immediately at Greenwich or in such parts of London whose longitude from Greenwich is known within one second of time. And the result of such comparisons is as follows.

On the 29th of October 1723 Dr. Halley observed the first interior contact of the limbs of Mercury and the Sun at $2^h\ 42' .26''$ apparent time at Greenwich. The Rev. Mr. Professor Bradley observed the same at $2^h\ 42' .38''$, at Wansted in Essex ($10''$ to the East of Greenwich) or at $2^h\ 42' .28''$ when reduced to the meridian of Greenwich. Mr. Graham in Fleetstreet observed the same at $2^h\ 42' .19''$, or at $2^h\ 42' .44''$, when reduced to Greenwich. The mean of these is $2^h\ 42' .32'',7$. In the observatory at Paris Mr. Maraldi observed the same at $2^h\ 51' .48''$ apparent time; and Mr. Delisse at $2^h\ 51' .37''$, but suspects it might have been some few seconds later. I will suppose it to have happened at $2^h\ 51' .43''$, 5. The difference of meridians therefore is $9'.10'',8$. If we take a mean of Dr. Halley's and Mr. Bradley's observations only, the difference of meridians is $9'.16'',5$.

In the year 1736 Dr. Bevis observed the last contacts of the limbs of Mercury and the Sun at $0^h\ 8' .33''$ at Greenwich. The same was observed at Paris by M. Maraldi and M. Cassini de Thury, and at Thury by Mr. Cassini, at $0^h\ 18' .05'',5$ by a mean of the three observations. The difference of longitude therefore is $9'.32'',5$.

In the year 1743 the last internal contact of the limbs was observed by Mr. Graham in Fleetstreet at $1^h\ 0' .42''$, and by Dr. Bevis at Beaufort-Buildings in the

the Strand at $1^{\text{h}}.0'.33''$: or by a mean of both when reduced to the meridian of Greenwich at $1^{\text{h}}.1'.04''$.— The same was observed by the Abbé de la Caille, by Mess. Maraldi, Monnier, and Cassini the son, at Paris, and by Mr. Cassini at Thury: which observations, when reduced to the meridian of the Royal Observatory, give $1^{\text{h}}.10''.15'',5$ for the time of the internal contact: the difference of meridians is therefore $9'.12''.5$. — By a mean of the observations of Mr. Graham and Dr. Bevis when reduced to Greenwich, the last external contact on the same day happened at $1^{\text{h}}.2'.42''$. and by a mean of the observations in France the same happened there at $1^{\text{h}}.12'.10''$. The difference of longitude therefore is $9'.28''$. N B. No observations were made of this transit at the Royal Observatory at Greenwich, on account of clouds.

In the year 1753 was another transit of Mercury, when the unfavourable state of the heavens a few seconds before the time of the internal contact prevented any observations from being made at Greenwich, as appears from a paper communicated to me by the executors of the late Dr. Bradley. Both contacts however were luckily very well observed, by Mr. Short, Dr. Bevis and Mr. Bird; by a mean of whose observations reduced to the meridian of Greenwich the internal contact happened at $10^{\text{h}}.9'.37'',5$. The same contact was observed by 13 observers at Paris, and was found not to happen sooner than $10^{\text{h}}.18'.36''$, nor later than $10^{\text{h}}.19'.03''$. But by a mean of all at $10^{\text{h}}.18'.45''$. The difference of meridians therefore is $9'.07'',5$. By a mean of the observations of Mr. Short, Dr. Bevis, Mr. Bird, Mr. Canton, and Mr. Sisson, all reduced to the meridian of Greenwich, the external contact

contact happened at $10^h.12'.17'',5.$ and at the Royal Observatory, by a mean of all the observations at Paris, at $10^h.21'.33''$. The difference of longitude therefore is $9'.15'',5.$ And if we take a mean of these 7 results, the Royal Observatory at Paris will be found to be $9'.17'' \frac{1}{2}$ to the East of the Royal Observatory at Greenwich, a determination very nearly agreeing with that mentioned by Sir Isaac Newton, and which, I believe, was deduced from a comparison of Dr. Halley's and Mr. Cassini's observations.

The Abbé de la Caille, in his memoir on the parallax of the Moon, supposes the difference of meridians to be $9'.17''$ tho' he has not mentioned from what authority he drew that conclusion. I shall therefore suppose the difference of meridians to be $9'.17''$.—The last internal contact was observed at Paris by Mr. de la Lande at $20^h.28' 25''$ or $26''$; at $20^h.28'.26''$ by Father Clouet, and by Mr. Maraldi and Mr. Barros separately at $20^h.28'.42'$. Mr. Pingré, in a very curious memoir on the Sun's parallax already referred to, supposes the internal contact to have happened at Paris at $20^h.28'.38''$. I shall therefore make use of the Abbé de la Caille's observation at $20^h.28'.37'' \frac{1}{2}$.

The difference of meridians between Paris and Stockholm, says Mr. Wargentin, is $1^h.2'.51''$ or $52''$ at most. Mr. de la Lande from a comparison of 17 observations of the first satellite of Jupiter made from 1750 to 1759 and communicated to him by Mr. Wargentin, determines the difference of longitude to be $1^h.3'.10''$. And the Abbé de la Caille, in his memoir on the Moon's parallax, supposes it to be $1^h.3'.13''$. As these two last determinations agree so nearly together, I shall suppose Stockholm to be

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$1^{\text{h}}.3'.10''$ to the East of Paris, and $1^{\text{h}}.12'.27''$ to the East of Greenwich; and the last internal contact to have happened at $21^{\text{h}}.30'.09'',5$, which is a mean between the observations of Mess. Wargentin and Klingenstiern.

The City of Cajaneburg in Sweden is $38'.40''$ to the East of Stockholm, according to very late observations; and therefore Cajaneburg is $1^{\text{h}}.51'.07''$ to the East of Greenwich. The 2d internal contact happened at $22^{\text{h}}.7'.59''$, when the error in writing down the minutes is corrected according to the instruction given in Philosophical Transactions, for 1761, p. 231. Indeed (supposing the longitude of Cajaneburg as above set down to be exact) it is very easy to prove that the error of one minute was made at the egress rather than at the ingress.

The City of Tobolski in Siberia (according to the observation of the end of the solar eclipse on June 3d by Mr. Chappe and Mr. Planmann at Cajaneburg and calculated by Mr. Pingré) is $2^{\text{h}}.42'.11''$ to the East of Cajaneburg, and this determination is also confirmed by Mr. Wargentin's observation of the same phase. Tobolski therefore is $4^{\text{h}}.33'.18''$ to the East of Greenwich: and I suppose Mr. Chappe to have observed the last internal contact at $0^{\text{h}}.49'.23''$, without making any allowance for the luminous ring which appeared round Venus in his telescope.

The Observatory at Upsal (according to Mr. Wargentin in the Philosophical Transactions) is $1^{\text{h}}.1'.10''$ to the East of Paris, and is therefore $1^{\text{h}}.10'.27''$ to the East of Greenwich. By taking a mean of the three observations made there, the internal contact happened at $21^{\text{h}}.28'.06''$.

Tornea*

Tornea° has been generally supposed to be $1^h.27'30''$ to the East of Paris; but with this difference of meridians, the observations at Tornea°, tho' made by Mr. Hellant, a very excellent observer, will give a parallax of the Sun much less than the other observations made in high Northern latitudes. In order to settle the longitude of this place, I am of opinion that we may have recourse with safety, and without incurring the charge of reasoning in a circle, to the observation of the transit itself; I mean the observation of the internal contact at the ingress. Whether we suppose the Sun's parallax to be $8''$ or $10''$, the first internal contact would have happened sooner at Tornea° than at Stockholm $19''$ or $24''$. As the Sun's parallax will readily be allowed to be more than $8''$, I shall suppose the first internal contact to have happened $21''$ sooner. Tornea° is therefore $24'55''$ to the East of Stockholm, and consequently $1^h.37'.22''$ to the East of Greenwich. I shall make use of Mr. Hellant's observation of the internal contact at $21^h.54'.08''$ in preference to that of Mr. Lagerbom.

Abo, the capital of Finland, where Mr. Justander observed the last internal contact at $21^h.45'.19''$ (when a correction is made in the minutes) is $1^h.11'.29''$ to the East of Paris, and $1^h.28'.34''$ to the East of Greenwich.

At Herno-sand, which is $1^h.11'.29''$ to the East of Greenwich, I shall suppose the 2d internal contact was observed at $21^h.28'.52''$, as published in the Philosophical Transactions by Mr. Short from the Swedish acts.

I find the Island of Rodrigues by comparing three observations of eclipses of Jupiter's satellites with

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others

others made in England and at the Cape, to be $4^{\text{h}}.12'.38''$ to the East of Greenwich: and this determination is exactly confirmed by Mr. Pringré's comparison of the same eclipses. The observation of the occultation of a fixt star gives the longitude $6''$ or $7''$ greater. In the Philosophical Transactions, and even in the former part of the volume of the Memoirs of the Academy of Sciences for 1761, we find the internal contact was observed at Rodrigues at $0^{\text{h}}.34'.47''$; And yet in the memoire on the Sun's parallax it is said to have happened at $0^{\text{h}}.36'.49''$. Upon comparing this latter with the time by the clock, it should seem that Mr. Pingré had committed a mistake in subtracting the error of his clock instead of adding it. But he has no where mentioned any reason for this difference.

Gottingen, where the celebrated Mr. Mayer observed the first internal contact at $20^{\text{h}}.58'.26''$ to the East of Paris is $30' 16''$ or $39' 33''$ to the East of Greenwich.

The Abbé de la Caille has placed Bologna $36'.03''$ to the East of Paris: By comparing the observations of the transit of Mercury, I find, by a mean of three results agreeing very nearly together, that Bologna is $45'.15''$ to the East of Greenwich. Mr. Zanotti observed there the 2d internal contact at $21^{\text{h}}.04'.34''$. But as he used a refracting telescope of $2\frac{1}{2}$ feet, and as two other observers with telescopes of 10 and 22 feet saw the contact $24''$ later, I shall suppose it to have happened at $21^{\text{h}} 4' 58''$.

At Florence, the internal contact was observed with a reflector of more than 4 feet at $21^{\text{h}}.4'.28''$ by Father Ximenes. The longitude of this place is $34' 48''$ to the East of Paris, according to the table in the Connoissance des Mouveemens Celestes, or $35'.58''$ according

according to the table in the Elemens d'Astronomie by Mr. Cassini. By taking a mean of both, Florence is $44'40''$ to the East of Greenwich.

The longitude of St. Peters at Rome is $49'54''$ according to the French Astronomers. The internal contact was observed to happen at $21^{\text{h}}.09'.36''$. But as it is not said where this observation was made, the longitude given above will be found to be somewhat inaccurate.

Observations were also made at Madrid and Lisbon; at the former, the internal contact happened at $20^{\text{h}}.6'.56''$ apparent time : and at Lisbon at $19^{\text{h}}.44'.26''$. The longitude of Madrid, as given in the Philosophical Transactions, is certainly erroneous ; being more than a minute and a half too little, if the observation of the transit can be depended upon. At Lisbon, the longitude of the place was not determined by Mr. Ciera, who observed the transit, when Mr. Pingré, from whom I have taken the observation, left it in his way from Rodrigues. From the best accounts that I can collect, particularly from the 385th number of the Philosophical Transactions, and from an account of some observations by Mr. Short, Lisbon is about $36'.26''$ to the West of Greenwich.

Now in order to deduce the Sun's parallax from the observations related above, I proceeded in the following manner. Having subtracted the difference of longitude between Greenwich and the Cape $= 1^{\text{h}}.13'.28''$ from $21^{\text{h}}.39'.52''$ the mean of the observed times at the Cape, and compared the remainder with the observed time at Greenwich, I find that the internal contact was observed $7'.24''$ later at the Cape

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than at Greenwich, on account of parallax. I then calculated what would be the effect of parallax at each place, supposing the Sun's parallax to be 9 seconds; and found that the time of the internal contact would be accelerated $1'.16'',63$ at Greenwich, and retarded $6'.31',09$ at the Cape: the whole effect of parallax therefore is $7'.47'',72$. But the difference in time, as found by observation, is only $7'.24''$: and therefore the difference by calculation is to the difference by observation, as the assumed parallax is to the true parallax on the day of the transit, which by this observation is $8'',543$. The parallax resulting from each observation will be found in the following table, which will be sufficiently explained by the foregoing example.

| Places. | Difference of calculated times. | Difference of observed times | Sun's parallax. |
|----------------------|---------------------------------|------------------------------|-----------------|
| Greenwich - - - - - | 7 47, 72 | 7 24 | 8, 543 |
| Paris - - - - - | 7 28, 40 | 7 03, 5 | 8, 494 |
| Stockholm - - - - - | 8 53, 72 | 8 41, 5 | 8, 712 |
| Upfal - - - - - | 9 01, 83 | 8 45 | 8, 727 |
| Cajaneburg - - - - - | 9 42, 30 | 9 32 | 8, 841 |
| Tobolski - - - - - | 10 29, 06 | 10 18, 5 | 8, 848 |
| Tornea° - - - - - | 9 48, 95 | 9 38 | 8, 832 |
| Abo - - - - - | 9 11, 16 | 8 59 | 8, 801 |
| Hernosand - - - - - | 9 21, 17 | 9 01 | 8, 676 |
| Rodrigues - - - - - | 3 19, 72 | 2 13 | 5, 993 |
| Gottingen - - - - - | 7 54, 36 | 7 31 | 8, 558 |
| Bologna - - - - - | 7 03, 31 | 6 41 | 8, 525 |
| Florence - - - - - | 6 57, 79 | 6 36 | 8, 536 |
| Rome - - - - - | 6 45, 16 | 6 41 | 8, 907 |
| | | | Such |

Such is the result of a comparison of the best observations made in places whose longitudes are as accurately ascertained as the present state of Astronomy will permit: by a mean of the whole, rejecting only the observation at Rodrigues, the Sun's parallax on the day of the transit is $8',692$. — I have excluded the comparison of the observation at Rodrigues, because the parallax resulting from it differs so considerably from the rest. If we suppose the internal contact to have really happened one minute sooner, through a mistake in writing down the observation, the parallax will then be $8',697$.

This observation made at Rodrigues, supposing it exact, will furnish another term wherewith to compare the several observations made in Europe. The Sun's parallax resulting from each observation may be seen in the following table.

| Places. | Difference of calculated times. | Difference of observ'd times. | Sun's par- allax. | Difference of observed times. | Sun's parallax. |
|-----------------------------|---------------------------------------|-------------------------------------|----------------------|-------------------------------------|--------------------|
| Greenwich - - - - 4 28,00 | 5 11 | " 10,444 | 4 11 | " 8,429 | |
| Paris - - - - 4 8,98 | 4 50,5 | 10,500 | 3 50,5 | 8,332 | |
| Stockholm - - - - 5 39,00 | 6 28,5 | 10,314 | 5 28,5 | 8,721 | |
| Upsal - - - - 5 42,11 | 6 32 | 10,312 | 5 32 | 8,734 | |
| Cajaneburg - - - - 6 22,58 | 7 19 | 10,327 | 6 19 | 8,915 | |
| Tobolski - - - - 7 09,34 | 8 05,5 | 10,177 | 7 05,5 | 8,919 | |
| Tornea° - - - - 6 29,23 | 7 25 | 10,289 | 6 25 | 8,902 | |
| Abo - - - - 5 51,46 | 6 47 | 10,422 | 5 47 | 8,886 | |
| Hernosand - - - - 6 01,45 | 6 49 | 10,183 | 5 49 | 8,690 | |
| Göttingen - - - - 4 34,64 | 5 18 | 10,421 | 4 18 | 8,454 | |
| Bologna - - - - 3 43,59 | 4 33 | 10,787 | 3 33 | 8,372 | |
| Florence - - - - 3 38,07 | 4 23 | 10,854 | 3 23 | 8,449 | |
| Cape of Good Hope - 3 19,72 | 2 13 | 5,993 | 3 13 | 8,697 | |

The

The mean of the whole, rejecting the comparison of the Cape, is $10'',419$; supposing the internal contact to have happened at $0^h.36'.49''$. But if a mistake of one minute was really committed, the 3d column will receive a considerable alteration and the parallax resulting from each observation will be represented in the last column, the mean of which is $8'',654$, agreeing as nearly as possible with the parallax resulting from all the best observations compared with the Cape.

Mr. Pingre finding the parallax resulting from his own observation to differ so widely from that deduced from the Cape, and that both observations might be made to agree by supposing an error of one minute in the observation at Rodrigues, has examined every source of error that might be committed; and upon the whole sees reason to prefer his own observation to that of Mr. Mason, *not because he could find no mistake in his own, but because he has proved that no mistake could possibly be committed.* His observation indeed is in some measure confirmed by comparing all the observations with that at Lisbon: from which comparison if the longitude above laid down may be depended upon, the Sun's parallax is somewhat more than 10 seconds.

The several observations, that have been compared with the observations both of the Cape and Rodrigues, may also be compared together; and by combining some of them, we may obtain different results, upon which we may more or less depend, as the differences between the observed times are greater or less.

Places

| Places compared. | Difference of calculated times. | Difference of observ'd times. | Sun's par- allax. |
|--------------------------|---------------------------------------|-------------------------------------|----------------------|
| Tobolski and Greenwich | - 2 41, 34 | 2 54, 5 | 9.734 |
| Tobolski and Paris | - 3 0, 36 | 3 15 | 9.736 |
| Tobolski and Göttingen | - 2 34, 70 | 2 47, 5 | 9.744 |
| Tobolski and Stockholm | - 1 30, 34 | 1 37, 0 | 9.663 |
| Tobolski and Upsal | - 1 27, 23 | 1 33, 5 | 9.646 |
| Tobolski and Bologna | - 3 25, 75 | 3 37, 5 | 9.513 |
| Tobolski and Florence | - 3 31, 27 | 3 42, 5 | 9.525 |
| Stockholm and Greenwich | - 1 11, 0 | 1 17, 5 | 9.824 |
| Stockholm and Paris | - 1 30, 02 | 1 38, 0 | 9.797 |
| Stockholm and Bologna | - 1 55, 41 | 2 00, 5 | 9.396 |
| Stockholm and Florence | - 2 0, 93 | 2 05, 5 | 9.340 |
| Tornea° and Göttingen | - 1 54, 59 | 2 07 | 9.974 |
| Tornea° and Paris | - 2 20, 25 | 2 34, 5 | 9.914 |
| Tornea° and Greenwich | - 2 1, 23 | 2 14 | 9.948 |
| Cajaneburg and Greenwich | - 1 54, 58 | 2 08 | 10.054 |
| Cajaneburg and Paris | - 2 13, 6 | 2 28, 5 | 10.003 |
| Cajaneburg and Göttingen | - 1 47, 94 | 2 01 | 10.088 |
| Cajaneburg and Florence | - 2 44, 51 | 2 56 | 9.628 |
| Cajaneburg and Bologna | - 2 38, 99 | 2 51 | 9.679 |
| Upsal and Paris | - 1 33, 13 | 1 41, 5 | 9.808 |
| Upsal and Greenwich | - 1 14, 13 | 1 21 | 9.836 |
| Hernosand and Paris | - 1 52, 47 | 1 57, 5 | 9.402 |
| Hernosand and Greenwich | - 1 33, 4 | 1 37 | 9.342 |
| Hernosand and Bologna | - 2 17, 86 | 2 20 | 9.139 |
| Hernosand and Florence | - 2 23, 38 | 2 25 | 9.101 |
| Abo and Paris | - 1 42, 4 | 1 55, 5 | 10.145 |
| Abo and Greenwich | - 1 23, 44 | 1 33 | 10.031 |
| Abo and Bologna | - 2 07, 8 | 2 18 | 9.714 |
| Abo and Florence | - 2 13, 3 | 2 23 | 9.649 |
| Tornea° and Bologna | - 2 45, 64 | 2 57 | 9.617 |
| Tornea° and Florence | - 2 51, 16 | 3 02 | 9.569 |
| Greenwich and Paris | - 0 19, 02 | 0 20, 5 | 9.700 |

The mean of the whole is 9", 695.

| | | |
|--|---|--------|
| It has been shewn that the parallax result- | } | " |
| ing from the total durations — is | | 9, 579 |
| — from a comparison of the observation at Madras with those of Tobolski and Ca- | | 9, 763 |
| janeburg is — — — | | 9, 724 |
| — from the least distance of the centers — | | 9, 920 |
| — from the observations combined together is | | 9, 695 |

It can hardly be supposed that as such different methods give a parallax of the Sun on the day of the transit equal to $9'',736$, that this parallax should yet be only 8,692 as deduced from a comparison of the observations with the Cape, while the same observations compared with those of Rodrigues and Lisbon shew that the parallax exceeds 10 seconds. Let us therefore suppose that the observers at the Cape have set down their observation one minute too soon, tho' it must be confessed that the time of the duration at the egress cannot warrant such a correction, and that the time of the internal contact should have been observed at $21^{\text{h}}.40'.52''$; the parallax, by taking a mean, will then be $9'',732$, exactly agreeing with a mean of all the other determinations. And in this Quantity of the Sun's parallax we must either acquiesce, or remain as ignorant of the true quantity of it as we were before, till we can have recourse to the next transit on June 3d 1769, when the planet Venus will again pass over the Sun's disk, having something more

more than 10 minutes of North latitude ; and will be so favourably circumstanced, that, if the errors in observing each contact do not exceed 4" or 5", the quantity of the Sun's parallax may be determined within less than $\frac{1}{100}$ th part of the whole : as the total duration, or the interval between the two internal contacts, will be found to be about 18 minutes longer at Tornea^o than at Mexico. But the several circumstances of that transit must be the subject of a future paper. Let it suffice at present to observe that it will in part be visible to the inhabitants of this island, as Venus will be seen wholly entered upon the Sun's disk more than half an hour before the time of sun-set at Greenwich.

LVI. *A Discourse on the Locus for three and four Lines celebrated among the ancient Geometers, by H. Pemberton, M.D.R.S. Lond. et R.A. Berol. S. In a Letter to the Reverend Thomas Birch, D.D. Secretary to the Royal Society.*

S I R,

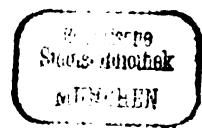
Dec. 15. 1763.

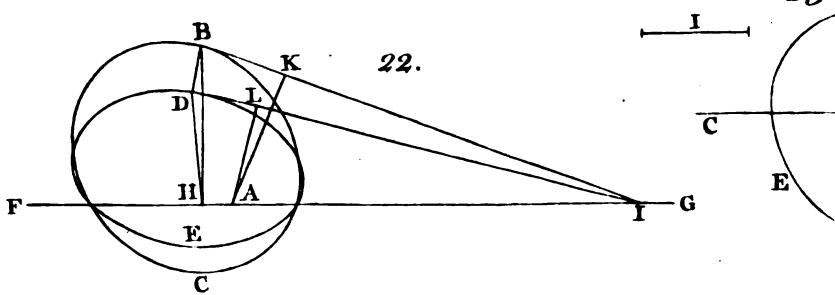
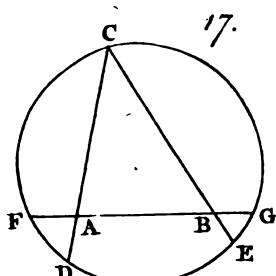
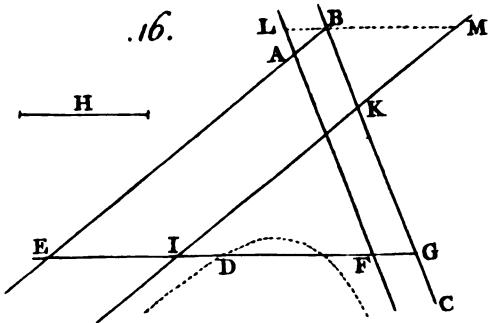
Read at R. S. 15 Dec. 1763. MY worthy friend, and associate in my early studies, the collector of the late Mr. Robins's mathematical tracts, thought it conducive to a more compleat vindication of the memory of his friend against an insinuation prejudicial to his candour, to make some mention of the course, I took in my early mathematical pursuits, and how soon I became attached to the ancient manner of treating geometrical subjects. This gave occasion to my looking into some of my old papers, amongst which I found a discussion of the problem relating to the *locus ad tres & quatuor lineas* celebrated among the ancients, which I then communicated to a friend or two, whose sentiments of those ancient sages were the same with mine. What I had drawn up on this subject is contained in the papers, I herewith put into your hands, which if you shall think worthy of being laid before our honourable society, they are intirely at your disposal.

I am your most obedient servant,

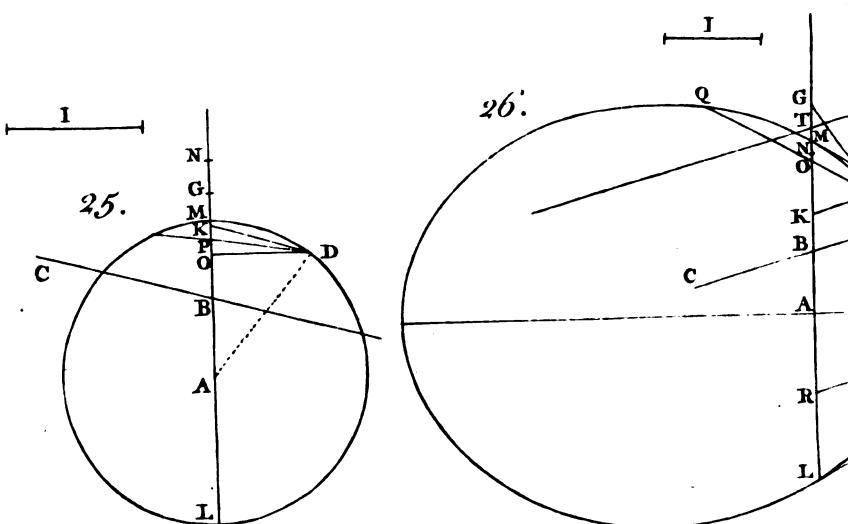
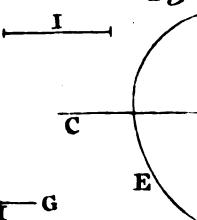
H. Pemberton.

T H E

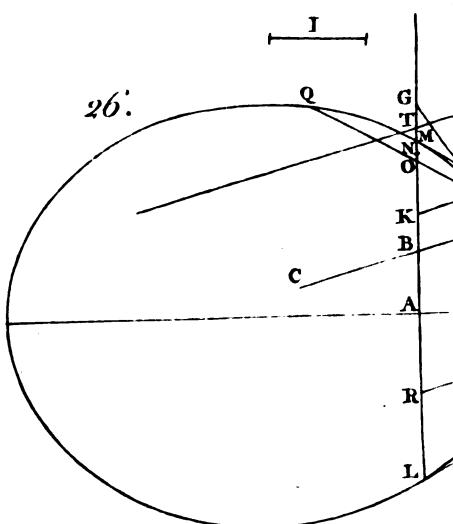




23.



26.



THE describing a conic section through the angles of a quadrilateral with two parallel sides is so ready a means of assigning *loci* for the solution of solid problems, that it cannot be doubted, but this gave rise to the general problem concerning three and four lines mentioned by Apollonius, and described by Pappus; and it may be learnt from Sir Isaac Newton, who has considered the problem, how easily the most extensive form of it is reducible to the case, which I have supposed to give rise to it.

Sir Isaac Newton refers the general problem to this: Any quadrilateral A B C D being proposed, to find the *locus* of the point P, whereby PRQ being drawn parallel to A C and S P T parallel to A B, the ratio of the rectangle contained under QP, PR to that under SP, PT TAB. XXIV. Fig. 1. shall be given; and this by pursuing the steps, whereby he proves, that the point P will in every quadrilateral be in a conic section, may be readily reduced to the case of a quadrilateral with two sides parallel, after this manner. Draw B_t and D_N parallel to A C, then find the point M in N D, that the rectangle under M D N be to that under A N B in the ratio given, and draw C_r M d.

Here R r will be to A Q, or SP, as M D to A N, and B_t, or QP, to T_t as N D to N B whence the rectangle under R r, QP will be to that under SP, T_t as that under M D N to that under A N B, that is, in the ratio given of the rectangle under R P Q to that under S P T. Therefore, by taking the sum of the antecedents and of the consequents,

consequents, the rectangle under rPQ will be to that under SPt , that is, to the rectangle under AQB , in the quadrilateral $ABCd$, whose two sides AC, Bd , are parallel, in the given ratio.

In like manner, if three of the given lines pass through one point, as the lines CA, CB, CD , and the rectangle under QPR be to that under SPt Fig. 2. in a given ratio, this case is with the same facility reduced to the like quadrilateral thus.

Draw BE parallel to AC , that shall cut ST produced in t , and let the point F be taken, that the rectangle under CA, EF be to the square of AB in the ratio given; then CrF being drawn, Bt , or QP , will be to Tt as AC to AB , and Rr to AQ , or SP , as EF to AB ; whence the rectangle under QP, Rr will be to that under Tt, SP , as that under AC, EF to the square of AB , that is, in the given ratio of the rectangle under QPR to that under SPt , and the rectangle under QP, Rr will be to that under SPt or AQB in the quadrilateral $ABCf$, whose two sides AC, BF are parallel, in the same given ratio.

Now let $ABCD$ be a quadrilateral having the two sides AC, BD parallel, with any conic section passing through the four points A, B, C, D ; Fig. 3, 4, 5. also, the point E being taken in the section, and EFG being drawn parallel to AC or BD , let the ratio of the rectangle under AGB to the rectangle under FEF be given: then the conic section will be given.

Let the sides AB, CD meet in M , and draw Ml bisecting AC and BD in K and L . Then the diameter

diameter of the section, to which $A C$ and $B D$ are lines ordinate applied, will be in the line $M I$; and if $N P$, $Q S$ are tangents to the section, and parallel to $A C$ and $B D$, the points O , R , in which they intersect $M I$, will be the points of their contact, and the vertexes of that diameter. But the square of $N O$ is to the rectangle under $A N B$, and the square of $Q R$ to the rectangle under $A Q B$, as the rectangle under $E G H$ or $F E G$ to that under AGB , therefore in a given ratio; but the ratio of $N M$ to $N O$, the same as that of $Q M$ to $Q R$, is also given; whence the ratio of the square of $N M$ to the rectangle under $A N B$, or of the square of $O M$ to the rectangle under $K O L$, is given, as likewise the ratio of the square of $R M$ to the rectangle under $K R L$.

Now in the ellipsis the square of $M O$, the distance of the remoter vertex of the diameter $O R$ from M , is greater than the rectangle under $K O L$; that is, the ratio given of the rectangle under $F E G$ to that under AGB must be greater than the ratio of the square of half the difference between $A C$ and $B D$ to the square of $A B$. But in the hyperbola the square of $M O$ is less than the rectangle under $K O L$; whereby the ratio of the rectangle under $F E G$ to that under AGB shall be less than that of the square of half the difference between $A C$ and $B D$ to the square of $A B$ [a].

[a] As the square of $O M$ shall be greater or less than the rectangle under $K O L$, the square of $N M$ will be respectively greater or less than the rectangle under $A N B$; therefore the ratio of the square of $N O$ to the rectangle under $A N B$, that is, of the rectangle under $F E G$ to that under AGB , will be accordingly greater

In both cases, if the point T be such, that the rectangle under M O T be equal to that under L O K, whereby M O shall be to O T in Fig. 3, 4. the given ratio of the square of M O to the rectangle under L O K, the given rectangle under K M L will be to the rectangle under L T K (by Prop. 35. L. 7. Papp. [b]) in this given ratio, and therefore given ; consequently the points T and O will be given.

In like manner, if the rectangle under M R V be equal to that under L R K, so that M R be to R V in the given ratio of the square of R M to the rectangle under L R K, the given rectangle under K M L (by Prop. 22. L. 7. Papp.) will be to the rectangle under L V K in the same given proportion, whence the points V and R will be given.

Thus in both cases the points T and V will be found by applying to the given line K L a rectangle exceeding by a square, to which the given rectangle under K M L shall be in the given ratio of the square of M O to the rectangle under K O L, or of the square of M R to the rectangle under K R L; M O being to O T, and M R to R V, in that given ratio.

But in the last place, if this given ratio be that Fig. 5. of equality, so that the square of R M be equal to the rectangle under K R L, by adding to both the rectangle under M R L, that under R M L will be equal to that under K M, L R, and M R to R L as K M to M L, and the vertex R of the diameter

greater or less than the ratio of the square of N O to the square of N M, which is the same with that of the square of the difference between A K, B L to the square of A B.

[b] See pag. 511.

R I

R I will be given, the conic section being here a parabola, this diameter having thus but one vertex.

Hitherto the point E, when the line E F G falls between A C and B D, is without the quadrilateral, and within the lines A B, C D, when E F G is without the quadrilateral.

But when E is within the lines A C, B D in the first case, and without in the second, the *locus* of the point E will be opposite sections, each passing through two angles of the quadrilateral.

When one section passes through A and C, and the other through B and D; then if the diameter M I be drawn, as before, and to K L be applied a rectangle deficient by a square, to which the given rectangle under K M L shall be in Fig. 6. the given ratio of the square of M O to the rectangle under K O L, or of the square of M R to the rectangle under K R L, the points T and V, constituting the rectangles under K T L and under K V L, being thus found, M O will be to O T, and M R to R V, in this given ratio (by prop. 30. L. 7. Papp.) O and T being the vertexes of the diameter M I.

But the rectangles under K T L, K V L cannot be assigned, as here required, unless the ratio given for that of the square of O M to the rectangle under K O L, or that of the square of R M to the rectangle under K R L, be not less than that of the rectangle under K M L to the square of half K L; that is, when the ratio of the square of O N to the rectangle under A N B, and that of the square of R Q to the rectangle under A Q B, or that of the given ratio of the rectangle under F E G to that under A G B is not less than that of the rectangle

under AK, BL to the square of half AB, or of the rectangle under AC, BD to the square of AB.

But if one of the opposite sections pass through A and B, and the other through C and D, the ratio of the rectangle under FEG to that under AGB will be less than that of the rectangle under

Fig. 7. AC, BD to the square of AB. For CL

being drawn parallel to AB, and AD joined and continued to M, the line DM falls wholly within the section passing through C and D : therefore KM is less than KL, and the ratio of KD to KL less than that of KD to KM, that is, of BD to AB ; whence BK being equal to AC, and CK to AB, the ratio of the rectangle under BKD to that under CKL, being the ratio of the rectangle under EGH, or FEG, to that under AGB, will be less than the ratio of the rectangle under AC, BD to the square of AB.

And here the point L is given ; for the given rectangle under BKD is to that under CKL in the given ratio of the rectangle under HGE, or that under FEG, to the rectangle under AGB ; hence CK, equal to AB, being given, KL is given, and consequently the point L.

Again, BL being joined, and NEOP drawn parallel to AB, also GEF continued to Q, as AG, equal to CQ, to FQ so will CK be to DK, and OP to EG, equal to OB, as KL to BK, consequently the rectangle under OP, AG will be to that under EG, FQ as that under KL, CK to that under KB, DK, that is, as the rectangle under AGB to that under FEG ; and by combining the antecedents and consequents the rectangle under PEG will be to that under QEG in the same given ratio.

More-

Moreover DK being to AC as KM to CM , the ratio of DK to AC , that is, the ratio of the rectangle under BKD to the square of AC , will be less than the ratio of KL to CL , or the ratio of the rectangle under CKL to that under AB, CL ; therefore, by permutation and inversion, the ratio of the rectangle under CKL to the rectangle under BKD , that is, the given ratio of the rectangle under NEP to that under ANC , equal to that under GEQ , is greater than the ratio of that under AB, CL to the square of AC . And hence the opposite sections passing through the angles of the quadrilateral $ABCL$, whose sides AB, CL are parallel, will be given as before.

When the given ratio of the square of OM to the rectangle under LOK shall be that of Fig. 6. the rectangle under KML to the square of half KL , whereby the given ratio of the rectangle under FEG to that under AGB shall be that of the rectangle under AC, BD to the square of AB , the points T and V shall unite in one, bisecting KL , and the points O and R shall also unite in one, dividing the line KLM harmonically; and then the *locus* of the point E will be each of the diagonals of the quadrilateral.

In the last place, if the diagonals AD, BC of the quadrilateral were drawn, cutting GE Fig. 8. in I and K , and the ratio of the rectangle under KEI to that under AID were given, and not that of the rectangle under GEF to that under AGB ; then the intersection of these diagonals, as L , will be in the line drawn from M bisecting AC , and BD , and the point L will fall within the quadrilateral, whereby the *locus*, when an

T t t 2

ellipsis

ellipsis or single hyperbola, will be assigned by the 36th proposition of the foresaid book of Pappus: and when opposite sections, by the 30th proposition, or be reduced to the preceding cases thus.

Since KG will be to GB as CA to AB , and IG to GA as BD to AB , the rectangle under KGI will be to that under AGB , in the given ratio of the rectangle under AC , BD to the square of AB . Therefore when the ratio of the rectangle under KEI to that under AID is given, the rectangle under AID also bearing a given ratio to that under AGB , the ratio of the rectangle under KEI to that under AGB will be given, and in the last place the ratio of the rectangle under GEF to that under AGB will be given, this rectangle under GEF being the excess of that under KGI above that under KEI [c]. And thereby the sections will be determined, as before.

AND thus may the *locus* of the point sought be assigned in all the cases of this ancient problem, which Sir Isaac Newton has distinctly explained. The other cases, he has alluded to, may be treated, as follows.

When three of the given lines shall be parallel, as AC , BD , and HI , the fourth line being AB , and $KELM$ being parallel to AB , the Fig. 9. ratio of the rectangle under KEL to the rectangle under EG and EM shall be given; that is, three points A , B , and H being given in the line AB , with the line GE insisting on AB in a given angle, that the rectangle under AGB shall be to that under GH and GE in a given ratio: then

[c] By Prop. 193. Lib. 7. Papp.

take

take A N equal to B H, and draw N O parallel to A C, B D, and H I.

Then if N P be drawn, that P O be to O N in the given ratio, N P will be given in position, and P O will be to O N, that is, E G, as the rectangle under K E L to that under E M, E G; so that the rectangle under K E L will be equal to that under P O, E M. But the rectangle under O K M is equal to the excess of that under O E M above that under K E L [d]; therefore the rectangle under O K M, or that under N A H, or under N B H, is equal to that under E M and the excess of O E above O P, that is, to the rectangle under P E M; the point E therefore is in an hyperbola described to the given asymptotes P N, M H, and passing through A and B.

Again if two of the given lines only are parallel, but the rectangles otherwise related to them, than as above. Suppose the ratio of the rectangle under A G, E F to that under B G, G E is given. Let C D meet A B in L, and let H E I, M F N be drawn parallel to A B, and L K parallel to A C and B D. Then the parallelogram E M will be to the parallelogram E B in the given Fig. 10. ratio. Take A O to O B in that ratio, and draw O P parallel to A C and B D. Here the point O will be given, and the parallelogram P A will be in the given ratio to the parallelogram P B; whence A B will be to B O as the parallelogram B H to the parallelogram B P, and as the difference between the parallelograms E M and E B to the parallelogram E B, consequently as the parallelogram G M to the parallelogram P G; therefore the ratio of the rectangle under A G, F G to the rectangle under

[d] By Prop. 194. Lib. 7. Papp.

E G,

$E G$, $E P$ or $O G$ will be given; and in the last place the ratio of $F G$ to $G L$ being given, the ratio of the rectangle under $A G$ and $G L$ to that under $E G$, $O G$ will be given. And thus three points A , L , O , will be given with $G E$ insisting on $A B$ in a given angle, as in the preceding case.

Moreover, $A C$ and $B D$ being parallel, $A B$ and $C D$ may be also parallel. And then, when the ratio of the rectangle under $A G B$ to that under $G E F$ Fig. 11. is given, the determination of the *locus* is so obvious as not to have required a distinct explanation. But when the rectangle under $A G$, $E F$ bears a given ratio to that under $B G$, $G E$; let the diagonals $A D$, $B C$ be drawn, and $H E L K$ drawn parallel to $A D$. Then the rectangle under $H E L$ will be to that under $K E I$ in the same given ratio; and if $C M$ be taken to $M B$ in the same ratio, the lines $M N P$, $M O Q$ drawn, the first parallel to $A C$, $B D$, and the other parallel to $A B$, $C D$, will be given in position, and the diagonal $B M$ will bisect both $I K$, $N O$, and $H L$; therefore the rectangle under $H E L$ being to that under $K E I$ as $M C$ to $M B$, that is, as $N H$ to $N K$, hereby division the rectangle under $H E L$ will be to that under $I H K$ [e] as $N H$ to $H K$; therefore equal to that under $N H$ and $I H$ or $K L$. But the rectangle under $N E O$ is equal to the sum of the rectangles under $H N L$ and under $H E L$ [f]; therefore the rectangle under $N E O$ is equal to that under $N H$, $N K$, equal to that under $A P D$, that is, equal to that under $P A Q$, or that under $P D Q$, the diagonal $B M$ bisecting both $P Q$ and

[e] By the prop. of Papp., before cited. [f] By the same.
A D.

A D. But thus the point E is in an hyperbola described to the asymptotes MN, MO, and passing through A and D.

THE determination of this *locus* for three lines is solved almost explicitly by Apollonius in the three last propositions of his third book of Conics. For if the three lines proposed were AB, AC, BC, and the point sought D, that the ratio of the rectangle under EDF (the line EF being drawn parallel to BC) should be in a given ratio to the square of a line drawn from D to BC in a given angle, the square of which line will be in a given ratio to the rectangle under BE, CF; then if BH, CI are drawn parallel to AC and AB respectively, also BDL, CDK drawn through D, the square of BC will be to the rectangle under BK, CL as the rectangle under DF, DE, to that under CF, BE.

Hence if the ratio of the rectangle under DF, DE to the square of a line drawn from D on BC in a given angle, is given; the square of this line being in a given ratio to the rectangle under CF, BE, the ratio of the rectangle under BK, CL to the square of BC will be given; whence a conic section passing through D will in all cases be given.

In the first place let the point D be within the angle BAC. Then if BC be bisected by the line AM, this will be a diameter to the conic section, which shall touch BA, AC in the points B, C, and BC will be ordinately applied to that diameter; the vertex of this diameter being N, the given ratio of the rectangle under BK, CL to

the square of BC will be compounded of the ratio of the square of MN to the square of NA, and of the ratio of the rectangle under BAC to the fourth part of the square of BC; and thus the line AM will be divided in N in a given ratio, and the point N, one vertex of the diameter, to which BC is ordinately applied, will be given.

If AN be equal to NM, the point N will be the only vertex of this diameter, and the section will be a parabola.

Otherwise by taking the point O in AM extended, so that the ratio of AO to OM be the same with that of AN to NM, the point O will be the other vertex of the diameter.

And here if the ratio of AN to NM be that of a greater to a less, the point O will fall beyond M from A within the angle BAC, the conic section being an ellipsis.

But if the ratio of AN to NM be that of a less to a greater, the point O will fall on the other side of A, and the section will be an hyperbola.

Fig. 13. And in this case if the opposite section be drawn, that also will be the *locus* of the point D within the angle vertical to the angle BAC.

In the last place, if D be in either of the collateral angles, AM drawn as before will contain a secondary diameter in opposite sections, one of which Fig. 14. shall touch BA in B, and the other CA in C. Then if one of these sections pass thro' D, the sections will be given. For here PAQ being drawn through A parallel to BC, the given ratio of the rectangle under CL, BK to the square of

of BC will be the same with that of the given rectangle under BAC to the square of AP: therefore AP is given, and thence the sections. For let RS be the secondary diameter, to which BC is ordinarily applied, and T the center of the opposite sections. Then the square of BM will be to the rectangle under AMT as the square of the transverse diameter conjugate to the secondary diameter RS to the square of this secondary diameter; and if a line were drawn from M to P, this would touch the hyperbola BP in P [g], and the square of AP will be to the rectangle under MAT in the same ratio; therefore the given ratio of the square of MB to the square of AP will be that of the rectangle under AMT to the rectangle under MAT, or the ratio of MT to AT; consequently the ratio of MT to AT is given, and thence the point T. But also the diameter RS is given in magnitude, the square of RT or of ST being equal to the rectangle under MTA; whence in the last place the transverse diameter conjugate to this is also given; for the square of this diameter is to the square of RT as the given square of BM to the rectangle under AMT now also given.

But a more simple case may also be proposed in three lines, when the ratio of the rectangle under EDF should be equal to the rectangle under a given line, and that drawn from D to BC in a given angle. Fig. 15.

This line will bear, both to BE and FC, a given ratio, and the rectangle under EDF will be in a given

[g] Apoll. conic. L. II. prop. 40.

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ratio to the rectangle under the given line and $E B$ or $C F$.

Let the line given be H , and take $M B$ and $N C$, that the rectangle under $M B C$, and that under $B C N$ be to that under $B A$ and H in the given ratio of the rectangle under $E D F$ to that under $B E$ and H , $B M$ and $C N$ being equal. Then draw from M and N lines parallel to $B A$, $C A$, which shall intersect $E F$ in K and L , whereby, $M K$ cutting $C A$ in I , the rectangle under $M B C$ will be to that under $B A$ and H as the rectangle under $B M C$ to that under $M I$ and H , and also as the rectangle under $E K F$ to that under $K I$ and H , that is, as the rectangle under $E D F$ to that under H and $B E$ or $M K$, whence by adding the antecedents and consequents the rectangle under $K D L$ will be to the rectangle under H and $M I$ in the same given ratio, which is also that of the rectangle under $B M C$ to the same rectangle under H and $M I$: the point D therefore is in an hyperbola passing through B and C having for asymptotes the lines $M K$ and $N L$ given in position, the rectangle under $K D L$ being equal to that under $B M C$, or that under $M B N$.

If the two lines $A B$ and $A C$ are parallel, the *locus* may be known to be a parabola by the last proposition of the fourth book of Pappus.

But if $B C$ were parallel to one of the other, the *locus* will be an hyperbola, as the preceding, but assigned by a shorter process.

Suppose the given lines to be $A E$, $A F$,
 Fig. 16. and $B C$ parallel to $A F$. And let the rectangle under $E D F$ be equal to that under $D G$, and the given line H , the line $E G$ making given angles with $A E$, $A F$. Here take $E I$ equal to H ,

and deduct from both the rectangles that under E I or H, and DF, whereby will be left the rectangle under IDF equal to that under H and FG, both whose sides are given. Draw therefore IK parallel to AE, and the rectangle under IDF will be equal to this given rectangle, the given lines KI, AF being the asymptotes to the hyperbola passing through D.

Coroll. If LM be drawn through B parallel to EF, LB shall be equal to FG, and BM equal to EI or H, whereby the hyperbola opposite to that passing through D will pass through B.

S C H O L I U M.

The propositions of Pappus, which have been referred to in pag. 500, 501, 504, l. 2. are given by him, among others, for Lemmas subservient to the lost treatise of Apollonius *De sectione determinata*, and the four here cited respect and comprehend all the cases of the problem, where three points are given in any line, and a fourth is required such, that the rectangle under the segments of the proposed line intercepted between the point sought, and two of the given points, shall bear a given ratio to the square of the segment terminated by the third point.

The cases indeed of the problem, from the diversity of situation in the points given to the point sought and to one another, are in number six. The given extreme of the segment to constitute the square may either be without the other two given points, or between them. And when it is without, the point sought may be required to be taken without them all, either on the side opposite to the given extreme of the segment to constitute the square, which will be one case, or it may be required to fall on the same side,

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which will be a second case. If it be required to fall between this point and the other two, this will be a third case. A fourth case will be, when the point sought shall be required to fall between the other two points. Also when the given extreme of the segment to constitute the square lies between the other two given points, the point sought may be required to fall, either there also, or without, composing the 5th, and 6th cases.

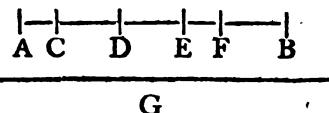
The propositions in Pappus referring to these cases, though but four in number, suffice for them all, each proposition being applicable to the problem two ways. For instance the thirty-fifth proposition, as expressed by Pappus, is this, being the first above cited. Three points C, D, E being taken in the line A B, so that the rectangle under A B E be equal to that under C B D, A B is to B E as the rectangle under D A C to that under C E D. Now A B is to B E, both as the square of A B to the rectangle under A B E, and as the rectangle under A B E to the square of B E. Therefore if the ratio of A B to B E be given, the ratio of the square of A B to the rectangle under C B D will be given, which is the first of the cases above described, and also the ratio of the rectangle under C B D to the square of B E given, which is the second case. In both cases the rectangle under D A C will be to that under C E D in the given ratio of A B to B E. But in the first the rectangle under D A C will be given, and the point E in the rectangle under C E D to be found by applying a rectangle, which shall bear a given ratio to the given rectangle under D A C to the given line C D exceeding by a square; and in the second case the rectangle

rectangle under C E D is given, and A in the rectangle under D A C to be found by applying to the given line C D a rectangle exceeding by a square, which shall bear a given ratio to the rectangle under C E D now given; whence by the ratio of A B to B E given the point B will be found in both cases.

The 22d proposition either way applied refers to the 3d case only, the 30th relates both to the 4th and 5th, and the 36th proposition to the remaining 6th.

The 45th, and other following propositions, are accommodated to the solution of Apollonius's problem, when four points are given, and a fifth required, which with the given points shall form four segments such, that the rectangle under two shall bear a given proportion to the rectangle under the other two. The various cases of this problem appear to have been the subject of the second book of the mentioned treatise of Apollonius; and, according to the character given by Pappus of those propositions, these lemmas serve to reduce them to problems in the first book, not those above mentioned, but those, where three points being given, the rectangle under the segments included by two, and a fourth point shall bear a given proportion to the rectangle under the segment formed by the third point and a given line.

For instance the 46th proposition is this; in the line A B four points A, C, E, B being given; and the point F assumed between E and B;



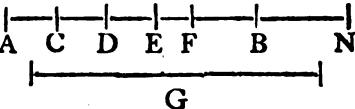
also D taken, according to the 41st proposition, that the rectangle under A D C be equal to that under B D E; if G be equal to the sum of A E, C B, the rectangle

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gle under AFC together with that under EFB will be equal to the rectangle under G and DF.

Here if it were proposed to find the point F, that the ratio of the rectangle under AFC to that under EFB should be given, the ratio of the rectangle under AFC to that under DF and the given line G would be given.

But this analysis may be carried on to a compleat solution of the problem thus. If CN be taken to G in the given ratio of the rectangle under AFC to that under DF and G, the point N will be given, and the rectangle under A F, CN will be to



that under AF, G in this ratio of CN to G; consequently the excess of the rectangle under AF, CN above that under AFC, that is, the rectangle under AFN, will be to the excess of the rectangle under AF and G above that under DF and G, or the given rectangle under AD, G, in the same given ratio, and in the last place the rectangle under AFN will equal the given rectangle under AD and CN.

Here I have chosen this proposition in particular, because the case of the problem, to which it is subservient, is subject to a determination, when FN shall be equal to AF. And then the rectangle under AFN being equal to that under AD and CN, as CN to FN so is AF to AD, and by division as CF to FN so DF to AD; therefore when AF is equal to FN, CF will be to AF as FD to AD: consequently CD to FD as FD to AD, and the square of DF equal to the rectangle under ADC, when the problem admits of a single solution only, wherein the rectangle under AFC

AFC will bear to that under EFB a less ratio than in any other situation of the point F between E and B.

Moreover CN is to G as the rectangle under AFC to the sum of the rectangles under AFC and EFB; therefore FN being equal to AF, when the problem is limited to this single solution, the rectangle under AFC shall be to the rectangles under AFC and EFB together as the sum of AF and FC to G, which is equal to the sum of AE and CB; whence by division the ratio of the rectangle under AFC to that under EFB, when the problem is limited to this single solution, will be that of the sum of AF and CF to the excess of FB above EF.

Thus directly do these lemmas correspond with Apollonius's first mode of solution, and lead to the general principle of applying to a given line a rectangle exceeding or deficient by a square, which shall be equal to a space given. This being a simple case of the 28th and 29th propositions of the 6th book of Euclid's elements, admits of a compendious solution. Such a one is exhibited by Snellius in his treatise on these problems (in Apollon. Batav.) and Des Cartes has exhibited another more contracted in it's terms, but not therefore more useful. It may also be performed thus. If upon a given line AB any triangle ACB be erected at pleasure; then if the legs CA, CB, whether equal or unequal, be continued to Fig. 17. D and E, that the rectangles under CAD and CBE be each equal to the given space; and a circle be described through C, D, E cutting AB extended in F and G, the rectangle under BFA and BGA will each be equal to Fig. 18. the space given. Also if in the legs CA, CB the rectangles under CAD and CBE be each taken.

taken equal to the space given, and a circle in like manner be described through C, D, E, cutting A B in F and G, the rectangles under A F B and A G B will each be equal to the given space. Here it is evident, that the space given must not exceed the square of half A B, when equal, the circle will touch A B in its middle point.

P O S T S C R I P T.

AS this application to a given line of a rectangle exceeding or deficient by a square, or the more general problem treated of in the sixth book of the elements, of applying a space to a line so as to exceed or be deficient by a parallelogram given in species, is the most obvious result, to which the analysis of plane problems, not too simple to require this construction, leads; so the descriptions of the conic sections here treated of, stand in the like stead in regard to the higher order of problems styled solid from the use of the conic sections deemed necessary for their genuine solution. And these are the only modes of solution, the modern algebra, which grounds its operations on one or two elementary propositions only, naturally leads to. But as the form of analysis amongst the antients, by expatiating through a larger field, often was found to arrive at conclusions much more concise and elegant, than could offer themselves in a more confined track; the antient sages in geometry, that the solid order of problems might not want this advantage, sought out that copious and judicious collection of properties attending the conic sections, which

which, with some useful additions from later writers, have been handed down to us.

And as the advantages of this ancient system of analysis cannot be too much inculcated in an age, wherein it has been so little known, and almost totally neglected, permit me, Sir, to close this address to you with an example in each species of problems.

Were it proposed to draw a triangle given in species, that two of its angles might touch each a right line given in position, and the third angle a given point. It is obvious, how difficult it would be to adopt a commodious algebraic calculation to this problem; notwithstanding it admits of more than one very concise solution, as follows.

Let the lines given in position be A B, Fig. 19, A C and the given point D, the triangle Fig. 20, 21. given in species being E D F.

In the first place suppose a circle to pass through the three points A, E, D, which shall intersect A C in G. Then E G, D G being joined, the angle D E G will be equal to the given angle D A C, both insisting on the same arch D G; also the angle E D G is the complement to two right of the given angle B A C: these angles therefore are given, and the whole figure E F G D given in species. Consequently the angle E G F, and its equal A D E will be given together with the side D E of the triangle in position.

Again, suppose a circle to pass through the three points A, E, F, cutting A D in H, and Fig. 20. E H, F H joined. Here the angle E F H will be equal to the given angle E A H, and the an-

gle F E H equal to the given angle F A H. Therefore the whole figure E H F D is given in species, and consequently the angle A D E, as before.

In the last place suppose a circle to circumscribe Fig. 21. the triangle, and intersect one of the lines, as A C, in I. Here D I being drawn, the angle D I F will be equal to the given angle D E F in the triangle; consequently D I is inclined to A C in a given angle, and is given in position, as also the point I given; whence I E being drawn, the angle F I E will be the complement of the angle E D F in the triangle to two right. Therefore I E is given in position, and by its intersection with the line A B gives the point E, with the position of D E, and thence the whole triangle, as before.

Here it may be observed, that the angle D of the triangle E D F given in species touching a given point D, and another of its angles touching A C, the line I E here found is the locus of the third angle E.

Again, in the astronomical lectures of Dr. Keil, it is proposed to find the place of the earth in the ecliptic, whence a planet in any given point of its orbit shall appear stationary in longitude, and a solution is given from the late eminent astronomer, Dr. Halley, upon the assumption, that the orbit of the earth be considered as a circle concentric to the Sun.

But for a compleat solution of this problem let the following lemma be premised.

The velocity of a planet in longitude bears to the velocity of the earth the ratio, which is compounded of the subduplicate ratio of the *latus rectum* of the greater axis of the planet's orbit to the *latus rectum* of

of the greater axis of the earth's orbit, of the ratio of the cosine of the angle, which the orbit of the planet makes with the plane of the ecliptic, to the radius, and of the ratio of a line drawn in any angle from the center of the sun to the tangent of the orbit of the earth at the point, wherein the earth is, to a line drawn in the same angle from the sun to the tangent of the orbit of the planet projected upon the plane of the ecliptic at the place of the planet in the ecliptic.

Let A be the sun, BC the orbit of any planet, DE the same projected on the plane of the ecliptic, FG being the line of the nodes, B the place of the planet in its orbit, D its projected place: then the plane through B and D, which shall be perpendicular to both the planes BC and DE, intersecting those planes in BH, DH, the lines BH, DH will be both perpendicular to the line of the nodes, and the angle BHD the inclination of the orbit to the plane of the ecliptic. But tangents drawn to BC and DE at the points B and D respectively will meet the line of the nodes, and each other in the same point I, and the velocity of the planet in longitude will be to its velocity in the orbit BC, as DI to BI.

Now from the point A let AK fall perpendicular on BI, and AL be perpendicular to DI: then the ratio of DI to IB will be compounded of the ratio of DI to DH, or of AI to AL, of the ratio of DH to BH, and of that of BH to BI, that is, of AK to AI. But DH is to BH as the cosine of the inclination of the orbit to the radius, and the two ratios, that of AI to AL, and that of AK to AI, compound the ratio of AK to AL: therefore

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the velocity of the planet in longitude is to the velocity in its orbit in the ratio compounded of that of the cosine of the inclination of the planet's orbit to the radius, and that of A K to A L.

Moreover the ratio of the velocity of the planet in B to the velocity of the earth in any point of its orbit is compounded of the subduplicate of the ratio of the *latus rectum* of the greater axis of the planet's orbit to the *latus rectum* of the greater axis of the earth's orbit, and of the ratio of the perpendicular let fall from the sun on the tangent of the earth's orbit at the earth to A K, the perpendicular let fall on the tangent of the planet's orbit at B. Therefore the velocity of the planet in longitude, when in B, to the velocity of the earth in any point of it's orbit is compounded of the subduplicate ratio of the *latus rectum* of the greater axis of the planet's orbit, to the *latus rectum* of the greater axis of the earth's orbit, of the ratio of the co-sine of the inclination of the planet's orbit to the radius, and of the ratio of the foresaid perpendicular on the tangent of the earth's orbit to A L, the perpendicular on D I : these perpendiculars being in the same ratio with any lines drawn in equal angles to the respective tangents.

This being premised, the place of a planet in the ecliptic being given, the place of the earth, whence the planet would appear stationary in longitude, may be assigned thus.

A denoting the sun, let B be a given place of any planet in it's orbit projected orthographically on the plane

Fig. 23. of the ecliptic, CB the tangent to the planet's projected orbit at the point B, which will therefore be given in position. Also let DE be the

the orbit of the earth; and the point D the place of the earth, whence the planet would appear stationary in longitude at B.

Join AB, and draw a tangent to the earth's orbit at the point D, which may meet CB in F, and the line AB in G; draw also AH making with DF the angle AHD equal to that under ABC. Then the point D being the place, whence the planet appears stationary in longitude, as FB to FD so will the velocity of the planet in longitude in B be to the velocity of the earth in D, this velocity of the planet in B being also to the velocity of the earth in D in the ratio compounded of the subduplicate of the ratio of the *latus rectum* of the greater axis of the planet's orbit, to the *latus rectum* of the greater axis of the orbit of the earth, of the ratio of the co-sine of the inclination of the planet's orbit to the plane of the ecliptic to the radius; and of the ratio of AH to AB: therefore the ratio of FB to FD will be compounded of the same ratios; and if I be taken, that the ratio of AB to I be compounded of the two first of these, I will be given in magnitude, and the ratio of FB to FD will be compounded of the ratio of AB to I, and of AH to AB: Whence FB will be to FD as AH to I; and the angles CBA, or FBG, and AHG being equal, whereby FG will be to FB as AG to AH, by equality FG will be to FD as AG to I, and DK being drawn parallel to FB, BG will be to BK as FG to FD, and therefore as AG to I.

But now, as this problem may be distributed into various cases, in the first place consider the earth as moving in a circle concentric to the sun, and likewise CB, the tangent to the planet's orbit, perpendicular to AB.

But

But here DK also will be perpendicular to AB , and AB meeting the earth's orbit in L and M , the rectangle under KA G will be equal to the square of AM . But BG being to BK as AG to I , if BN be taken equal to I , BG will be to BK as AG to BN , and AB to KN also as AG to BN , and the rectangle under NK , AG equal to that under AB and I : therefore the rectangle under KA G being equal to the square of AM , NK will be to KA as the rectangle under AB , I to the square of AM , that is, in a given ratio, and KD with the point D will be given in position.

Again, when CB is not perpendicular to LM , let DO be perpendicular to LM . Then the rectangle under OAG will be equal to the square of AM . But BN being taken equal

Fig. 25. to I , as before, the rectangle under NK , AG will be equal to that under AB , I ; whence NK will be to AO in the given ratio of the rectangle under AB , I to the square of AM . Therefore NP being taken to PA in that ratio, the point P will be given, and KP , the excess of NP above NK , will be to PO , the excess of AP above AO , in the same ratio. Hence, as DK is parallel to CB and DO perpendicular to LM , the triangle KOD is given in species, and if PD be drawn, the angle OPD will be given; for the co-tangent of the angle OKD will be to the co-tangent of the angle OPD , as KO to OP , that is, as the rectangle under AB , I together with the square of AM to the square of AM , and hence the point D is given by the line PD drawn from a given point P in a given angle APD ; and if AD be drawn, AD will be to AP as the sine of the angle APD to the sine of the angle PDA ; this angle therefore

therefore is given, and the angles A P D, P D A being given, the angle P A D is given.

Coroll. Here, where the orbit of the earth is supposed a circle, the ratio of I to A B, that is, of the rectangle under A B, I to the square of A B, will be compounded of the subduplicate ratio of A M, the semidiameter of the earth's orbit, to half the *latus rectum* to the greater axis of the planet's orbit, and of the ratio of radius to the co-sine of the inclination of the planet's orbit to the plane of the ecliptic ; and adding on both sides the ratio of the square of A B to the square of A M, the ratio of the rectangle under A B, I to the square of A M will be compounded of the ratio of the square of A B to the rectangle under A M and the mean proportional between A M and the half of this *latus rectum* of the planet's orbit, and of the ratio of the radius to the co-sine of the inclination of the net's orbit.

In the next place, though the earth's orbit is not a circle concentric to the sun ; yet if the projection of the planet falls on the line perpendicular to the axis of the earth's orbit, the point A will still bisect L M.

In this case draw to the points L and M tangents to the ellipsis meeting in P, from whence through D draw P D meeting the ellipsis again in Q, and intersecting L M in O. Here if a tangent be drawn to the ellipsis in Q, it will meet the tangent at Fig. 26. D on the line L M in the point G.

Now L G will be to G M as L O to O M, and the point A bisecting L M, the rectangle under G A O will be equal to the square of A M. But B G is to B K as A G to I. Therefore B N being taken equal to I, A B will be to K N as A G to I, and the rectangle under A B, I equal to that under A G,

KN : whence AO being to KN as the rectangle under GAO to that under AG and KN , AO will be to KN as the given square of AM to the rectangle under AB and I , also given.

Draw RP parallel to CB , and take PS to AP , also NT to AR in this given ratio inverted. Then will the points T and S be both given, also AO will be to KN , and RO to KT , as AR to NT , that is, as AP to PS . Therefore if TV be drawn parallel to CB , that is, to KD , and VS parallel to LM , these lines will be both given in position; and $WDXY$ being also drawn parallel to LM , WD will be equal to KT , and RO being to KT , as AP to PS , DY will be to WD as XP to PS , and by composition YW to WD as XS to PS , and the given rectangle under YW , or SV , and PS equal to that under WD , and XS . Whence SV being parallel to LM , the point D will be in an hyperbola passing thro' P , and having for asymptotes the lines VS , VT given in position.

But if the projection of the planet fall on the axis of the earth's orbit, or the same continued, AB extended to the earth's orbit in L and M will be the axis of that orbit.

If also CB should be perpendicular to AB , KD would be ordinate applied to LM ; and Fig. 27. the point R being taken, that Q being the center of the orbit, the rectangle under AQR be equal to the square of QM , the same will be equal also to the rectangle under GQK ; whence as GQ to AQ so RQ to QK , and AG to AQ as KR to QK . But, as above, BG being to BK as AG to I , and BN taken equal to I , BG will be to BK as AG to BN , and AB to KN also as AG to BN or I . Therefore if NS be taken to AB as I to AQ , by equality

equality NS will be to NK as AG to AQ , that is, as KR to QK ; and in the last place NS to KS as KR to QR , that is, the rectangle under SKR equal to the given rectangle under NS, QR ; whence the point K , the position of KD , and thence the point D will be given.

But if DK be not ordinitely applied to LM , let DO be ordinitely applied to LM . Then here the rectangle under AQR , equal to the square of QM , will be equal to that under OQG , and GQ to AQ as QR to OQ , whence by Fig. 28. composition AG to AQ as OR to OQ . But BN being now also taken equal to I , and NS to AB as I to AQ , AB will be here in like manner to KN as AG to I , and NS to KN as AG to AQ : therefore NS will be to KN as OR to OQ , and by conversion NS to KS as OR to QR . But NS and QR being both given in magnitude, if SP be taken to NS as QR to PR , the point P will be given, and also by equality SP will be to KS as OR to PR ; whence if RV be drawn parallel to DO , and ST to KD , both RV and ST will be given in position, one passing through the given point R , parallel to the ordinates applied to the axis LM , and the other through the point S also given, and parallel to KD or CB : also DTV being drawn parallel to ML , DT will be equal to KS and DV equal to OR , therefore as SP to DT so DV to PR , and the rectangle under SPR equal to that under TDV , consequently the point D in an hyperbola passing thro' P , and having for asymptotes the lines ST, RV given in position.

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In

In the last place when the line L M drawn through the sun in A, and the projected place of the planet in B, is neither the axis of the earth's orbit, nor bisected in A, the tangents to the points L, M

Fig. 29. being drawn to meet in P, let LM be bisected in Q, and the point R taken, that the rectangle under AQR be equal to the square of QM, whereby PDO being drawn, the rectangle under AQR shall be equal to that under OQG, and QG to AQ as QR to QO, or by composition AG to AQ as OR to QO. Therefore if NB be here also taken equal to I, and NS to AB as I to AQ, AB being as before, to NK as AG to I; by equality NS will be to NK as AG to AQ, that is, as OR to QO. Whence by conversion NS will be to KS as OR to QR; and if PT be drawn parallel to CB and SV be here taken to NS as QR to TR, by equality SV will be to KS as OR to TR and also by conversion SV to KV as OR to OT. Moreover SV will be given in magnitude, and the point V given; therefore VW drawn parallel to CB, or KD, will here be given in position. But WDXY being also drawn parallel to RV, SV will be to KV, or DW, as YD to XD, and YZ being taken equal to the given line SV, YZ will be to DW as ZD to XW, equal to TV, and the given rectangle under YZ, TV equal that under WDZ. Therefore Γ Z being drawn parallel to RP, R Γ , and its equal YZ, being given, the line Γ Z is given in position, and the point D in an hyperbola having for asymptotes VW, Γ Z, and passing through P.

Thus is this problem in all cases solved either by a right line, or an hyperbola given in position, which shall intersect the projected orbit in the point sought. For though in each case the projection of the planet has here been considered as within the orbit of the earth, the form of argumentation will be altogether similar, were the projection of the planet without. And this is agreeable to the method, I have pursued throughout this discourse, where I have always accommodated the expression to one situation only of the terms given and sought in each article; the variation necessary for the other cases, when one has been duly explained, being sufficiently obvious.

In the 5th volume of the Commentaries of the Royal Academy at Petersbourg is given an algebraical computation for a general solution of this problem in the orbits of any two planets projected on the plane of the ecliptic; but with this oversight of applying to the projected orbits a proposition from Dr. Keil's Astronomical Lectures, which relates to the real orbits (*a*).

However from the geometrical solution now given a calculation for assigning the point D may be formed without difficulty. L D M being the orbit of the earth, A is the focus, and R P perpendicular to the

(*a*) The demonstration of Dr. Keil's proposition proceeds on the known property in the planets of having their periodic times in the sesquiplicate ratio of the axes of their orbits, which confines the proposition to the real orbits; for in each planet the periodic time through the projected orbit is the same, as through the real, though the axis in one be not equal to the axis of the other.

Y y y 2

axis.

axis. Let this axis be ab meeting $R P$ in c , ΓZ in d , $P T$ in e and $W V$ in f . Then the angle $a A M$ is given, being the distance between the heliocentric place of the planet in the ecliptic from the earth's aphelion. Also $P T$ being parallel to $C B$, the angle $A T e$, and consequently the angle $A e T$, will in like manner be given, whence the points Γ , R , T , V being given, as in the solution above, the points d , c , e , and f will be given, the triangles $A R c$, $A T e$, being given in species, and similar respectively to the triangles $A \Gamma d$, and $A V f$. Also the rectangle under $W D Z$ being equal to that under $R \Gamma$, $V T$, if $D K$ be continued to the axis in g , and $D b$ be drawn parallel to $P R$, the rectangle under $f g$, $b d$ is equal to that under $f e$, $d c$, and both being deducted from the rectangle under $f b d$ the excess of the rectangle under $f b d$ above that under $f e$, $d c$ will be equal to that under $g b d$, so that this difference will be a mean proportional between the square of $b d$ and the square of $b g$, which is in a given ratio to the square of $b D$, and therefore in a given ratio to the rectangle under $a b b$, $D b$ being ordinately applied to the axis $a b$.

Thus a biquadratic equation may be formed, whereby the point b shall be found, and thence the point D , whose distance from A is to $b e$ as the excentricity of the earth's orbit to half its axis.

Therefore I shall only observe farther, that here occurs an obvious question, what, in so extended a search for principles leading to the solution of any problem, as the ancient analysis admits of, can conduct to the most genuine upon each several occasion.

But

But for this end, where commodious principles do not readily offer themselves, the most general means is to consider first simple cases of the problem in question, and from thence to proceed gradually to the more complex, as has been here done in the present problem, where the several preceding cases lead one after another to the points and lines required for the last case, wherein the problem is stated in its most extensive form.

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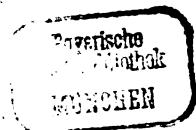
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E R R A T A.

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2. line 21, &c. for *Victoriam* in aversa parte *Gradientem* ferentes, denarios tamen, in perpetuam rei memoriam, jussit.—*insert—Denarios tamen, Victoriam in aversa parte Cauentem præ se ferentes, in perpetuam rei memoriam, signari* jussit.

V O L. LIII.

Pag. 136 line 21: *dele have.*

- 375 2 from the bottom, *dele every.*
- 397 first line in the note, for $r^{\theta-1}$ read $r^{\theta-2}$.
- 401 erase the asterisk in the 4th line from the top,
and place it in the 10th line.
- 405 6 and 10; *for on* read *in*.
- 415 4 from the bottom, for 1-2 E read 1+2 E
- 416 1 *for E* read Σ : and in the second line draw a
stroke over $mz - m^3 z^3$, &c.
- 418 3d and 4th lines in the note, *for comes almost as*
near read comes, in most cases, almost as near.
- 446 31 *for suspect r. suspect*.
- 448 22 *for fig. 2. r. fig. 1.*
- 446 17 *for fig. 1. r. fig. 3.*
- 30 *for fig. 2. r. fig. 4.*
- 451 29 *for fig. 3. r. fig. 5.*
- 453 12 *for equally r. unequally*
- 465 9 *for TAB. XXIII. r. TAB. XXII.*

Pe

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