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THE PHILOSOPHICAL TRANSACTIONS

(From the Year 1743, *to the Year* 1750)

ABRIDGED,

AND

Disposed under GENERAL HEADS.

The *Latin* PAPERS being translated into *English*.

By *JOHN MARTYN*, F. R. S.
Professor of BOTANY in the University of *Cambridge*.

VOLUME THE TENTH.

CONTAINING,

LONDON:
Printed for LOCKYER DAVIS and CHARLES REYMERS, against *Gray's-Inn-Gate*, *Holborn*, Printers to the ROYAL-SOCIETY.
MDCCLVI.

THE

PHILOSOPHICAL

TRANSACTIONS

(From the Year 1743, *to the Year* 1750)

ABRIDGED.

VOLUME THE TENTH.

CONTAINING,

PART I. The MATHEMATICAL PAPERS.

PART II. The PHYSIOLOGICAL PAPERS.

LONDON, 1756.

TO THE

PRESIDENT,

COUNCIL, and FELLOWS

OF THE

ROYAL SOCIETY

O F

LONDON,

For the improving of

NATURAL KNOWLEDGE,

This Abridgment of the PHILOSOPHICAL TRANSACTIONS is most humbly dedicated by

Chelsey, Jan. 1.
1756.

John Martyn.

A TABLE, ſhewing for what Months each Tranſaction was publiſhed.

VOL.	No.		
XLIII.	472.	*January*, *February*, *March*, and *April*,	1744.
	473.	*May*, and Part of *June*,	
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At the beginning of Vol. XLIII. *the following Advertisement is inserted.*

Where-ever it is said, at the head of any paper, *here printed with Additions*, or with *Alterations*; It is to be understood, that the author of such paper made such *additions* or *alterations* himself; for none of them have been made by the editor. And where it is said, *presented* on such a day; it implies that the paper was not read; the contents of it being of such a nature as not to be understood at a bare reading; and that therefore the subject in general was only mentioned, or the title read.

This Advertisement is also published at the beginning of Vol. XLVI.

This XLVI. Volume of the *Philosophical Transactions* concludes those published by the late *Cromwell Mortimer*, M. D. *Secretary* of the *Royal-Society*; the last number being printed off just before his death on the 7th of *January* 1752.

The following ERRATA *were corrected in the* Transactions at large, *too late to be rectified in the Abridgment.*

Vol. VIII. *Page* 199. *in the margin, for* Oct. 31. 1738. *read Oct.* 31. 1736.

This article therefore instead of being numbered XXVII. *should be* XXV. 4.

Vol. IX. *Page* 461, *line* 29. *read,* mentions not only a *Pen of Iron*, but also *the Point of a Diamond.*

The READER *is desired also to correct the following* ERRORS *in the present Volume.*

Page 301, *in the margin, for* Fig. 18. *read* Fig. 12.

Page 473, *in the table,* May 13. *for* 33, 23. (the height of the Barometer) *read* 30, 23. *and* July 24, *for* 22, 66. *read* 29, 66.

Page 481, *in the margin, for* July 15, *read July* 18.

Page 1034, *in the margin, for* Warren, *read* Warwick.

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THE

Philosophical Transactions

ABRIDGED.

PART I.

CONTAINING THE

Mathematical PAPERS.

CHAP. I.

ALGEBRA, ARITHMETICK, FLUXIONS, and *GEOMETRY.*

I. ALTHO' the Application of infinite Series, and the Quadrature of the conic Sections, to the inverse Method of Fluxions has exercised the Pens of the most able Mathematicians, and produced many curious and useful Discoveries; yet nothing has been hitherto given, that I know of, whereby the Fluents of radical Multinomials and Series, which do not begin to converge till after the second Term, can be determined, so as to be of Use in the Solution of Problems: the common Method, by expanding the given Expression, being altogether impracticable in this Case.

Of the Fluents of Multinomials, and Series affected by radical Signs, which do not begin to converge till after the second Term; in a Letter from T. Simpson *F. R. S. to* W. Jones *Esq; V. P. R. S.*

This little Essay is not merely an abstracted useless Speculation, but may be apply'd to good purpose in many difficult and important Inquiries into

Presented May 26, 1748. No 487. p. 328. *April.* &c. 1748.

into Nature; whereof I have put down one or two Instances, and shall further observe here, that most of the lunar Equations, given by Sir *I. Newton*, are only such Approximations as may be exhibited by the first Term of a Series derived by the Method here delivered.

Proposition. *The Fluent of* $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$ *being given (either in algebraic Terms, or from the Quadrature of the Conic Sections, &c.) it is proposed, by means thereof, to approximate the Fluent of* $\overline{a+cx^n+dx^{2n}+ex^{3n}+fx^{4n}\ \&c.}|^m \times x^{pn-1}\dot{x}$; *supposing the Series not to converge till after the second Term.*

Make $cz^n = cx^n + dx^{2n} + ex^{3n}$ &c. and let Q be the given Fluent of $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$, answering to any proposed Value of x: Moreover let $y = x^{pn}$, or $y^{\frac{1}{p}} = x^n$, and let this Value of x^n be substituted in the first Equation, and it will become $cz^n = cy^{\frac{1}{p}} + dy^{\frac{2}{p}} + ey^{\frac{3}{p}}$ &c. whereof the Root y being extracted, we shall (by making $R = -\frac{pd}{c}$, $S = \frac{pp+3}{2} \times \frac{d^2}{c^2} - \frac{pe}{c}$, $T = \frac{-pp+4p+5}{6} + \frac{d^3}{c^3} + \frac{pp+4}{1} \times \frac{de}{c^2} - \frac{pf}{c}$ &c.) have $y\ (x^{pn}) = z^{pn} + Rz^{pn+n} + Sz^{pn+2n}$ &c. whence we also obtain $x^{pn-1}\dot{x} = z^{pn-1}\dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1}\dot{z}$ &c.

Let this Value, with that of $cx^n + dx^{2n} + ex^{3n}$ &c. (above given) be now substituted in the proposed Fluxion, and it will become $\overline{a+cz^n}|^m \times \overline{z^{pn-1}\dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1}\dot{z}\ \&c.}$

Moreover, let v denote the Place, or Distance, of any Term, of this Expression, from the first (exclusive) then the Term itself (drawn into the common Multiplicator) will be denoted by $\overline{a+cz^n}|^m \times \frac{p+v}{p} \times Az^{pn+vn-1}\dot{z}$; and the Fluent thereof will be truly expressed by $\frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \cdots \frac{p+v}{p+m+v} \times \overline{\frac{a}{-c}}|^v$

$\times A$

$\times AQ + \frac{\overline{p+vA}}{p} \times \frac{\overline{a+cz^n}|^{m+1} \times z^{pn-n}}{p+m+v \times nc}$ into z^{vn} —

$\frac{p+v-1}{p+m+v-1} \times \frac{az^{vn-n}}{c} + \frac{p+v-1 . p+v-2}{p+v+m-1 . p+v+m-2}$

$\times \frac{a^2 z^{vn-2n}}{c^2}$ *&c.* continued to as many Terms as there are Units in v. Wherein let v be expounded by 1, 2, 3 *&c.* successively, and R, S, T, *&c.* by A respectively: By which means the Fluent of the whole Expression will be obtained.

Because the Fluent of the general Term, when the Multiplicator $\overline{a+cz}|^{m+1}$ becomes $= 0$, is barely $= \frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \ldots . \frac{p+v}{p+m+v} \times \frac{\overline{a}}{-c}\Big|^{v} \times AQ$ the Fluent of the whole Expression will, therefore, in this Case be truly defined by *Corol.* 1.

$Q \times \overline{1 - \frac{p+1 . Ra}{p+m+1 . C} + \frac{p+1 . p+2 . S2a}{p+m+1 . p+m+2 . C^2} - \frac{p+1 . p+2 . p+3 . Ta^3}{p+m+1 . p+m+2 . p+m+3 . c^3}}$ *&c.* Where Q denotes the Fluent of $\overline{a+cz^n}|^m \times z^{pn-1}\dot{z}$, when $z^n = \frac{a}{-c}$.

But, if $m+1$ and p be, each of them, the Half of an odd affirmative Number, and P be taken to denote the Periphery of a Circle whose Diameter is Unity, and $-c$ be put $= b$, then the Value of Q (or the Fluent of $\overline{a-bz^n}|^m \times z^{pn-1}\dot{z}$, when $z^n = \frac{a}{-b}$) will be *Corol.* 2.

$= \frac{a^{p+m}P}{nb^p} \times \frac{1. 3. 5. 7 \;\&c. \text{ (to } p - \frac{1}{2} \text{ Factors)} \times 1. 3. 5. 7 \;\&c. \text{ to } (m + \frac{1}{2} \text{ Factors)}}{2. 4. 6. 8. 10. 12 \;\&c. \text{ (to } p+m \text{ Factors)}}$

Therefore the *Whole*, required, Fluent, of $\overline{a - bx^n + dx^{2n} + ex^{3n} \;\&c.}|^m \times x^{pn-1}\dot{x}$ is, in this Case, equal to the Product of that Expression into the following Series, $1 + \frac{p+1 . Ra}{p+m+1 . b} + \frac{p+1 . p+2 . Sa^2}{p+m+1 . p+m+2 . b^2}$ *&c.* Wherein R is to be taken $= \frac{pd}{b}$, $S = \frac{p . p+3}{2} \times \frac{d^2}{b^2} + \frac{pe}{b}$, $T = \frac{p . p+4 . p+5}{6} \times \frac{d^3}{b^3} + \frac{p . p+4}{1} \times \frac{de}{b^2} + \frac{pf}{b}$, *&c.* according to what is above specified.

The

The Uſe of what has been deliver'd above will, in ſome meaſure, appear from the Solution of the two following Problems, which I ſhall ſubjoin as Examples thereof. The firſt is;

To find the Time of Oſcillation in the Arch of a Cycloid, in a Medium reſiſting according to the duplicate ratio of the Velocity.

Let A denote the whole Arch of the Semi-Cycloid, or the Length of the Pendulum, a the Arch deſcribed in the whole Deſcent, and x any variable Part thereof deſcribed from the Beginning of the Deſcent; and let the Denſity of the Medium be, every-where, as $\frac{1}{b}$: Then the Fluxion of the Time will be found =

$$\overline{a - 1 + \frac{2a}{b} \times \frac{x}{2} - \frac{2x^2}{2.3b} + \frac{4x^3}{2.3.4b^2} - \frac{8x^4}{2.3.4.5b^3}\ \&c.}\Big|^{-\frac{1}{2}}$$

$\times\ \overline{2A}\big|^{\frac{1}{2}} \times x^{\frac{1}{2}}\dot{x}$ *: which being compared with $\overline{a - bx^n + dx^{2n} + ex^{3n}\ \&c.}\big|$
vide Corol. 2. $^m \times x^{pn-1}\dot{x}$ we ſhall, in this Caſe, have $n = 1$, $m = -\frac{1}{2}$, $p = \frac{1}{2}$, $a = a$, $b = \overline{1 \times \frac{2a}{b}} \times \frac{1}{2}$, $\frac{d}{b} = \frac{2}{3b}$, $\frac{e}{b} = -\frac{1}{3b^2}$, $\frac{f}{b} = \frac{2}{15b^3}$ *&c.* Whence $R = \frac{1}{3b}$, $S = \frac{2}{9b^2}$, $T = \frac{8}{45b^3}$ *&c.* Alſo $\frac{a^{p+m}P}{nb^p} \times \frac{1.3.5.7(p-\frac{1}{2})}{2.4.6.8.10.(p+m)}$ $\times\ 1.3.5.7\ (m+\frac{1}{2})$ $= \frac{P}{b^{\frac{1}{2}}}$, and $1 + \frac{p+1.\ Ra}{p+m+1.\ b} + \frac{p+1.\ p+2.\ Sa^2}{p+m+1.\ p+m+2.\ b^2}\ \&c. = 1 + \frac{a}{2bb} + \frac{5a^2}{12b^2h^2} + \frac{7a^3}{18b^3h^3}\ \&c.$ Whence we have $\overline{2A}|^{\frac{1}{2}} \times \frac{P}{b^{\frac{1}{2}}} \times \overline{1 + \frac{a}{2bh} + \frac{5a^2}{nb^2h^2}}$, *&c.* for the Time of one Vibration of the Pendulum; which, by ſubſtituting $\overline{1 + \frac{2a}{b}}\big|^{\frac{1}{2}}$ for its Equal b, *&c.* becomes $PA^{\frac{1}{2}} \times \overline{1 * + \frac{a^2}{6b^2} - \frac{2a^3}{9b^3}}$ *&c.* From which it appears, that the Effect of the Reſiſtance on the Time of Vibration, in ſmall Arches, is nearly in the duplicate Ratio of thoſe Arches.

Princip. Prop. 27. B. 2. Sir *I. Newton* has, indeed, given a very different Solution to this Problem. But as the Concluſion here brought out exactly agrees with what I have elſewhere given, by a different Method, I have great Reaſon to believe I have no where fallen into an Error.

The

* The Inveſtigation of this, and the Fluxion in the following Example, are both given in my Eſſays.

The ſecond Example I ſhall give as an Illuſtration of the foregoing Method is,

To determine the Apſide Angle (or the Angle of the two Apſes at the Center) in an Orbit deſcribed by means of a centripetal Force, which varies according to any Power of the Diſtance.

In order to which, let the Velocity of the Body at the higher Apſe be to that whereby it might deſcribe a Circle at the ſame Diſtance from the Center, in the given Ratio of p to Unity; alſo let that Diſtance be denoted by Unity; and, ſuppoſing z to denote any other Diſtance, let the centripetal Force be univerſally expreſſed by z^n. Then the Fluxion of the Angle at the Center will be expreſſed by $\frac{-p\dot{z}}{z\sqrt{\overline{p^2+\frac{2}{n+1}}\times z^2-p^2-\frac{2z^{n+3}}{n+1}}}$. Put $a=1-p^2$, $v=\frac{n+3}{2}$ and $x=1-z^2$, and it will become $\frac{\frac{2}{1}\sqrt{1-a}\times\dot{x}}{\overline{1-x}\times\sqrt{ax+\frac{1-vx-\overline{1-x}|^{v}}{1-v}}}=\frac{2}{1}\,\overline{1-a}|^{\frac{1}{2}}$ into $\overline{a-\frac{vx}{2}+\frac{v.\overline{v-2}}{2.\ 3}\times x^2-\frac{v.\overline{v-2}.\overline{v-3}}{2.\ 3.\ 4}\times x^3\ \&c.}\Big|^{-\frac{1}{2}}\times\overline{x^{-\frac{1}{2}}\dot{x}+x^{\frac{1}{2}}\dot{x}+x^{\frac{3}{2}}\dot{x}\ \&c.}$

Now, to find the Fluent of the firſt Term hereof (drawn into the general Multiplicator) or $\overline{a-\frac{vx}{2}+\frac{v.\overline{v-2}}{2.\ 3}\times x^2\ \&c.}\Big|^{-\frac{1}{2}}\times x^{-\frac{1}{2}}\dot{x}$, we have (as before) $n=1$, $m=-\frac{1}{2}$, $p=\frac{1}{2}$, $b=\frac{v}{2}$, $\frac{d}{b}=\frac{v-2}{3}$, $\frac{e}{b}=-\frac{\overline{v-2}.\ \overline{v-3}}{3.\ 4}$, &c. Alſo $R=\frac{v-2}{6}$, $S=\frac{\overline{v-2}.\overline{4v-5}}{72}$; and conſequently the Fluent itſelf (when the Body arrives at the lower Apſe) $=\frac{P}{\sqrt{\frac{1}{2}v}}\times\overline{1+\frac{v-2}{2v}\times a+\frac{5.\ \overline{v-2}.\ \overline{4v-5}}{48v^2}\times a^2+\frac{7.\ \overline{v-2}.\ \overline{16v^2-37v+22}}{6.\ 48v^3}}$ &c. After the ſame manner the Fluent of the ſecond Term will come out $=\frac{P}{\sqrt{\frac{1}{2}v}}\times\overline{\frac{a}{v}+\frac{5.\ \overline{v-2}}{4v^2}\times a^2}$

+

$\overline{+ \frac{35 \cdot \overline{v-2} \cdot \overline{2v-3}}{48 v^3} \times a^3}$ &c. that of the third $= \frac{P}{\sqrt{\frac{1}{2}v}} \times \overline{\frac{3a^2}{2v^2} +}$ $\overline{\frac{35 \cdot \overline{v-2}}{12 v^3}} \times a^3$ &c. &c. &c. Whence, by collecting these several Fluents together, we have $\frac{P}{\sqrt{\frac{1}{2}v}} \times \overline{1 + \frac{1}{2}a + \frac{20v^2 - 5v + 2}{48 v^2}}$ $\overline{\times a^2 + \frac{112 v^3 - 63 v^2 + 42 v - 8}{6 \cdot 48 v^3} \times a^3}$ &c. for the Fluent of the whole Expression: And this, drawn into $\frac{1}{2} \times \overline{-1 - \frac{a}{2} - \frac{a^2}{8}}$ &c. $(= \frac{1}{2} \times \overline{1-a}|^{\frac{1}{2}})$ will be $= \frac{P}{\sqrt{2v}} \times \overline{1 * \frac{\overline{v-2} \cdot \overline{2v-1}}{48}}$ $\overline{\times \frac{a^2}{v^2} + \frac{\overline{\overline{v-2} \cdot \overline{2v-1}}|^2}{72} \times \frac{a^3}{v^3}}$ &c. $= \frac{P}{\sqrt{n+3}} \times \overline{1 * \frac{\overline{n-1} \cdot \overline{n+2}}{24}}$ $\overline{\times \frac{a^2}{\overline{n+3}|^2} + \frac{\overline{\overline{n-1} \cdot \overline{n+2}}|^2}{18} \times \frac{a^3}{\overline{n+3}|^3}}$ &c. which, in Degrees, gives $\frac{180}{\sqrt{n+3}} \times \overline{1 * + \frac{\overline{n-1} \cdot \overline{n+2}}{24} \times \frac{a^2}{\overline{n+3}|^2} + \frac{\overline{\overline{n-1} \cdot \overline{n+2}}|^2}{18}}$ $\overline{\times \frac{a^3}{\overline{n+3}|^3}}$ &c. for the true Measure of the Angle required.

A letter from Mr Abr. De Moivre, *F. R. S. to* William Jones, *Esq;* *F. R. S. concerning the easiest method for calculating the value of annuities upon lives, from tables of observations. Presented* June 7, 1744. N°. 473. p. 65. *May.* &c. 1744.

II. Altho' it has been an established custom, in the payment of annuities on lives, that the last rent is lost to the heirs of the late possessor of an annuity, if the person happens to die before the expiration of the term agreed on for payment, whether yearly, half-yearly, or quarterly: nevertheless, in this treatise I have suppos'd, that such a part of the rent should be paid to the heirs of the late possessor, as may be exactly proportion'd to the time elaps'd between that of the last payment, and the very moment of the life's expiring; and this by a proper, accurate, and geometrical calculation.

I have been induced to take this method, for the following reasons; first, by this supposition, the value of lives would receive but an inconsiderable increase; secondly, by this means, the several intervals of life, which, in the tables of observations, are found to have uniform decrements, may be the better connected together. It is with this view that I have framed the two following problems, with their solutions.

To

To find the value of an annuity, so circumstantiated, that it shall be on a life of a given age; and that, upon the failing of that life, such a part of the rent shall be paid to the heirs of the late possessor of an annuity, as may be exactly proportioned to the time intercepted between that of the last payment, and the very moment of the life's failing. Prob. I.

Let n represent the complement of life, that is, the interval of time between the given age, and the extremity of old-age, suppos'd at 86. Solution.

r the amount of 1 *l.* for one year.

α the logarithm of r.

P the present value of an annuity of 1 *l.* for the given time.

Q the value of the life sought.

Then $\frac{1}{r-1} - \frac{P}{\alpha n} = Q$

For, let z represent any indeterminate portion of n. Now the probability of the life's attaining the end of the interval z, and then failing, is to be expressed by $\frac{\dot{z}}{n}$, (as shewn in page 77, edit. 1. and in page 115, edit. 2. of my book of annuities upon lives) upon the supposition of a perpetual and uniform decrement of life. Demonstration.

But it is well known, that if an annuity certain, of 1 *l.* be paid during the time z, its present value will be $P = \frac{1 - r^{-z}}{r-1}$ or $\frac{1}{r-1} - \frac{1}{\overline{r-1} \times r^z}$.

And, by the laws of the doctrine of chances, the expectation of such a life, upon the precise interval z, will be expressed by $\frac{\dot{z}}{n \times \overline{r-1}} - \frac{\dot{z}}{nr^z \times \overline{r-1}}$; which may be taken for the ordinate of a curve, whose area is as the value of the life required.

In order to find the area of this curve, let $p = n \times \overline{r-1}$; and then the ordinate will become $\frac{\dot{z}}{p} - \frac{\dot{z}}{pr^z}$, a much more commodious expression.

Now it is plain, that the fluent of the first part is $\frac{z}{p}$: but as the fluent of the second part is not so readily discover'd, it will not be improper, in this place, to shew by what artifice I found it; for I do not know, whether the same method has been made use of by others: all

all that I can ſay, is, that I never had occaſion for it, but in the particular circumſtance of this problem.

Let, therefore, $r^z = x$; hence z log. $r =$ log. x; therefore $\dot{z}$ log. r $=$ (fluxion of the log. $x =$) $\frac{\dot{x}}{x}$, or $\alpha \dot{z} = \frac{\dot{x}}{x}$; conſequently $\dot{z} = \frac{\dot{x}}{\alpha x}$, and $\frac{\dot{z}}{r^z} = \frac{\dot{x}}{\alpha xx}$: but the fluent of $\frac{\dot{x}}{\alpha xx}$ is $(- \frac{1}{\alpha x} =) - \frac{1}{\alpha r^z}$; and therefore the fluent of $- \frac{\dot{z}}{p r^z}$ will be $+ \frac{1}{p \alpha r^z}$.

The ſum of the two fluents will be $\frac{z}{p} + \frac{1}{p \alpha r^z}$; but, when $z = 0$, the whole fluent ſhould be $= 0$, let therefore the whole fluent be $\frac{z}{p} + \frac{1}{p \alpha r^z} + q = 0$.

Now, when $z = 0$, then $\frac{z}{p} = 0$, and $\frac{1}{\alpha p r^z}$ becomes $\frac{1}{\alpha p}$ (for $r^z = 1$,) conſequently $\frac{1}{\alpha p} + q = 0$; and $q = - \frac{1}{\alpha p}$: therefore the area of a curve, whoſe ordinate is $\frac{\dot{z}}{p} - \frac{\dot{z}}{p r^z}$ will be $\left(\frac{z}{p} - \frac{1}{\alpha p} + \frac{1}{\alpha p r^z} = \right)$ $\frac{z}{p} - \overline{1 - \frac{1}{r^z}} \times \frac{1}{\alpha p}$.

But $P = \frac{1}{r - 1} - \frac{1}{\overline{r-1} \times r^z}$; therefore $1 - \frac{1}{r^z} = \overline{r - 1} \times P$, and the expreſſion for the area becomes $\frac{z}{n \times \overline{r - 1}} - \frac{P}{\alpha n}$: And putting n inſtead of z, that area, or the value of the life, will be expreſſed by $\frac{1}{r - 1} - \frac{P}{\alpha n}$. *Q. E. D.*

Thoſe who are well verſed in the nature of logarithms, I mean thoſe that can deduce them from the doctrine of fluxions and infinite ſeries, will eaſily apprehend, that the quantity here called α, is that which ſome call the hyperbolic logarithm; others, the natural logarithm: it is what Mr *Cotes* calls, the logarithm whoſe modulus is 1: laſtly, it is by ſome called *Neper*'s logarithm. And, to ſave the reader ſome trouble in the practice of this laſt theorem, the moſt neceſſary natural logarithms, to be made uſe of in the preſent diſquiſition about lives, are the following:

If

If $r = 1.04$, then will $\alpha = 0.0392207$.
$r = 1.05$, - - - $\alpha = 0.0487901$.
$r = 1.06$, - - - $\alpha = 0.0582589$.

It is to be obſerved, that the theorem here found, makes the values of lives a little bigger, than what the theorem found in the firſt problem of my book of annuities on lives, does; for, in the preſent caſe, there is one payment more to be made, than in the other; however, the difference is very inconſiderable.

But, altho' it be indifferent which of them is uſed, on the ſuppoſition of an equal decrement of life to the extremity of old-age; yet, if it ever happens, that we ſhould have tables of obſervations, concerning the mortality of mankind, intirely to be depended upon, then it would be convenient to divide the whole interval of life into ſuch ſmaller intervals, as, during which, the decrements of life have been obſerved to be uniform, notwithſtanding the decrements in ſome of thoſe intervals ſhould be quicker, or ſlower, than others; for then the theorem here found would be preferable to the other; as will be ſhewn hereafter.

That there are ſuch intervals, Dr *Halley*'s tables of obſervations ſufficiently ſhew; for inſtance; out of 302 perſons of 54 years of age, there remain, after 16 years (that is, of the age of 70) but 142; the decrements from year to year having been conſtantly 10; and the ſame thing happens in other intervals; and it is to be preſumed, that the like would happen in any other good tables of obſervations.

But, in order to ſhew, in ſome meaſure, the uſe of the preceding theorem, it is neceſſary to add another problem; which, tho' its ſolution is to be met with in the firſt edition of my book of annuities on lives, yet it is convenient to have it inſerted here, on account of the connexion that the application of the preceding problem has with it.

In the mean time, it will be proper to know, *What part of the yearly rent ſhould be paid to the heirs of the late poſſeſſor of an annuity, as may be exactly proportioned to the time elapſed between that of the laſt payment, and the very moment of the life's expiring.* To determine this, put A for the yearly rent; $\frac{1}{m}$ for the part of the year intercepted between the time of the laſt payment, and the inſtant of the life's failing; r the amount of 1 *l.* at the year's end: then will $\frac{r^{\frac{1}{m}} - 1}{n - 1} A$, be the ſum to be paid.

To find the value of an annuity for a limited interval of life, during which the decrements of life may be conſidered as equal. Prob. II.

Let a and b repreſent the number of people living in the beginning and end of the given interval of years. Solution.

 s repreſent

s represent that interval.

P the value of an annuity certain for that interval.

Q the value of an annuity for a life supposed to be necessarily extinct in the time s; or (which is the same thing) the value of an annuity for a life, of which the complement is s.

Then $Q + \frac{b}{a} \times \overline{P - Q}$ will express the value required.

Demonstration. For, let the whole interval between a and b be fill'd up with arithmetical mean proportionals; therefore the number of people living in the beginning and end of each year of the given interval s will be represented by the following series; *viz.*

$$a.\ \frac{sa - a + b}{s}.\ \frac{sa - 2a + 2b}{s}.\ \frac{sa - 3a + 3b}{s}.\ \frac{sa - 4a + 4b}{s}.$$

&c. to b.

Consequently, the probabilities of the life's continuing during 1, 2, 3, 4, 5, &c. years will be expressed by the series,

$$\frac{sa - a + b}{sa}.\ \frac{sa - 2a + 2b}{sa}.\ \frac{sa - 3a + 3b}{sa}.\ \frac{sa - 4a + 4b}{sa}.$$

&c. to $\frac{b}{a}$. Wherefore, the value of an annuity of 1 *l.* granted for the time s, will be expressed by the series

$$\frac{sa - a + b}{sar} + \frac{sa - 2a + 2b}{sar^2} + \frac{sa - 3a + 3b}{sar^3} + \frac{sa - 4a + 4b}{sar^4}.$$

&c. to $+ \frac{b}{ar^s}$; this series is divisible into two other series's, *viz.*

$$1st.\ \frac{s-1}{sr} + \frac{s-2}{sr^2} + \frac{s-3}{sr^3} + \frac{s-4}{sr^4}, \text{ \&c. to } + \frac{s-s}{sr^s}.$$

$$2d.\ \frac{b}{a} \times \overline{\frac{1}{sr} + \frac{2}{sr^2} + \frac{3}{sr^3} + \frac{4}{sr^4}, \text{ \&c. to } \frac{s}{sr^s}}.$$

Now, since the first of these series's begins with a term whose numerator is $s - 1$, and the subsequent numerators each decrease by unity; it follows, that the last term will be $= 0$; and, consequently, that series expresses the value of a life necessarily to be extinct in the time s. The sum of this series may be esteem'd as a given quantity; and is what I have expressed by the symbol Q in problem 1.

The second series is the difference between the two following series's,

$$\frac{b}{a} \times \overline{\frac{1}{r} + \frac{1}{r^2} + \frac{1}{r^3} + \frac{1}{r^4} + \text{ \&c. to } \frac{1}{r^s}}.$$

$\frac{b}{a}$

$$\frac{b}{a} \times \overline{\frac{s-1}{sr} + \frac{s-2}{sr^2} + \frac{s-3}{sr^3} + \frac{s-4}{sr^4} \text{ \&c. to } + \frac{s-s}{sr^s}}.$$

Where, neglecting the commom multiplier $\frac{b}{a}$, the first series is the value of an annuity certain to continue s years; which every mathematician knows how to calculate, or is had from tables already composed for that purpose: this value is what I have called P; and the second series is Q.

Therefore $Q + \frac{b}{a} \times \overline{P - Q}$ will be the value of an annuity on a life for the limited time. *Q. E. D.*

It is obvious, that the series denoted by Q, must of necessity have one term less than is the number of equal intervals contain'd in s; and therefore, if the whole extent of life, beginning from an age given, be divided into several intervals, each having it's own particular uniform decrements, there will be, in each of these intervals, the defect of one payment; which to remedy, the series Q must be calculated by problem 1.

To find the value of an annuity for an age of 54, *to continue* 16 *years and no longer.* Example.

It is found, in Dr *Halley*'s tables of observations, that a is 302, and b 172: now $n = s = 16$; and, by the tables of the values of annuities certain, $P = 10.8377$; also (by problem 1.) $Q = \left(\frac{1}{r-1} - \frac{P}{an} =\right)$ 6.1168. Hence it follows (by this problem), that the value of an annuity for an age of 54, to continue during the limited time of 16 years, supposing interest at 5 *per cent. per annum*, will be worth $(Q + \frac{b}{a} \times \overline{P - Q} =)$ 8.3365 years purchase.

From Dr *Halley*'s tables of observations, we find, that from the age of 49 to 54 inclusive, the number of persons, existing at those several ages, are, 357, 346, 335, 324, 313, 302, which comprehends a space of five years; and, following the precepts before laid down, we shall find, that an annuity for a life of 49, to continue for the limited time of 5 years, interest being at 5 *per cent. per annum*, is worth 4.0374 years purchase.

And, in the same manner, we shall find, that the value of an annuity on life, for the limited time comprehended between the ages of 42 and 49, is worth 5.3492 years purchase.

Now, if it were required to determine the value of an annuity on life, to continue from the age of 42 to 70, we must proceed thus:

It has been proved, that an annuity on life, reaching from the age of 54 to 70, is worth 8.3365 years purchase; but this value, being estimated from the age of 49, ought to be diminished on two accounts:

 First

becauſe of the probability of the life's reaching from 49 to 54, which probability is to be deduced from the table of obſervations, and is proportional to the number of people living at the end and beginning of that interval, which, in this caſe, will be found 302 and 357: The ſecond diminution proceeds from a diſcount that ought to be made, becauſe the annuity, which reaches from 54 to 70, is eſtimated 5 years ſooner, *viz.* from the age of 49, and therefore that diminution ought to be expreſſed by $\frac{1}{r^5}$; ſo that the total diminution of the annuity of 16 years will be expreſſed by the fraction $\frac{302}{357\,r^5}$, which will reduce it from 8.3365 years purchaſe to 5.5259; this being added to the value of the annuity to continue from 49 to 54, *viz.* 4.0374, will give 9.5633, the value of an annuity to continue from the age of 49 to 70. For the ſame reaſon, the value 9.5633, eſtimated from the age of 42, ought to be reduced, both upon account of the probability of living from 42 to 49, and of the diſcount of money for 7 years, at 5 *per cent. per annum*, amounting together to 3.8554, which will bring it down to 5.7079; to this adding the value of an annuity on a life to continue from the age of 42 to 49, found before to be 5.3492, the ſum will be 11.0571 years purchaſe, the value of an annuity to continue from the age of 42 to 70.

In the ſame manner, for the laſt 16 years of life, reaching from 70 to 86, when properly diſcounted, and alſo diminiſhed upon the account of the probability of living from 42 to 70, the value of thoſe laſt 16 years will be reduced to 0.8; this being added to 11.0571 (the value of an annuity to continue from the age of 42 to 70, found before), the ſum will be 11.8571 years purchaſe, the value of an annuity to continue from the age of 42 to 86; that is, the value of an annuity on a life of 42; which, in my tables, is but 11.57, upon the ſuppoſition of an uniform decrement of life, from an age given to the extremity of old-age, ſuppoſed at 86.

It is to be obſerved, that the two diminutions, above-mention'd, are conformable to what I have ſaid in the corollary to the ſecond problem of the firſt edition, printed in the year 1724.

Thoſe who have ſufficient leiſure and ſkill to calculate the value of joint lives, whether taken two and two, or three and three, in the ſame manner as I have done the firſt problem of this tract, will be greatly aſſiſted by means of the two following theorems:

If the ordinate of a curve be $\frac{z}{r^z}$; it's area will be $\frac{1}{a^2} - \frac{1}{a^2\,r^z} - \frac{z}{ar^z}$.

If the ordinate of a curve be $\frac{z^2}{r^z}$; it's area will be $\frac{2}{a^3} - \frac{2}{a^3\,r^z} - \frac{2z}{a^2\,r^z} - \frac{z^2}{a\,r^z}$.

I beg

I beg leave, in this place, to take notice, that in the theorem in line 12, page 63. of the second edition of my book of annuities on lives, instead of P, it ought to be $\frac{p}{n} P$; where n and p represent the complements of the age, in the beginning and end of a given interval of time.

And I desire the reader of that edition to adapt the fourth article of the rule put in words at length, in page 64, to the theorem so corrected: then the solution there given, and that in page 21. of the first edition, will perfectly agree; provided that the decrements of life be supposed, in both cases, uniform, from an age given, to the extremity of old-age.

I must also take notice of an accidental error, that has crept into the 25th proposition of the second edition; which I chuse to correct as follows;

1. Let the first line of the proposition, and part of the second line, as far as *A* exclusive, be erased.

2. Let the solution proceed thus: since the life of *A* is supposed to be worth 14 years purchase, when interest is at 4 *per cent. per annum*, it follows, from our tables, that *A* must be 35 years of age; therefore find, by the twenty-third proposition, the value of an annuity of a life for 35 to continue for a limited time of 31 years: let that value be subducted from the value of an annuity certain, to continue 31 years; and the remainder will be the value of the reversion.

An account of a new invented arithmetical instrument called a Shwan-pan, *or* Chinese Accompt-Table; *by* Gamaliel Smethurst.

Read Jan. 29. 1748. N°. 491. p. 22. Jan. &c. 1749.

III. The *Chinese* have for many ages picqued themselves on being the most wise of any nation in the world; but late experience and closer converse with them hath found this pride to be ill-grounded. One particular, in which they think they excel all mankind, is, their manner of accompting, which they do with an instrument composed of a number of wires with beads upon them, which they move backwards and forwards. This instrument they call a *Shwan-pan*.

Now I trust I have formed one on the plan of our 9 digits, that in no case falls short of the *Chinese Shwan-pan*, but in many excels theirs.

The *Chinese*, according to the accounts of travellers, are so happy as to have their parts of an integer in their coins, &c. decimated, so can multiply or divide their integers and parts as if they were only integers. This gives them the advantage over *Europeans* in reckoning their money, &c. But then, as they have no particular place set apart for the lesser denominations of coins, weights, measures, &c. their instrument can't be used in *Europe*, nor can it be so universally applied to arithmetick as mine, for I have provided for the different divisions of an integer into parts.

This instrument hath the advantage of our digits in a great many Cases. First, the figures can be felt, so may be used by a blind man. If it had no other, this alone would be sufficient to gain it the attention of mankind.

Another

Another advantage from it is, that, when attain'd, this method is much ſwifter than by our digits, and leſs liable to miſtakes: it is likewiſe not ſo burdenſome to the memory in working the rules of arithmetick, as by our digits, we being oblig'd to carry the tens in the mind from one place to another, which are ſet down by the *Shwan-pan.* —— One may work a whole night, without confuſing the head, or affecting the eyes in the leaſt.

It may be of great uſe to teach people the power of numbers, likewiſe to examine accompts by; for, as the perſon will, by the *Shwan-pan*, work it quite a different way, it will ſerve as if another perſon had gone thro' the accompt; if it proves right with the written one, they may reſt aſſured the work is true.

It may be a very pretty lure to lead young people to apply their minds to numbers *.

THEOREM.

An extract of a letter from William Jones, *Eſq*; *F. R. S. to* Martin Folks, *Eſq*; *Preſident of the* Royal Society; *containing a commodious diſpoſition of equations for exhibiting the relations of Goniometrical Lines. Preſented* 4 July 1747. N°. 483. p. 560. March, &c. 1753.

IV. *In a circle whoſe radius is* r, *let there be two arcs,* A *the greater,* a *the leſs, each in the firſt quadrant*; *put* s, t, ſ, *and* v, *for the ſine, tangent, ſecant, and verſed ſine of an arc*; s\`, t\`, ſ\`, *the ſine, tangent, ſecant of the complement, and* v\`, *the verſed ſine of the ſupplement of that arc*; *let* $z = \frac{1}{2}\,\overline{A + a}$, $x = \frac{1}{2}\,\overline{A - a}$; *or if* z *and* x *be put for the arcs, it will be* $A = z + x$, $a = z - x$.

Then will the terms in any column of the following table, be proportional to their correſponding ones in any other column.

A TABLE of the Relations of Goniometrical Lines.

$2s,z$	$\frac{s,2z}{s,\overline{A+a}}$	$\frac{v,2z}{v,\overline{A+a}}$	$s,A+s,a$	$\frac{\grave{s},a-\grave{s},A}{v,A-v,a}$
$2s,x$	$s,A-s,a$	$\frac{s,a-\grave{s},A}{v,A-v,a}$	$\frac{s,2x}{s,\overline{A-a}}$	$\frac{v,2x}{v,\overline{A-a}}$
$2\grave{s},z$	$\frac{\grave{v},2z}{v,\overline{A+a}}$	$\frac{s,2z}{s,\overline{A+a}}$	$\grave{s},A+\grave{s},a$	$s,A-s,a$
$2\grave{s},x$	$\grave{s},A+\grave{s},a$	$s,A+s,a$	$\frac{\grave{v},2x}{\grave{v},\overline{A-a}}$	$\frac{s,2x}{s,\overline{A-a}}$
r	$\grave{s},z$	s,z	$\grave{s},x$	s,x
$ſ,z$	r	t,z		
t,z	s,z	$ſ,z-\grave{s},z$		
$\grave{ſ},z$	$\grave{t},z$	r		
$t,z+t,z$	$\grave{ſ},z$	$ſ,z$		
$ſ,x$			r	t,x
t,x			s,x	$ſ,x-s,x$

* The inventor produced one of theſe inſtruments before the *Society*, and work'd ſeveral queſtions in arithmetick upon it. It much reſembles the *abacus* of the ancients. *C. M.*

From

From hence, almost an infinite number of theorems may easily be derived; some of which are the following, given here as examples of the use of the table.

I. $s,z \times s,x = \frac{1}{2}r \times \overline{\grave{s},a - \grave{s},A} = \frac{1}{2}r \times \overline{\grave{s},\overline{z - x} - \grave{s},\overline{z + x}}$
$= \frac{s,z}{\grave{\int},x}\, rr = \frac{s,x}{\grave{\int},z}\, rr.$

$\grave{s},z \times \grave{s},x = \frac{1}{2}r \times \overline{\grave{s},a + \grave{s},A} = \frac{1}{2}r \times \overline{\grave{s},\overline{z - x} + \grave{s},\overline{z + x}}$
$= \frac{\grave{s},z}{\int,x}\, rr = \frac{\grave{s},x}{\int,z}\, rr.$

II. If A, B, C, be any three angles; $Z = A + B$, $X = A - B$, $H = \frac{1}{2}\overline{A + B + C}$.

Then $\frac{1}{2}r \times \overline{v,C - v,X} = s,\frac{1}{2}\overline{C + X} \times s,\frac{1}{2}\overline{C - X} = s,\frac{1}{2}\overline{A + C - B} \times s,\frac{1}{2}\overline{B + C - A} = s,\overline{H - B} \times s,\overline{H - A}$.

And $\frac{1}{2}r \times \overline{v,Z - v,C} = s,\frac{1}{2}\overline{Z + C} \times s,\frac{1}{2}\overline{Z - C} = s,\frac{1}{2}\overline{A + B + C} \times s,\frac{1}{2}\overline{A + B - C} = s,H \times s,\overline{H - C}$.

III. $\frac{ss,z}{\grave{s}\grave{s},z} = \frac{tt,z}{rr} = \frac{rr}{\grave{t}\grave{t},z} = \frac{v,2z}{\grave{v},2z} = \frac{t,z}{\grave{t},z}$; Or $\frac{ss,\frac{1}{2}z}{\grave{s}\grave{s},\frac{1}{2}z} = \frac{tt,\frac{1}{2}z}{rr}$
$= \frac{rr}{\grave{t}\grave{t},\frac{1}{2}z} = \frac{v,z}{\grave{v},z} = \frac{t,\frac{1}{2}z}{\grave{t},\frac{1}{2}z}.$

IV. $\frac{1}{2}r = \frac{ss,z}{v,2z} = \frac{ss,\frac{1}{2}z}{v,z} = \frac{\grave{s}\grave{s},z}{\grave{v},2z} = \frac{\grave{s}\grave{s},\frac{1}{2}z}{\grave{v},z}$; and $s,z = \frac{2ss,\frac{1}{2}z}{t,\frac{1}{2}z}$
$= \frac{2\grave{s}\grave{s},\frac{1}{2}z}{\grave{t},\frac{1}{2}z}.$

V. $\frac{s,2}{v,z} = \frac{r}{t,\frac{1}{2}z} = \frac{\grave{t},\frac{1}{2}z}{r} = \frac{\grave{v},z}{s,z}.$

VI. $\frac{t,z}{t,x} = \frac{s,A + s,a}{s,A - s,a} = \frac{\grave{t},x}{\grave{t},z}$; and $\frac{rr}{t,z \times t,x} = \frac{\grave{t},z}{t,x} = \frac{\grave{t},x}{t,z}$
$= \frac{\grave{s},a + \grave{s},A}{\grave{s},a - \grave{s},A} = \frac{\grave{t},z \times \grave{t},x}{rr}.$

VII. $\frac{s,A}{s,a} = \frac{t,z + t,x}{t,z - t,x} = \frac{s,\overline{z + x}}{s,\overline{z - x}}$; if z and x are two arcs, then $A = z + x, a = z - x$.

VIII.

$$\text{VIII. } s,\overline{z \pm x} = \frac{s,z \times s',x \pm s',z \times s,x}{r} = \frac{t,z \pm t,x}{\int,z \times \int,x}.$$

$$\text{IX. } s',\overline{z \pm x} = \frac{s',z \times s',x \mp s,z \times s,x}{r} = \frac{rr \mp t,z \times t,x}{\int,z \times \int,x}\, r.$$

$$\text{X. } t,\overline{z \pm x} = \frac{t,z \pm t,x}{rr \mp t,z \times t,x}\, rr; \text{ and } t',\overline{z \pm x} = \frac{rr \mp t,z \times t,x}{t,z \pm t,x}.$$

$$\text{XI. } \int,\overline{z \pm x} = \frac{\int,z \times \int,x}{rr \mp t,z \times t,x}\, r; \text{ and } \int',\overline{z \pm z} = \frac{\int,z \times \int,x}{t,z \pm t,x}.$$

XII. In three equidifferent arcs A, z, a; where $z\ (= \frac{1}{2}\overline{A + a})$ is the mean arc, and $x\ (= \frac{1}{2}\overline{A - a})$ their common difference; put $p = \frac{s',x}{r}$, $q = \frac{s,x}{r}$; $P = 2p \times s,z$, $\mathcal{Q} = 2q \times s',z$.

Then $s,A = P - s,a = \mathcal{Q} + s,a$; And $s,a = P - s,A = s,A - \mathcal{Q}$.

XIII. Let $d = v,A - v,a = s',a - s',A$; then $ss,A - ss,a = \overline{2s',A + d} \times d = \overline{2s',a - d} \times d$. *

XIV. Let A, B, C, &c. be the sines, a, b, c, &c. the co-sines, a', b', c', &c. the tangents, of the arcs, α, β, γ, &c. whose number is n; the radius being r; put S for the product of the n co-sines, S', S'', S''', &c. for the sum of the products made of every sine, every two, three, &c. sines, by the other ($n - 1$, $n - 2$, $n - 3$, &c.) co-sines, where the co-sine noted by $n - n$ is unity.

Then the sine of $\overline{\alpha + \beta + \gamma + \delta, \text{\&c.}} = \overline{S' - S''' + S^{v} - S^{vii}, \text{\&c.}} \times \frac{1}{r^{n-1}}$.

And the co-sine of $\overline{\alpha + \beta + \gamma + \delta, \text{\&c.}} = \overline{S - S'' + S^{iv} - S^{vi}, \text{\&c.}} \times \frac{1}{r^{n-1}}$.

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* *Note*, When an arc is terminated in the second, third, or fourth quadrant, some of the signs (+ and —) of the terms in the preceding theorems, will, by the known rules, become contrary to what they now are.

XV.

XV. Also putting T' for the sum of the tangents of the arcs, α, β, γ, &c. T'', T''', $T^{\prime v}$, &c. for the sum of the products of every two, three, four, &c. tangents; and

$$\begin{aligned} A &= T' \\ B &= AT'' - T''' \\ C &= BT'' - AT^{\prime v} + T^{v} \\ D &= CT'' - BT^{\prime v} + AT^{v\prime} - T^{v\prime\prime} \\ E &= DT'' - CT^{\prime v} + BT^{v\prime} - AT^{v\prime\prime\prime} + T^{\prime x}. \end{aligned}$$

Put $R = \frac{1}{rr}$.

Then the tangent of $\overline{\alpha + \beta + \gamma + \delta, \&c.} = A + BR + CR^2 + DR^3 + ER^4$; &c.

XVI. Hence, the sine, tangent, and secant, of any arc a, being represented by s, t, f, the co-sine, co-tangent, and co-secant, by $\grave{s}$, $\grave{t}$, $\grave{f}$; those of the arc na are expressed as in the following theorems.

Putting $n' = n. \frac{n-1}{2}$; $n'' = n'. \frac{n-2}{3}$; $n''' = n''. \frac{n-3}{4}$; $n^{\prime v} = n'''. \frac{n-4}{5}$; &c.

Sine of $na = \overline{nA - n'' AP + n^{\prime v} BP - n^{v\prime} CP + n^{v\prime\prime\prime} DP, \&c.} \times \frac{\grave{s}^{n-1}}{r^{n-1}}$; where $P = \frac{ss}{\grave{s}\grave{s}}$; $A = s$; $B = AP$; $C = BP$; $D = CP$; &c.

Or $= \overline{ns - \frac{n-1}{2} . \frac{n-2}{3} AP + \frac{n-3}{4} . \frac{n-4}{5} BP - \frac{n-5}{6} . \frac{n-6}{7} CP \&c.} \times \frac{\grave{s}^{n-1}}{r^{n-1}}$; where A, B, C, &c. stand for the respective preceding terms.

Or $= ns + \frac{1+n}{2} . \frac{1-n}{3} AQ + \frac{3+n}{4} . \frac{3-n}{5} BQ + \frac{5+n}{6} . \frac{5-n}{7} CQ + \frac{7+n}{8} . \frac{7-n}{9} DQ$. &c. where $Q = \frac{ss}{rr}$; A, B, C, &c. stand as before.

XVII. Co-sine of $na = \overline{1 - n' P + n''' P^2 - n^{v} P^3 + n^{v\prime\prime} P^4, \&c.} \times \frac{\grave{s}^{n}}{r^{n-1}}$, where $P = \frac{ss}{\grave{s}\grave{s}}$.

Or $= r + \frac{0+n}{1} . \frac{0-n}{2} AQ + \frac{2+n}{3} . \frac{2-n}{4} BQ + \frac{4+n}{5} . \frac{4-n}{6}$

$C\mathcal{Q} + \frac{6+n}{7} \cdot \frac{6-n}{8} D\mathcal{Q}$, &c. where $\mathcal{Q} = \frac{ss}{r}$; and A, B, C, &c. stand for the respective preceding terms.

Or put $M = \overline{\frac{2s}{r}}\Big|^{n} \times r$; $N = \frac{rr}{4ss}$; $A = \frac{1}{2}$; $B = AN$; $C = BN$; $D = CN$, &c; $p = n$; $p' = n-1$; $p'' = n-2$, &c. And $a' = p$; $b' = p \cdot \frac{1}{2} p'''$; $c' = p \cdot \frac{1}{2} p^{iv} \cdot \frac{1}{3} p^{v}$; $d' = p \cdot \frac{1}{2} p^{v} \cdot \frac{1}{3} p^{vi} \cdot \frac{1}{4} p^{vii}$; $e' = p \cdot \frac{1}{2} p^{vi} \cdot \frac{1}{3} p^{vii} \cdot \frac{1}{4} p^{viii} \cdot p \frac{1}{5} p^{ix}$; &c.

The co-sine of $na = \overline{A - Ba' + Cb' - Dc' + Ed', \text{ \&c.}} \times M$.

XVIII. Let $A = -n'' + nn'$

$B = +n^{iv} - nn''' + An'$

$C = -n^{vi} + nn^{v} + Bn' - An'''$

$D = +n^{viii} - nn^{vii} + Cn' - Bn''' + An^{v}$ &c.

$A' = \frac{1}{n} \cdot n'' - n'$

$B' = \frac{1}{n} \cdot \overline{n''A' - n^{iv}} + n'''$

$C' = \frac{1}{n} \cdot \overline{n''B' - n^{iv}A' + n^{vi}} - n^{v}$

$D' = \frac{1}{n} \cdot \overline{n''C' - n^{iv}B' + n^{vi}A' - n^{viii}} + n^{vii}$ &c.

The tangent of $na = nt + At^3 r^{-2} + Bt^5 r^{-4} + Ct^7 r^{-6} + Dt^9 r^{-8}$ &c.

Or $= \overline{n + AN + BN^2 + CN^3 + DN^4 + \text{ \&c.}} \times t$, where $N = \frac{tt}{rr}$.

Or $= na' + Ab' + Bc' + Cd' + De'$, &c. where $a' = t$; $b' = Na'$; $c' = Nb'$; $d' = Nc'$; &c.

Or $= \frac{n - n''N + n^{iv}N^2 - n^{vi}N^3 + n^{viii}N^4, \text{ \&c.}}{1 - n'N + n'''N^2 - n^{v}N^3 + n^{vii}N^4. \text{ \&c.}} \times t$.

Co-tangent of $na = \overline{r^2 + A't^2N + C't^2N^2 + D't^2N^3 + E't^2N^4 \text{ \&c.}} \times \frac{rr}{nt}$; where $N = \frac{tt}{rr}$.

Or $= \overline{1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5, \text{ \&c.}} \times \frac{1}{7} r^2 t'$; where $N = \frac{rr}{t't'}$.

Or

Or $= \dfrac{1 - n' N + n''' N^2 - n^{v} N^3 + n^{vii} N^4 - n^{ix} N^5, \text{\&c.}}{n - n'' N + n^{iv} N^2 - n^{vi} N^3 + n^{viii} N^4 - n^{x} N^5, \text{\&c.}} \times \dfrac{rr}{t}$; where $N = \dfrac{tt}{rr}$.

XIX. Let $A = n'$

$$B = An' - n'''$$

$$C = Bn' - An''' + n^{v}$$

$$D = Cn' - Bn''' + An^{v} - n^{vii} \text{ \&c.}$$

$$A' = \frac{1}{n} . n''$$

$$B' = \frac{1}{n} . \overline{n'' A' - n^{iv}}$$

$$C' = \frac{1}{n} . \overline{n'' B' - n^{iv} A + n^{vi}}$$

$$D' = \frac{1}{n} . \overline{n'' C' - n^{iv} B' + n^{vi} A' - n^{viii}} \text{ \&c.}$$

Secant of $na = \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \text{\&c.}} \times M$.

Or $= \dfrac{1}{1 - n' N + n''' N^2 - n^{v} N^3 + n^{vii} N^4, \text{\&c.}} \times M$; where $N = \dfrac{tt}{rr}$, $M = \dfrac{r \int n}{rn}$.

Co-fecant of $na = \overline{1 + A' N + B' N^2 + C' N^3 + D' N^4 + E' N^5, \text{\&c.}} \times M$; where $N = \dfrac{tt}{rr}$, $M = \dfrac{rr \int n}{ntrn}$.

Or $= \dfrac{1}{n - n' N + n^{iv} N^2 - n^{vi} N^3 + n^{viii} N, \text{\&c.}} \times M$; where $N = \dfrac{tt}{rr}$, $M = \dfrac{rn - 2}{t \int n - 2}$.

XX. Let c be the chord of an arc (a) of the circumference of a circle, whofe diameter is d. Put $N = \dfrac{cc}{dd}$.

The chord of $na = nc + \dfrac{1+n}{2} . \dfrac{1-n}{3} AN + \dfrac{3+n}{4} . \dfrac{3-n}{5} BN +$

 $5+n$

$\frac{5+n}{6} \cdot \frac{5-n}{7} CN + \frac{7+n}{8} \cdot \frac{7-n}{9} DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

As the preceding theorems are easily deduced from the first, so the following are most readily seen to be the immediate consequences of these; and all depending upon no other principles than what are generally made use of in common computations.

XXI. Putting s, s', t, t', f, f', for the sine, co-sine, tangent, co-tangent, secant, co-secant, of an arc (a), and v it's versed sine; let $q'=\frac{1}{2}$; $q''=\frac{1}{3}q'$; $q'''=\frac{1}{4}q''$; $q^{v}=\frac{1}{5}q'''$; $q^{v}=\frac{1}{6}q^{v}$; &c. $N=\frac{aa}{rr}$.

Then $s=\overline{1-q''N+q^{v}N^{2}-q^{vi}N^{3}+q^{viii}N^{4}+q^{x}N^{5}, \&c.} \times a$.

$=a-q''a^{3}r^{-2}+q^{v}a^{5}r^{-4}-q^{vi}a^{7}r^{-6}+q^{viii}a^{9}r^{-8}$, &c.

$=a-\frac{1}{2.3}AN+\frac{1}{4.5}BN-\frac{1}{6.7}CN+\frac{1}{8.9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

And $s'=r-q'a^{2}r^{-1}+q'''a^{4}r^{-3}-q^{v}a^{6}r^{-5}+q^{vii}a^{8}r^{-7}$, &c.

$=\overline{1-q'N+q'''N^{2}-q^{v}N^{3}+q^{vii}N^{4}\quad q^{x}N^{5}, \&c.} \times r$.

$=r-\frac{1}{1.2}a^{2}r^{-1}+\frac{1}{3.4}AN-\frac{1}{5.6}BN+\frac{1}{7.8}CN$, &c. A, B, C, &c. as before.

XXII. Also $v=q'a^{2}r^{-1}-q^{v}a^{4}r^{-3}+q^{vi}a^{6}r^{-5}-q^{viii}a^{8}r^{-7}$, &c.

$=\frac{1}{1.2}a^{2}r^{-1}-\frac{1}{3.4}AN-\frac{1}{5.6}BN-\frac{1}{7.8}CN-\frac{1}{9.10}DN$, &c.

$=\frac{1}{1.2}N-\frac{1}{3.4}AN-\frac{1}{5.6}BN-\frac{1}{7.8}CN$, &c. $\times r$. A, B, C, &c. as before.

XXIII. Let $A=+q'\ -q''$

$B=-q'''+q^{v}\ +Aq'$

$C=+q^{v}\ -q^{vi}\ +Bq'-Aq'''$

$D=-q^{vii}+q^{viii}\ +Cq'-Bq'''+Aq^{v}$, &c.

And $A'=-A$

$B'=-B-AA'$

$C'=-C-BA'-AB'$

$D'=-D-CA'-BB'-AC'$, &c.

Tangent

Tangent $t = a + Aa^3r^{-2} + Ba^5r^{-4} + Ca^7r^{-6} + Da^9r^{-8}$, &c.

Or $= \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \text{\&c.}} \times a$.

Co-tangent $t' = a^{-1}r^2 + Aa + B'a^3r^{-2} + C'a^5r^{-4} + D'a^7r^{-6}$, &c.

Or $= \overline{rr + Aa^2 + B'Na^2 + C'N^2a^2 + D'N^3a^2, \text{\&c.}} \times \frac{1}{a}$.

XXIV. Also let $\alpha = +q'$

$$\beta = -q''' + \alpha q'$$
$$\gamma = +q^{v} - \alpha q''' + \beta q'$$
$$\delta = -q^{vii} + \alpha q^{v} - \beta q''' + \gamma q', \text{\&c.}$$

And $\alpha' = +q''$

$$\beta' = -q^{iv} + \alpha' q''$$
$$\gamma' = +q^{vi} - \alpha' q^{iv} + \beta' q''$$
$$\delta' = -q^{viii} + \alpha' q^{vi} - \beta' q^{iv} + \gamma' q'', \text{\&c.}$$

Secant $s = r + \alpha a^2r^{-1} + \beta a^4r^{-3} + \gamma a^6r^{-5} + \delta a^8r^{-7}$, &c.

Or $= \overline{1 + \alpha N + \beta N^2 + \gamma N^3 + \delta N^4, \text{\&c.}} \times r$.

Co-secant $s' = a^{-1}r^2 + \alpha' a + \beta' a^3r^{-2} + \gamma' a^5r^{-4} + \delta' a^7r^{-6}$, &c.

Or $= \overline{rr + \alpha' aa + \beta' Naa + \gamma' N^2aa + \delta' N^3aa, \text{\&c.}} \times \frac{1}{a}$. where $N = \frac{aa}{rr}$.

XXV. Putting $p' = \frac{1}{2}$; $p'' = \frac{3}{4}p'$; $p''' = \frac{5}{6}p''$; $p^{iv} = \frac{7}{8}p'''$; $p^{v} = \frac{9}{10}p^{iv}$; &c. $N = \frac{ss}{rr}$.

Then arc $a = \overline{1 + \frac{1}{3}p'N + \frac{1}{5}p''N^2 + \frac{1}{7}p'''N^3 + \frac{1}{9}p^{iv}N^4, \text{\&c.}} \times s$.

Or $= s + \frac{1}{3}p'AN + \frac{1}{5}p''BN + \frac{1}{7}p'''CN + \frac{1}{9}p^{iv}DN$, &c.

Or $= s + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN$, &c.

where A, B, C, &c. stand for the respective preceding terms.

XXVI. If v is the versed sine of an arc a, diameter being d, $M = \frac{v}{d}$, $R = \sqrt{dv}$.

Then arc $a = \overline{1 + \frac{1.1}{2.3}M + \frac{3.3}{4.5}AM + \frac{5.5}{6.7}BM + \frac{7.7}{8.9}CM, \text{\&c.}} \times R$; A, B, C, &c. are as before.

XXVII.

XXVII. And putting $N = \frac{tt}{rr}$, $A = t$, $B = AN$, $C = BN$, $D = CN$, *&c.*

Then arc $a = t - \frac{1}{3}AN + \frac{1}{5}BN - \frac{1}{7}CN + \frac{1}{9}DN + \frac{1}{11}EN$, *&c.*

Or $= 1 - \frac{1}{3}N + \frac{1}{5}N^2 - \frac{1}{7}N^3 + \frac{1}{9}N^4 + \frac{1}{11}N^5$, *&c.* $\times t$.

XXVIII. Also, if c is the chord of an arc (a); and $N = \frac{cc}{dd}$.

Then arc $a = c + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN$, *&c.*

where A, B, C, *&c.* stand for the respective preceding terms.

An Essay on Quantity; *occasioned by reading a Treatise, in which* Simple *and* Compound Ratio's *are applied to* Virtue *and* Merit, *by the Rev. Mr* Reid; *communicated in a Letter from the Rev.* Henry Miles *D. D. & F. R. S. to* Martin Folkes, *Esq; Pr. R. S. Read* Nov. 3 1748. N°. 489. p. 505. October, *&c.* 1748.

V. Since mathematical demonstration is thought to carry a peculiar evidence along with it, which leaves no room for further dispute; it may be of some use, or entertainment at least, to inquire to what subjects this kind of proof may be applied.

Mathematics contain properly the doctrine of measure; and the object of this science is commonly said to be quantity; therefore quantity ought to be defined, what may be measured. Those who have defined quantity to be whatever is capable of more or less, have given too wide a notion of it, which I apprehend has led some persons to apply mathematical reasoning to subjects that do not admit of it.

Pain and pleasure admit of various degrees, but who can pretend to measure them? Had this been possible, it is not to be doubted but we should have had as distinct names for their various degrees, as we have for measures of length or capacity; and a patient should have been able to describe the quantity of his pain, as well as the time it began, or the part it affected. To talk intelligibly of the quantity of pain, we should have some standard to measure it by; some known degree of it so well ascertained, that all men, when they talked of it, should mean the same thing; we should also be able to compare other degrees of pain with this, so as to perceive distinctly, not only whether they exceed or fall short of it, but how far, or in what proportion; whether by an half, a fifth, or a tenth.

Sect. I. *What Quantity is.*

Whatever has quantity, or is measurable, must be made up of parts, which bear proportion to one another, and to the whole; so that it may be increased by addition of like parts, and diminished by subtraction, may be multiplied and divided; and, in a word, may bear any proportion to another quantity of the same kind, that one line or number can bear to another. That this is essential to all mathematical quantity, is evident

dent from the first elements of algebra, which treats of quantity in general, or of those relations and properties which are common to all kinds of quantity, Every algebraical quantity is supposed capable not only of being increased and diminished, but of being exactly doubled, tripled, halfed, or of bearing any assignable proportion to another quantity of the same kind. This then is the characteristick of quantity; whatever has this property may be adopted into mathematicks; and it's quantity and relations may be measured with mathematical accuracy and certainty.

SECT. 2. *Of Proper and Improper Quantity.*

There are some quantities which may be called *proper*, and others *improper*. This distinction is taken notice of by *Aristotle*; but it deserves some explication.

I call that *proper* quantity which is measured by it's own kind; or which of it's own nature is capable of being doubled or tripled, without taking in any quantity of a different kind as a measure of it. Thus a line is measured by known lines, as inches, feet, or miles; and the length of a foot being known, there can be no question about the length of two feet, or of any part or multiple of a foot. And this known length, by being multiplied or divided, is sufficient to give us a distinct idea of any length whatsoever.

Improper quantity is that which cannot be measured by it's own kind; but to which we assign a measure by the means of some proper quantity that is related to it. Thus velocity of motion, when we consider it by itself, cannot be measured. We may perceive one body to move faster, another slower; but we can have no distinct idea of a proportion or *ratio* between their velocities, without taking in some quantity of another kind to measure them by. Having therefore observed, that by a greater velocity a greater space is passed over in the same time, by a less velocity a less space, and by an equal velocity an equal space; we hence learn to measure velocity by the space passed over in a given time, and to reckon it to be in exact proportion to that space: and having once assigned this measure to it, we can then, and not till then, conceive one velocity to be exactly double, or half, or in any other proportion to another; we may then introduce it into mathematical reasoning without danger of confusion, or error, and may also use it as a measure of other improper quantities.

All the kinds of proper quantity we know, may, I think, be reduced to these four, extension, duration, number, and proportion. Though proportion be measurable in it's own nature, and therefore hath proper quantity; yet as things cannot have proportion which have not quantity of some other kind, it follows, that whatever has quantity must have it in one or other of these three kinds, extension, duration, or number. These are the measures of themselves, and of all things else that are measurable.

Number is applicable to some things, to which it is not commonly applied by the vulgar. Thus, by attentive consideration, lots and chances of various kinds appear to be made up of a determinate number of chances that are allowed to be equal; and by numbering these, the va-

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lues and proportions of those which are compounded of them may be demonstrated.

Velocity, the quantity of motion, density, elasticity, the *vis insita*, and *impressa*, the various kinds of centripetal forces, and different orders of fluxions, are all improper quantities; which therefore ought not to be admitted into mathematics, without having a measure of them assigned. The measure of an improper quantity ought always to be included in the definition of it; for it is the giving it a measure that makes it a proper subject of mathematical reasoning. If all mathematicians had considered this as carefully as Sir *I. Newton* appears to have done, some labour had been saved both to themselves and to their readers. That great man, whose clear and comprehensive understanding appears, even in his definitions, having frequent occasion to treat of such improper quantities, never fails to define them, so as to give a measure of them, either in proper quantities, or in such as had a known measure. This may be seen in the definitions prefixed to his *Princip. Phil. Nat. Math.*

It is not easy to say how many kinds of improper quantity may in time, be introduced into mathematics, or to what new subjects measures may be applied: but this I think we may conclude, that there is no foundation in nature for, nor can any valuable end be served by, applying measure to anything but what has these two properties. First, it must admit of degrees of greater and less. Secondly, it must be associated with, or related to, something that has proper quantity, so as that when one is increased, the other is increased, when one is diminished, the other is diminished also; and every degree of the one, must have a determinate magnitude or quantity of the other, corresponding to it.

It sometimes happens, that we have occasion to apply different measures to the same thing. Centripetal force, as defined by *Newton*, may be measured various ways, he himself gives different measures of it, and distinguishes them by different names, as may be seen in the above-mentioned definitions.

In reality, I conceive that the applying of measures to things that properly have not quantity, is only a fiction or artifice of the mind, for enabling us to conceive more easily, and more distinctly to express and demonstrate, the properties and relations of those things that have real quantity. The propositions contained in the two first books of *Newton*'s *principia* might perhaps be expressed and demonstrated, without those various measures of motion, and of centripetal and impressed forces which he uses: but this would occasion such intricate and perplexed circumlocutions, and such a tedious length of demonstrations as would fright any sober person from attempting to read them.

SECT 3. *Corol* 1. From the nature of quantity we may see what it is that gives mathematics such advantage over other sciences, in clearness and certainty; namely, that quantity admits of a much greater variety of relations than any other subject of human reasoning; and at the same time, every relation or proportion of quantities may, by the help of lines and numbers,

be

be so distinctly defined, as to be easily distinguished from all others, without any danger of mistake. Hence it is, that we are able to trace it's relations through a long process of reasoning, and with a perspicuity and accuracy which we in vain expect in subjects not capable of mensuration.

Extended quantities, such as lines, surfaces, and solids, besides what they have in common with all other quantities, have this peculiar, That their parts have a particular place and disposition among themselves: a line may not only bear any assignable proportion to another, in length or magnitude, but lines of the same length may vary in the disposition of their parts; one may be streight, another may be part of a curve of any kind or dimension, of which there is an endless variety, The like may be said of surfaces and solids. So that extended quantities, admit of no less variety with regard to their form, than with regard to their magnitude: and as their various forms may be exactly defined and measured, no less than their magnitudes, hence it is that geometry, which treats of extended quantity, leads us into a much greater compass and variety of reasoning than any other branch of methematicks. Long deductions in algebra for the most part are made, not so much by a train of reasoning in the mind, as by an artificial kind of operation, which is built on a few very simple principles: But in geometry, we may build one proposition upon another, a third upon that, and so on, without ever coming to a limit which we cannot exceed. The properties of the more simple figures can hardly be exhausted, much less those of the more complex ones.

It may I think be deduced from what hath been above said, that mathematical evidence is an evidence *sui generis*, not competent to any proposition which does not express a relation of things measurable by lines or numbers. All proper quantity may be measured by these, and improper quantities must be measured by those, that are proper. Sect. 4. Coroll. 2.

There are many things capable of more and less, which perhaps are not capable of mensuration. Tastes, smells, the sensations of heat and cold, beauty, pleasure, all the affections and appetites of the mind, wisdom, folly, and most kinds of probability, with many other things too tedious to enumerate, admit of degrees, but have not yet been reduced to measure, nor, as I apprehend, ever can be. I say, most kinds of probability, because one kind of it, *viz.* the probability of chances is properly measurable by number, as is above observed.

Although attempts have been made to apply mathematical reasoning to some of these things, and the quantity of virtue and merit in actions has been measured by simple and compound *ratio's*; yet I do not think that any real knowledge has been struck out this way: it may perhaps, if discretely used, be a help to discourse on these subjects, by pleasing the imagination, and illustrating what is already known; but until our affections and appetites shall themselves be reduced to quantity, and exact measures of their various degrees be assigned, in vain shall we essay to measure virtue and merit by them. This is only to ring changes upon words, and to make a ſhew of mathematical reaſoning, without advancing one ſtep in real knowledge.

Sect. 5. *Coroll.* 3. I apprehend the account that hath been given of the nature of proper and improper quantity may alſo throw ſome light upon the controverſy about the force of moving bodies, which long exerciſed the pens of many mathematicians, and for what I know is rather dropped than ended; to the no ſmall ſcandal of mathematics, which hath always boaſted of a degree of evidence, inconſiſtent with debates that can be brought to no iſſue.

Though philoſophers on both ſides agree with one another, and with the vulgar in this, that the force of a moving body is the ſame, while it's velocity is the ſame, is increaſed, when it's velocity is increaſed, and diminiſhed, when that is diminiſhed. But this vague notion of force, in which both ſides agree, though perhaps ſufficient for common diſcourſe, yet is not ſufficient to make it a ſubject of mathematical reaſoning: In order to that, it muſt be more accurately defined, and ſo defined, as to give us a meaſure of it, that we may underſtand what is meant by a double or a triple force. The *ratio* of one force to another cannot be perceived but by a meaſure; and that meaſure muſt be ſettled not by mathematical reaſoning, but by a definition. Let any one conſider force without relation to any other quantity, and ſee whether he can conceive one force exactly double to another; I am ſure I cannot, nor ſhall, till I ſhall be endowed with ſome new faculty; for I know nothing of force but by it's effects, and therefore can meaſure it only by it's effects. Till force then is defined, and by that definition a meaſure of it aſſigned, we fight in the dark about a vague idea, which is not ſufficiently determined to be admitted into any mathematical propoſition. And when ſuch a definition is given, the controverſy will preſently be ended.

Sect. 6. *Of the Newtonian meaſure of force.* You ſay, the force of a body in motion is as it's velocity: either you mean to lay this down as a definition as *Newton* himſelf has done; or you mean to affirm it as a propoſition capable of proof. If you mean to lay it down as a definition, it is no more than if you ſhould ſay, I call that a double force which gives a double velocity to the ſame body, a triple force which gives a triple velocity, and ſo on in proportion. This I intirely agree to; no mathematical definition of force can be given that is more clear and ſimple, none that is more agreeable to the common uſe of the word in language. For, ſince all men agree, that the force of the body being the ſame, the velocity muſt alſo be the ſame; the force being increaſed or diminiſhed, the velocity muſt be ſo alſo, what can be more natural or proper, than to take the velocity for the meaſure of the force?

Several other things might be advanced to ſhew that this definition agrees beſt with the common popular notion of the word Force. If two bodies meet directly with a ſhock, which mutually deſtroys their motion without producing any other ſenſible effect, the vulgar would pronounce, without heſitation, that they met with equal force; and ſo they do, according to the meaſure of force above laid down: for we find by experience, that in this caſe their velocities are reciprocally as their quantities

tities of matter. In Mechanics, where by a machine two powers or weights are kept in *equilibrio*, the vulgar would reckon that theſe powers act with equal force, and ſo by this definition they do. The power of gravity being conſtant and uniform, any one would expect that it ſhould give equal degrees of force to a body in equal times, and ſo by this definition it does. So that this definition is not only clear and ſimple, but it agrees beſt with the uſe of the word Force in common language, and this I think is all that can be deſired in a definition.

But if you are not ſatisfied with laying it down as a definition, that the force of a body is as it's velocity, but will needs prove it by demonſtration or experiment; I muſt beg of you, before you take one ſtep in the proof, to let me know what you mean by force, and what by a double or a triple force. This you muſt do by a definition which contains a meaſure of force. Some primary meaſure of force muſt be taken for granted, or laid down by way of definition; otherwiſe we can never reaſon about it's quantity. And why then may you not take the velocity for the primary meaſure as well as any other? you will find none that is more ſimple, more diſtinct, or more agreeable to the common uſe of the word Force: and he that rejects one definition that has theſe properties, has equal right to reject any other. I ſay then, that it is impoſſible, by mathematical reaſoning or experiment, to prove that the force of a body is as it's velocity, without taking for granted the thing you would prove, or ſomething elſe that is no more evident than the thing to be proved.

Let us next hear the *Leibnitzian*, who ſays, that the force of a body is as the ſquare of it's velocity. If he lays this down as a definition, I ſhall rather agree to it, than quarrel about words, and for the future ſhall underſtand him, by a quadruple force to mean that which gives a double velocity, by nine times the force that which gives three times the velocity, and ſo on in duplicate proportion. While he keeps by his definition, it will not neceſſarily lead him into any error in Mathematics or Mechanics. For, however paradoxical his concluſions may appear, however different in words from theirs who meaſure force by the ſimple *ratio* of the velocity; they will in their meaning be the ſame: juſt as he who would call a foot twenty-four inches, without changing other meaſures of length, when he ſays a yard contains a foot and a half, means the very ſame as you do, when you ſay a yard contains three feet. Sect. 7. *Of the* Leibnitzian *meaſure of Force.*

But tho' I allow this meaſure of force to be diſtinct, and cannot charge it with falſhood, for no definition can be falſe, yet I ſay in the firſt place, it is leſs ſimple than the other; for why ſhould a duplicate *ratio* be uſed where the ſimple *ratio* will do as well? In the next place, this meaſure of force is leſs agreeable to the common uſe of the word Force, as hath been ſhewn above; and this indeed is all that the many laboured arguments and experiments, brought to overturn it, do prove. This alſo is evident, from the paradoxes into which it has led it's defenders.

We are next to conſider the pretences of the *Leibnitzian*, who will undertake to prove by demonſtration, or experiment, that force is as the ſquare of the velocity. I aſk him firſt, what he lays down for the firſt meaſure of force? the only meaſure I remember to have been given by the philoſophers of that ſide, and which ſeems firſt of all to have led *Leibnitz* into his notion of force, is this: the height to which a body is impell'd by any impreſſed force, is, ſays he, the whole effect of that force, and therefore muſt be proportional to the cauſe: but this height is found to be as the ſquare of the velocity which the body had at the beginning of it's motion.

In this argument I apprehend that great man has been extremely unfortunate. For, 1*ſt*, whereas all proof ſhould be taken from principles that are common to both ſides, in order to prove a thing we deny, he aſſumes a principle which we think farther from the truth; namely, that the height to which the body riſes is the whole effect of the impulſe, and ought to be the whole meaſure of it. 2*dly*, His reaſoning ſerves as well againſt him as for him: for may I not plead with as good reaſon at leaſt thus? the velocity given by an impreſſed force is the whole effect of that impreſſed force; and therefore the force muſt be as the velocity. 3*dly*, Suppoſing the height to which the body is raiſed to be the meaſure of the force, this principle overturns the concluſion he would eſtabliſh by it, as well as that which he oppoſes. For, ſuppoſing the firſt velocity of the body to be ſtill the ſame; the height to which it riſes will be increaſed, if the power of gravity is diminiſhed; and diminiſhed, if the power of gravity is increaſed. Bodies deſcend ſlower at the equator, and faſter towards the poles, as is found by experiments made on pendulums. If then a body is driven upwards at the equator with a given velocity, and the ſame body is afterwards driven upwards at *Leipſick* with the ſame velocity, the height to which it riſes in the former caſe will be greater than in the latter; and therefore according to his reaſoning, it's force was greater in the former caſe; but the velocity in both was the ſame; conſequently the force is not as the ſquare of the velocity any more than as the velocity.

SECT. 8. *Reflections on this controverſy.*

Upon the whole, I cannot but think the controvertiſts on both ſides have had a very hard taſk; the one to prove, by mathematical reaſoning and experiment, what ought to be taken for granted; the other by the ſame means to prove what might be granted, making ſome allowance for impropriety of expreſſion, but can never be proved.

If ſome mathematician ſhould take it in his head to affirm, that the velocity of a body is not as the ſpace it paſſes over in a given time, but as the ſquare of that ſpace; you might bring mathematical arguments and experiments to confute him; but you would never by theſe force him to yield, if he was ingenuous in his way; becauſe you have no common principles left you to argue from, and you differ from one another, not in a mathematical propoſition, but in a mathematical definition.

Suppoſe a philoſopher has conſider'd only that meaſure of centripetal force which is proportional to the velocity generated by it in a given time, and from this meaſure deduces ſeveral propoſitions. Another

ther philoſopher in a diſtant country, who has the ſame general notion of centripetal force, takes the velocity generated by it, and the quantity of matter together, as the meaſure of it. From this he deduces ſeveral concluſions, that ſeem directly contrary to thoſe of the other. Thereupon a ſerious controverſy is begun, whether centripetal force be as the velocity, or as the velocity and quantity of matter taken together. Much mathematical and experimental duſt is raiſed; and yet neither party can ever be brought to yield; for they are both in the right, only they have been unlucky in giving the ſame name to different mathematical conceptions. Had they diſtinguiſhed theſe meaſures of centripetal force as *Newton* has done, calling the one *Vis centripetæ quantitatis acceleratrix*, the other *quantitas motrix*; all appearance of contradiction had ceaſed, and their propoſitions, which ſeem ſo contrary, had exactly tallied.

CHAP. II.

OPTICKS.

I. I have obſerved, in Mr *Baker's Microſcope made eaſy, edit.* 1743, *p.* 47, that Mr *Martin* has invented a Micrometer, to be applied to any Microſcope whatſoever. I have for ſome years made uſe of another ſort of Micrometer, which I have applied to one of Mr *Scarlet's* Microſcopes, and placed it in the *focus* of the firſt eye-glaſs. It is a very ſmall piece of the thinneſt black ſilk, divided into very minute ſquares, and is extended on a little ring of wood or paper, in ſuch a manner, that it may conveniently be placed in the *focus* of the firſt eye-glaſs. Theſe ſquares indeed are not all of the ſame magnitude. But, as this conduces greatly to the more eaſy and convenient enumeration of them, which would be impoſſible if they were all of the ſame magnitude, ſo it is little or no hindrance in deducing the concluſions. For as often as I have counted 20, 30, or 40 of theſe ſquares, according to one line of the Micrometer, or fine ſilk. I have proceeded in counting the whole line, and let me begin to count from which end I will, I have always compared it exactly with ſome certain object placed under the Microſcope; and thus I have found the number of little ſquares to anſwer to the diameter of the object ſo juſtly, that there is very ſeldom half a ſquare too much or too little, which may very ſafely be ſlighted in ſuch an incomprehenſible ſubtility of objects.

Of the Application of a Micrometer to a Microſcope; by Sam Chriſtian Hollman, *Profeſſ. Pub. Ord. in the Univerſity of* Gottingen. N°. 475. p. 239. Jan. &c. 1745.

When by repeated experiments I had found, that the diameter of an object was inlarged at leaſt 27 times, I allowed the augmentation to be only 25 times, that I might be certain that the augmentations of the following glaſſes, found by my Micrometer, were not greater, but leſs than the truth, when I had them found, that N°. 1 of the ſame Microſcope magnified 250 times, and that the *animalcula ſeminalia humana*, without

without their tails, appeared hardly ſo big as a large cheeſe-mite does to the naked eye, it became evident, that 15,625,000 of theſe animalcules were contained in the ſpace of a cheeſe-mite. And yet I have obſerved much ſmaller animalcules than theſe, in an infuſion of common pepper, or even of common hay, after it had ſtood for ſome days. By the uſe of the ſame Micrometer, I alſo found two ways of determining the quantity of ſeminal animalcules in the milt of a fiſh, more accurately than had been done by *Leuwenhoeck*. I ſhall only add at preſent, that one cubical decimal line of a Rheniſh foot, in the milt of a carp, contained above 244,140,625 ſeminal animalcules; and that the whole milt of a carp, weighing not quite 2 Norrinberg pounds, which had 1084 grains, made about 2080 cubical decimal lines, as I found by a hydroſtatical experiment. That whole milt therefore contained above 507,812,500,000 ſeminal animalcules. But if we ſuppoſe the half of this milt only to conſiſt of animalcules, and the other half to be a fluid in which they live, which will eaſily be allowed to exceed the truth, by all thoſe who have obſerved how ſmall a proportion of fluid there is in the ſeed of this fiſh, before it has been diluted by water; there will, even upon this ſuppoſition, be more than 253,906,250,000 living animalcules in the ſeed of a carp, weighing leſs than 2 Norrinberg pounds; which tho' it is beyond the reach of our imagination, does not exceed the power of the infinite creator.

Gottingen, Oct. 15, 1744.

Of fallacious Viſion thro' compound Microſcopes; by Philip Fredrick Gmelin, *Med. Licent. Wurterbergenſis.* N°. 476. p. 386, April &c. 1745. *Preſented* May 9. 1745.

II. Being informed by a friend, that if a common ſeal was applied to the *focus* of a compound Microſcope, or optical tube, which has 2 or 3 convex, or plano-convex glaſſes, that part which is cut the deepeſt in, would appear very convex, and ſo on the contrary; and that ſometimes, but very ſeldom, it would appear in the ſame ſtate, as to the naked eye, I was deſirous to make the obſervation myſelf; and found it conſtantly to happen, as my friend told me. I thought the experiment worthy of being farther proſecuted: and accordingly, on the 16th of laſt *April*, the morning not being very clear, but in a pretty light chamber, I viewed a watch hanging againſt a plain wall, thro' the left part of an optical tube: the whole of it appeared concave, and fixed into the wall. I alſo obſerved ſome flies, that were running about the wall, and they appeared in like manner. I alſo viewed a ſmall globe of a Thermometer filled with red ſpirit: and this alſo ſeemed hollow, and fixed within the frame. I found the ſame to happen with the raiſed parts of garments of all colours, and with the brazen protuberances of a ſmall cabinet; all which appeared concave, and deeply ſunk into the cloth and wood. I alſo viewed a ſmall ſtags head, cut in wood, and hanging horizontally on the wall; this alſo appeared concave, and fixed into the wall.

After this I obſerved a ball of one of Fahrenheit's Thermometors, full of quickſilver: but it did not change it's natural convexity; nor did the empty glaſs ball of the inverted Thermometer, hanging againſt the wall, tho'

tho' the lower ball of the ſame filled with red ſpirit, and that alſo of Fahrenheit's filled with ſpirit loſt their convexity. Hence I preſently concluded, that white or ſhining uncoloured bodies appeared under the *focus* of this tube in the ſame manner as they appear to the naked eye, at the ſame time I muſt fairly acknowledge, that an aſſiſting friend has ſometimes made obſervations directly oppoſite to mine in the ſame circumſtances: nay, in a darker day I myſelf have found my obſervations quite contrary to thoſe which I had made the day before. Hence, tho' the obſervation with the ſeal held conſtantly the ſame, I imagined there muſt be ſome particular circumſtances hitherto unobſerved, in which theſe objects appeared thus perverted. I therefore endeavoured to diſcover ſome certain laws, according to which theſe perverted objects always appeared when expoſed to theſe *foci*, and ſome others, according to which they conſtantly appeared as when they were expoſed to the naked eye. After various experiments I partly obtained my end.

As often as I viewed any object, riſing upon a plane, of what colour ſoever, provided it was neither white nor ſhining, with the eye and optical tube directly oppoſite to it, the elated parts appeared depreſſed, and the depreſſed parts elated, as it happened in the ſeal, as often as I held the tube perpendicularly, and brought it in ſuch a manner, that it's whole ſurface almoſt covered the laſt glaſs orb of the tube; and in like manner it happened under the compound Microſcope. But as often as I viewed any of the other objects depending perpendicularly from a perpendicular plane, in ſuch a manner, that the tube was ſupported in a horizontal ſituation directly oppoſite to it, the ſame always happened, and the appearance was not altered, when the object hung obliquely or even horizontally. I was mightily delighted with the obſervation of a tobacco pipe, which had a porcellane bowl of a ſnowy whiteneſs, and a tube of horn almoſt black, and hung obliquely from a horn; the bowl preſerved it's natural convexity, and the tube was deeply ſunk, and ſeemed to be almoſt immerſed in the wall. I alſo obſerved, that when I placed the watch horizontally on a horizontal plane, and then looked on it perpendicularly, near the window, it no longer appeared ſo depreſſed, and ſurrounded with a ſhady ring; whence I began to ſuſpect, that all theſe fallacies were owing to ſhade, juſt as Painters can elevate or depreſs a figure by making the ground lighter or deeper. Thus when the raiſed object was ſo placed between the windows, that it might be illuminated on all ſides, it did not change it's convexity. But at laſt I diſcovered a method of making objects always appear with their natural convexity. If any object hung againſt a wall, or was contiguous to it in any ſituation whatſoever, I viewed ſidewiſe in ſuch a manner as not to oppoſe the tube directly againſt it, but below the eminence near the plain at ſome diſtance. By theſe means the protuberances of the cabinet, and other objects, always appeared to me with their true natural convexity. With regard to the ſeal, I held it in ſuch a manner, that the

the whole circumference was perpendicular, or rather a little inclined. Then I applied the lower rim of the tube exactly to the upper margin of the disk of the seal, so that the tube formed an obtuse angle with the seal; then carefully preserving the same situation, I very gently moved the tube from the rim of the seal upon it's face; and thus I always saw the seal with it's true natural face. But why all these things happen exactly after this manner, I do not pretend to determine, nor why white, or uncoloured transparent shining bodies, rising in any manner above any plane, afford an exception from this rule of Vision, and do not appear depressed when viewed after the method above mentioned.

C H A P. III.

A S T R O N O M Y.

A letter to George *Earl of* Macclesfield, *concerning an* apparent Motion *observed in some of the* fixed Stars; *by the Rev.* James Bradley, *D.D. Astron. Reg. F. R. S.* N°. 485. p. 1. Jan. 1747-8. *Read* Jan. 7, 1747.

I. THE great exactness, with which instruments are now constructed, hath enabled the Astronomers of the present age to discover several changes in the positions of the heavenly bodies; which, by reason of their *smallness*, had escaped the notice of their predecessors. And altho' the causes of such motions have always subsisted, yet philosophers had not so fully consider'd, what the effects of those known causes would be, as to demonstrate *a priori* the *phænomena* they might produce; so that theory itself is here, as well as in many other cases, indebted to practice, for the discovery of some of it's most elegant deductions. This points out to us the great advantage of cultivating *this*, as well as every other branch of natural knowledge, by a regular series of observations and experiments.

The progress of Astronomy indeed has always been found, to have so great a dependence upon accurate observations, that, till such were made, it advanced but slowly: for the first considerable improvements that it received, in point of theory, were owing to the renowned *Tycho Brahè*; who far exceeding those that had gone before him, in the exactness of his observations, enabled the sagacious *Kepler* to find out some of the principal laws, relating to the motion of the heavenly bodies. The invention of telescopes and pendulum-clocks affording proper means of still farther improving the *praxis* of Astronomy; and these being also soon succeeded by the wonderful discoveries made by our great *Newton*, as to it's theory; the science, in both respects, had acquired such extraordinary advancement, that future ages seemed to have little room left, for making any great improvements. But, in fact, we find the case to be very different; for, as we advance in the means of making more nice inquiries, new points generally offer themselves, that demand our attention. The subject of my present letter to to your Lordſhip, is a proof of the truth of this remark: for, as ſoon as I had diſcovered the cauſe, and ſettled the laws of the aberrations of the fixed ſtars, ariſing from the motion of light, *&c.* * my attention was again excited by another *new phænomenon*, *viz.* an annual change of declination in ſome of the fixed ſtars; which appeared to be ſenſibly *greater* about that time, than a preceſſion of the equinoctial points of 50″ in a year would have occaſioned. The quantity of the difference, tho' ſmall in itſelf, was rendered perceptible, thro' the exactneſs of my inſtrument, even in the firſt year of my obſervations; but being then at a loſs to gueſs, from what cauſe that greater change of declination proceeded, I endeavoured to allow for it in my computations, by making uſe of the *obſerved* annual difference, as mentioned in the ſame Paper.

From *that* time to the preſent, I have continued to make obſervations at *Wanſted*, as opportunity offered, with a view of diſcovering the laws and cauſe of this *phænomenon:* for, by the favour of *Matthew Wymondeſold*, Eſq; my inſtrument has remained, where it was firſt erected; ſo that I have been able, without any interruption, which the removal of it to another place would have occaſioned, to proceed on with my intended ſeries of obſervations, for the ſpace of twenty years: a term ſomewhat exceeding the whole period of the changes, that happen in this *phænomenon.*

When I ſhall mention the *ſmall* quantity of the deviation, which the ſtars are ſubject to, from the cauſe that I have been ſo long ſearching after; I am apprehenſive, that I may incur the cenſure of ſome perſons, for having ſpent ſo much time in the purſuit of ſuch a ſeeming trifle: but the candid lovers of ſcience will, I hope, make due allowance for that natural ardour, with which the mind is urged on towards the diſcovery of truths, in themſelves perhaps of *ſmall* moment, were it not that they tend to illuſtrate others of greater uſe.

The apparent motions of the heavenly bodies are ſo complicated, and affected by ſuch a variety of cauſes; that in many caſes it is extremely difficult to aſſign to each it's due ſhare of influence; or diſtinctly to point out, what part of the motion is the effect of one cauſe, and what of another: and whilſt the joint effects of *all* are only attended to, great irregularities and ſeeming inconſiſtencies frequently occur; whereas, when we are able to allot to each particular cauſe it's proper effect, harmony and uniformity uſually enſue.

Such ſeeming irregularities being alſo blended with the unavoidable errors, to which aſtronomical obſervations muſt be always liable, as well from the imperfection of our ſenſes, as of the inſtruments that we uſe, have often very much perplex'd thoſe, who have attempted to ſolve the *phænomena:* and till means are diſcovered, whereby we can ſeparate and diſtinguiſh the *particular* part of the whole motion, that is owing to each

* See Vol. vi. Part I. Chap. iii. Sect. 5.

respective cause, it will be impossible, to be well assured of the truth of any solution. For these reasons, we generally find, that the more exact the instruments are, that we use, and the more regular the series of observations is, that we take; the sooner we are enabled to discover the cause of any new *phænomenon*. For when we can be well assured of the limits, wherein the errors of the observations are contain'd; and have reduced them within as narrow bounds as possible, by the perfection of our instruments; we need not hesitate to ascribe such apparent changes, as manifestly exceed those limits, to some other causes. Upon these accounts it is incumbent upon the *practical* Astronomer, to set out at first with the examination of the correctness of his instruments; and to be assured that they are sufficiently exact for the use he intends to make of them: or at least he should know, within what limits their errors are confined.

This practice has, in an eminent manner, been lately recommended by your Lordship's noble example; who having, out of a singular regard for the science of Astronomy, erected an observatory, and furnished it with as complete an *apparatus* of instruments, as our best artists could contrive; would not fully rely on their exactness, till their divisions had undergone the strictest re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human skill can at present produce.

The lovers of *this* science in general, cannot but acknowledge their obligations to your Lordship on this account; but I find myself more particularly bound to do it; since, by means of your most accurate observations, I have been enabled to settle some *principal elements*; which I could not at present otherwise have done, for want of an instrument at the Royal Observatory, *proper* for that purpose: for the large *mural quadrant*, which is there fixed to observe objects lying southward of the zenith, however *perfect* an instrument it may be in itself, is not alone sufficient to determine, with proper exactness, either the *latitude* of the Observatory, or the quantity of refraction corresponding to different altitudes: for it being too heavy to be conveniently removed; and the room wherein it is placed, being too small to admit of it's being turned to the opposite side of the wall, whereon it now hangs; I cannot, by *actual* observations of the circumpolar stars, settle those necessary points; and therefore have endeavoured to do it, by comparing my own with your Lordship's observations: and until this defect in the *apparatus* belonging to the Royal Observatory be removed, we must be indebted to your Lordship, for the knowledge of it's true situation.

A mind intent upon the pursuit of any kind of knowledge, will always be agreeably entertained, with what can supply the most proper means of attaining it: such, to the practical Astronomer, are exact and well-contriv'd instruments; and I reflect with pleasure on the opportunities I have enjoyed, of cultivating an acquaintance and friendship with the person, that, of all others, has most contributed to their improvement.

ment. For I am ſenſible, that if my own endeavours have, in any reſpect, been effectual to the advancement of Aſtronomy; it has principally been owing to the advice and aſſiſtance given me by Mr *George Graham*; whoſe great ſkill and judgment in mechanicks, join'd with a complete and practical knowlenge of the uſes of aſtronomical inſtruments, enable him to contrive and execute them in the moſt perfect manner.

The Gentlemen of the *Royal Academy* of *Sciences*, to whom we are ſo highly obliged for their exact admeaſurement of the quantity of a degree under the arctic circle, have already given the world very convincing proofs of *his* care and abillities in thoſe reſpects; and the particular delineation, which they have lately publiſhed, of the ſeveral parts of the *ſector*, which he made for them, hath now rendered it needleſs, to enter upon any minute deſcription of *mine* at *Wanſted*; both being conſtructed upon the ſame principles, and differing in their component parts, chiefly on account of the different purpoſes, for which they were intended.

As mine was originally deſigned to take only the *differences* of the zenith diſtances of ſtars, in the various ſeaſons of the year, without any view of diſcovering their *true* places; I had no occaſion to know exactly, what point on the limb correſponded to the *true* zenith: and therefore no proviſion was made in my ſector, for the changing of it's ſituation for that purpoſe. Neither was it neceſſary that the diviſions or points on the arc ſhould be ſet off, with the utmoſt accuracy, equidiſtant from each other; becauſe, when I obſerve any particular ſtar, the ſame ſpot or point being firſt biſected by the plumb-line, and then the ſcrew of the Micrometer turn'd until the ſtar appears upon the middle of the wire, that is fixed in the common focus of the glaſſes of the Teleſcope; I can thereby collect, how far the ſtar is from that given point at the time of obſervation: and afterwards, by comparing together the ſeveral obſervations that are made of it, I am able to diſcover what apparent change has happen'd. The quantity of the viſible alteration, in the poſition of the ſtars, being expreſſed by revolutions and parts of a revolution, of the ſcrew of the Micrometer; I endeavoured to determine, with great care, the true angle anſwering thereto: and after various trials, I thoroughly ſatisfied myſelf, both of the equality of the threads of the ſcrew, and of the preciſe number of ſeconds correſponding to them.

But altho' theſe points could be ſettled with great certainty, I was nevertheleſs obliged to make one ſuppoſition; which perhaps to ſome perſons may ſeem of too great moment in the preſent inquiry, to be admitted without an evident proof from facts and experiments. For I *ſuppoſe*, that the line of collimation of my Teleſcope has invariably preſerved the ſame direction, with reſpect to the diviſions upon the arc, during the whole courſe of my obſervations, And indeed it was on account of the objections, which might have been raiſed againſt ſuch a *poſtulate*, that I thought it neceſſary, to continue my ſeries of obſervations for ſo many

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years, before I published the conclusions, which I shall at present endeavour to draw from them.

Whoever compares the result of the several trials, that have been made by the gentlemen of the *Academy* of *Sciences*, for determining the zenith point of their sector, since their return from the north; will, I presume, allow that *mine* is not an unreasonable or precarious *supposition:* since it is evident, from their observations, that the line of collimation of that instrument underwent no sensible change in it's direction, during the space of more than a whole year; altho' it was several times taken down, and set up again in different and remote places; whereas *mine* hath always remained suspended in the same place.

But besides such a strong argument for the probability of the truth of my *supposition*, I have the satisfaction of finding it *actually* verified by the observations themselves; which plainly prove, that at the end of the full period of the deviations which I am going to mention, the stars are found to have the same positions by the instrument, as they ought to *have*, supposing the line of collimation to have continued unaltered from the time when I first began to observe.

I have already taken notice, in what manner this *phænomenon* discover'd itself to me at the end of my first year's observations, *viz.* by a *greater* apparent change of declination in the stars near the equinoctial colure, than could arise from a precession of 50″ in a year; the mean quantity now usually allowed by Astronomers. But there appearing at the same time, an effect of a quite contrary nature, in some stars near the solstitial colure, which seem'd to alter their declination *less* than a precession of 50″ required; I was thereby convinced, that all the *phænomena*, in the different stars, could not be accounted for, merely by supposing, that I had assumed a wrong quantity for the precession of the equinoctial points.

At first, I had a suspicion, that some of these small apparent alterations in the places of the stars, might possibly be occasioned by a change, in the materials, or in the position of the parts of my sector: but, upon considering how firmly the arc, on which the divisions or points are made, is fastened to the plate, wherein the wire is fixed that lies in the focus of the object-glass; I saw no reason to apprehend, that any change could have happened in the position of that wire and those points. The suspension therefore of the plummet being the most likely cause, from whence I conceived any uncertainty could arise; and the wire of which had been broken three or four times in the first year of my observations: I attempted to examine, whether part of the 'foremention'd apparent motions might not have been owing, to the different plumb-lines that had been made use of. In order to determine this, I adjusted a particular point of the arc to the plumb-line, with all the exactness I could; and then taking off the old wire, I immediately hung on another, with which the same spot was again compared. I repeated the experiment three or four times, and thereby fully satisfied myself, that no sensible

sible error could arise from the use of different plumb-lines; since the various adjustments of the same point agreed with each other, within less than half a second.

Having then, from such trials, sufficient reason to conclude, that these *second* unexpected deviations of the stars, were not owing to any imperfection of my instrument; after I had settled the laws of the aberrations arising from the motion of light, *&c.* I judged it proper to continue my observations of the same stars; hoping that, by a regular and longer series of them, carried on thro' several succeeding years, I might, at length, be enabled to discover the *real* cause of such apparent inconsistencies.

As I resided chiefly at *Wansted*, after my sector was erected there in 1727, till the beginning of *May* 1732, when I removed from thence to *Oxford*: I had, during my abode at *Wansted*, frequent opportunities of repeating my observations; and thereby discovered so many particulars relating to these *phænomena*, that I began to guess what was the real cause of them.

It appeared from my observations, that, during this interval of time, some of the stars near the solstitial colure, had changed their declinations 9″ or 10″ *less*, than a precession of 50″ would have produced; and, at the same time, that, others near the equinoctial colure, had altered theirs about the same quantity *more*, than a like precession would have occasioned: the north pole of the equator seeming to have approached the stars, which come to the meridian with the sun, about the vernal equinox and the winter solstice; and to have receded from those, which come to the meridian with the sun, about the autumnal equinox and the summer solstice.

When I consider'd these circumstances, and the situation of the ascending node of the moon's orbit, at the time when I first began my observations; I suspected, that the moon's action upon the equatorial parts of the earth might produce these effects: for, if the precession of the equinox be, according to Sir *I. Newton*'s principles, caused by the actions of the sun and moon upon those parts; the plane of the moon's orbit being at *one* time, above ten degrees *more* inclined to the plane of the equator, than at *another*; it was reasonable to conclude, that the part of the whole annual precession, which arises from her action, would in different years be varied in it's quantity; whereas the plane of the ecliptic, wherein the sun appears, keeping always nearly the same inclination to the equator; *that* part of the precession, which is owing to the sun's action, may be the same every year: and from hence it would follow, that, altho' the *mean* annual precession, proceeding from the joint actions of the sun and moon, were 50″; yet the *apparent* annual precession might sometimes exceed, and sometimes fall short, of that mean quantity, according to the various situations of the nodes of the moon's orbit.

In 1727, when my instrument was first set up, the moon's ascending node was near the beginning of *aries*; and consequently, her orbit was as much inclined to the equator, as it can at any time be; and then the *ap-*

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parent annual preceſſion was found, by my firſt year's obſervations, to be greater than the *mean:* which proved, that the ſtars near the equinoctial colure, whoſe declinations are moſt of all affected by the preceſſion, had changed *theirs*, above $\frac{1}{10}$ more than a preceſſion of 50″ would have cauſed. The ſucceeding years obſervations proved the ſame thing; and in 3 or 4 years time the difference became ſo conſiderable, as to leave no room to ſuſpect, that it was owing to any imperfection, either of the inſtrument or obſervations.

But ſome of the ſtars, which I had obſerved, that were near the ſolſtitial colure, having appeared to move, during the ſame time, in a manner contrary to what they ought to have done, by an increaſe in the preceſſion; and the deviations in them being as remarkable as in the others, I perceived that ſomething more, than a meer change in the quantity of the preceſſion, would be requiſite to ſolve this part of the *phænomenon.* Upon comparing my obſervations of ſtars near the ſolſtitial colure, that were almoſt oppoſite to each other in right aſcenſion, I found, that they were equally affected by this cauſe; for whilſt *γ draconis* appeared to have moved northward, the ſmall ſtar, which is the 35th *Camelopardali Hevel.* in the *Britiſh* catalogue, ſeem'd to have gone as much towards the ſouth: which ſhew'd, that this apparent motion, in both thoſe ſtars, might proceed from a nutation in the earth's axis; whereas the compariſon of my obſervations of the ſame ſtars, *formerly* enabled me to draw a different concluſion, with reſpect to the cauſe of the annual aberrations ariſing from the motion of light. For the apparent alteration in *γ draconis*, from *that* cauſe, being as great again as in the other ſmall ſtar, proved, that *that phænomenon* did not proceed from a *nutation* of the earth's axis; as, on the contrary, *this* may. Upon making the like compariſon between the obſervations of other ſtars, that lie nearly oppoſite in right aſcenſion, whatever their ſituations were with reſpect to the cardinal points of the equator, it appeared, that their change of declination was nearly equal, but contrary; and ſuch as a nutation or motion of the earth's axis would effect.

The moon's aſcending node being got back towards the beginning of *capricorn* in 1732, the ſtars near the equinoctial colure appeared, about that time, to change their declinations no more, than a preceſſion of 50″ required; whilſt ſome of thoſe near the ſolſtitial colure altered *theirs* above 2″ in a year leſs, than they ought. Soon after, I perceived the annual change of declination of the former to be diminiſhed, ſo as to become *leſs* than 50″ of preceſſion would cauſe; and it continued to diminiſh till 1736, when the moon's aſcending node was about the beginning of *libra*, and her orbit had the *leaſt* inclination to the equator. But by this time, ſome of the ſtars near the ſolſtitial colure had altered their declinations 18″ *leſs*, ſince 1727, than they ought to have done from a preceſſion of 50″. For *γ draconis*, which in thoſe 9 years ſhould have gone about 8″ more *ſoutherly*, was obſerved in 1736, to appear 10″ more *northerly*, than it did in 1727.

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As this appearance in *γ draconis*, indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic; and as several Astronomers have supposed *that* inclination to diminish regularly: if this *phænomenon* depended upon such a cause, and amounted to 18″ in 9 years, the obliquity of the ecliptic would, at that rate, alter a whole minute in 30 years; which is much faster than any observations, *before* made, would allow. I had reason therefore to think, that *some part* of this motion at the least, if not the *whole*, was owing to the moon's action upon the equatorial parts of the earth; which I conceived, might cause a libratory motion of the earth's axis. But as I was unable to judge, from only 9 years observations, whether the axis would entirely recover the same position, that it had in 1727. I found it necessary to continue my observations thro' a whole period of the moon's nodes; at the end of which I had the satisfaction to see, that the stars returned into the same positions again; as if there had been no alteration at all in the inclination of the earth's axis: which fully convinced me, that I had guessed rightly as to the cause of the *phænomena*. This circumstance proves likewise, that if there be a gradual diminution of the obliquity of the ecliptic; it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself: because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane.

During the course of my observations, as Mr *Machin* was employed in considering the theory of gravity, and it's consequences, with regard to the celestial motions; I acquainted him with the *phænomena* that I had observed: and at the same time mentioned, *what* I suspected to be the cause of them. He soon after sent me a table, containing the quantity of the annual precession in the various positions of the moon's nodes, as also the corresponding nutations of the earth's axis; which was computed upon the *supposition*, that the *mean* annual precession is 50″, and that the whole is governed by the pole of the moon's orbit only: and therefore he imagined, that the numbers in the table would be too *large*; as in fact they were found to be. But it appeared, that the changes which I had observed, both in the annual precession and nutation, kept the same law, as to increasing and decreasing, with the numbers of his table. Those were calculated upon the supposition, that the pole of the equator, during a period of the moon's nodes, moved round in the periphery of a little circle, whose center was 23° 29′ distant frrm the pole of the ecliptic; having itself also an angular motion of 50″ in a year, about the same pole: the north pole of the equator was conceived to be in *that* part of the small circle, which is farthest from the N. pole of the ecliptic, at the time when the moon's ascending node is in the beginning of *aries*: and in the opposite point of it, when the same node is in *libra*.

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Such an hypothesis will account for an acceleration and retardation of the annual preceffion; as also for a nutation of the earth's axis: and if the diameter of the little circle be supposed equal to 18″; which is the whole quantity of the nutation, as collected from my observations of *γ draconis:* then all the *phænomena* in the several stars which I observed, will be very nearly solved by it.

Fig. 1. Let *P* represent the mean place of the pole of the equator, about which point, as a center, suppose the true pole to move in the circle *ABCD*, whose diameter is 18″. Let *E* be the pole of the ecliptic, and *EP* be equal to the mean distance between the poles of the equator and ecliptic; and suppose the true pole of the equator to be at *A*, when the moon's ascending node is in the beginning of *aries*; and at *B*, when the node gets back to *capricorn*; and at *C*, when the same node is in *libra:* at which time the N. pole of the equator being nearer the N. pole of the ecliptic, by the whole diameter of the little circle *AC* equal to 18″; the obliquity of the ecliptic will then be so much *less* than it was, when the moon's ascending node was in *aries.* The point *P* is supposed to move round *E*, with an equal retrograde motion, answerable to the mean precession arising from the joint actions of the sun and moon: while the true pole of the equator moves round *P*, in the circumference *ABCD*, with a retrograde motion likewise, in a period of the moon's nodes, or of eighteen years, and seven months. By this means, when the moon's ascending node is in *aries*, and the true pole of the equator at *A*, is moving from *A* towards *B:* it will approach the stars, that come to the meridian with the sun about the vernal equinox; and recede from those that come with the sun near the autumnal equinox, *faster* than the *mean* pole *P* does. So that, while the moon's node goes back from *aries* to *capricorn*, the *apparent* precession will seem so much *greater* than the *mean*; as to cause the stars, that lie in the equinoctial colure, to have altered their declination 9″, in about 4 years and 8 months, *more* than the mean precession would do: and in the same time, the N. pole of the equator will seem to have approached the stars, that come to the meridian with the sun at our winter solstice, about 9″; and to have receded as *much* from those, that come with the sun at the summer-solstice.

Thus the *phænomena* before recited are in general conformable to this

Fig. 2. hypothesis. But to be more particular; let *S* be the place of a star, *PS* the circle of declination passing thro' it, representing it's distance from the mean pole, and *γPS* it's mean right ascension. Then if *O* and *R* be the points, where the circle of declination cuts the little circle *ABCD*; the *true* pole will be nearest that star at *O*, and farthest from it at *R*; the whole difference amounting to 18″, or to the diameter of the little circle. As the true pole of the equator is supposed to be at *A*, when the moon's ascending node is in *aries*; and at *B*, when that node gets beck to *capricorn*; and the angular motion of the true pole about *P*, is likewise supposed equal to that of the moon's node about *E*, or the pole of the ecliptic: since, in these cases, the true pole of the equator

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is 90 degrees before the moon's aſcending node, it muſt be ſo in all others.

When the true pole is at A, it will be at the ſame diſtance from the ſtars that lie in the equinoctial colure, as the mean pole P is, for I neglect at preſent the caſe of ſuch ſtars as are *very* near the pole of the equator; and as the true pole recedes back from A towards B, it will approach the ſtars, that lie in that part of the colure repreſented by P ♈; and recede from thoſe, that lie in P ♎; not indeed with an *equable* motion; but in the *ratio* of the *ſine* of the diſtance of the moon's node from the beginning of *aries*. For if the node be ſuppoſed to have gone backwards from *aries* 30°, or to the beginning of *piſces*; the point, which repreſents the place of the true pole, will in the mean time, have moved in the little circle, thro' an arc, as AO, of 30° likewiſe: and would therefore in effect have approached the ſtars that lie in the equinoctial colure P ♈, and have receded from thoſe that lie in P ♎, $4''\frac{1}{2}$; which is the *ſine* of 30° to the *radius* AP. For if a perpendicular fall from O upon PA, it may be conceived as part of a great circle, paſſing thro' the true pole and any ſtar lying in the equinoctial colure. Now the ſame proportion, that holds in theſe ſtars, will obtain likewiſe in all others; and from hence we may collect a general rule, for finding how much nearer or farther, any particular ſtar is, to or from, the *mean* pole, in any given poſition of the moon's node.

For, if *from the R. aſcenſion of the ſtar, we ſubſtract the diſtance of the moon's aſcending node from aries; then the radius will be to the ſine of the remainder, as* $9''$, *is to the number of ſeconds, that the ſtar is nearer to, or farther from the true, than the mean pole.* When that remainder is *leſs* than 180°, the ſtar is *nearer* to the true, than to the mean pole; and the contrary, when it is *greater* than 180°.

This motion of the *true* pole, about the *mean* at P, will alſo produce a change in the R. aſcenſions of the ſtars, and in the places of the equinoctial points; as well as in the obliquity of the ecliptic: and the quantity of the equations, in either of theſe caſes, may be eaſily computed for any given poſition of the moon's nodes. But as it may be needleſs, to dwell longer on the explication of the hypotheſis; I ſhall now proceed to ſhew it's correſpondency with the *phænomena*, relating to the alterations of the polar diſtances of ſome of the ſtars which I have obſerved: by laying before your Lordſhip the obſervations themſelves, together with the computations that are neceſſary; in order to form a right judgment about the cauſe of theſe appearances.

I have endeavoured to find the exact quantity of the *mean* preceſſion of the equinoctial points, by comparing my own obſervations made at *Greenwich*, with thoſe of *Tycho Brahè* and others, which I judged to be moſt proper for that purpoſe. But as many of the ſtars, which I compared, gave a different quantity; I ſhall aſſume the mean reſult; which gives a preceſſion of one degree in ſeventy-one years and an half: this agreeing very well likewiſe with my obſervations that were taken at *Wanſted.*

Wanſted. The numbers in the following tables, which expreſs the change of declination in each ſtar, are computed upon the ſuppoſition, that the *mean* obliquity of the ecliptic was 23°. 28′. 30″, and that it continued the *ſame*, during the whole courſe of my obſervations. And as the moon's aſcending node was in the beginning of *aries* about *March* 27th 1727, I have reduced the place of each ſtar to *that* time; by allowing the proper change of declination from that day, to the day of each reſpective obſervation.

It being alſo neceſſary to make an allowance for the *aberrations* of light; I have again examined my obſervations, that were moſt proper to determine the tranſverſe axis of the ellipſis, which each ſtar ſeems to deſcribe; and have found it to be neareſt to 40″; which number I therefore make uſe of in the following computations.

The diviſions or points upon the limb of my ſector are placed five minutes of a degree from each other; and are numbered ſo, as to ſhew the polar diſtances nearly; the *true* polar diſtance exceeding that, which is ſhewn by the inſtrument, about 1′. 35″. When I firſt began to obſerve, I generally made uſe of *that* point on the limb, which was neareſt to the ſtar's polar diſtance, without regarding whether it was more northerly, or more ſoutherly than the ſtar: but as it ſometimes happened, that the original point, with which I at firſt compared the ſtar, became, in proceſs of time, pretty remote from it; I afterwards brought the plummet to another point, that was nearer to it; and carefully examined, what number of revolutions of the ſcrew of the Micrometer, *&c.* correſponded to the diſtance between the different points, that I had made uſe of: by which means I was able to reduce all the obſervations of the ſame ſtar to the ſame point, without ſuppoſing the ſeveral diviſions to be accurately 5′ aſunder.

I have expreſſed the diſtance of each ſtar from the point of the arc, with which it was compared, in *ſeconds* of a degree and *tenth parts* of a ſecond, exactly as it was collected from the obſervations; altho' I am ſenſible, that the obſervations themſelves are liable to an error of more than a *whole* ſecond; becauſe I meet with ſome, that have been made within two or three days of each other, that differ 2″, even when they are not marked as *defective* in any reſpect.

It would be too tedious, to ſet down the whole number of the obſervations that I have made; and therefore I ſhall give only enough of them, to ſhew their correſpondency with the 'forementioned hypotheſis in the ſeveral years, wherein any were made of the ſtars here recited. When *ſeveral* obſervations have been taken of the ſame ſtar, within a few days of each other; I have either ſet down the mean reſult, or *that* obſervation which beſt agreed with it. I have likewiſe commonly choſen thoſe, that were made near the ſame ſeaſon of the year, in ſuch ſtars as gave me the opportunity of making that choice; particularly in γ *draconis*, which was generally obſerved about the end of *Auguſt* or the beginning of *September*; *that* being the uſual time, when I went to *Wanſted*

Wanſted on purpoſe to obſerve both *that*, and alſo ſome of the ſtars in the *great Bear.* But the weather proving cloudy at that ſeaſon in 1744, prevented my making a ſingle obſervation, either of γ *draconis*, or any other ſtar, while I was there; which is the cauſe of one vacancy in a ſeries of 20 ſucceeding years, wherein that particular ſtar had been obſerved. Such ſtars, as were either not viſible in the day-time, towards the beginning of *September*, or came at ſuch hours in the night, as would have incommoded the family of the houſe wherein the inſtrument is fixed, were but ſeldom obſerved, after I went to reſide at *Oxford:* which is the reaſon, why the ſeries of obſervations of *thoſe* is ſo imperfect, as ſometimes to leave a chaſm for ſeveral years together. But notwithſtanding this, I doubt not, but upon the whole they will be found ſufficient, to ſatisfy your Lordſhip of the general correſpondency between the *hypotheſis* and the *phænomena*, in the ſeveral ſtars; however different their ſituations are, with reſpect to the cardinal points of the equator.

As I made more obſervations of γ *draconis* than of any other ſtar; and it being likewiſe very near the zenith of *Wanſted*; I will begin with the recital of ſome of them. The point upon the limb, with which this ſtar was compared, was 38°. 25′ from the N. pole of the equator, according to the numbers of the arc of my *ſector*. The firſt column, in the following table, ſhews the year and the day of the month, when the obſervations were made; the next gives the number of *ſeconds*, that the ſtar was found to be *S.* of 38°. 25′: the third contains the alterations of the polar diſtance, which the *mean* preceſſion, at the rate of one degree in 71 ½ years, would cauſe in this ſtar, from *March* 27th 1727, to the day on which the obſervation was taken: the fourth ſhews the aberrations of light: the fifth, the equations ariſing from the 'forementioned hypotheſis: and the ſixth gives the *mean* diſtance of the ſtar from the point with which it was compared, found, by collecting the ſeveral numbers, according to their ſigns, in the 3d, 4th, and 5th columns, and applying them to the *obſerved diſtances* contain'd in the ſecond.

If the obſervations had been perfectly exact, and the ſeveral equations of their *due* quantity; then all the numbers in the laſt column would have been equal; but ſince they differ a little from one another; if the *mean* of all be taken, and the extremes are compared with it, we ſhall find no greater difference, than what may be ſuppoſed to ariſe from the uncertainty of the obſervations themſelves; it no where amounting to more than 1″ ½. The hypotheſis therefore ſeems, in this ſtar, to agree extremely well with the obſervations here ſet down; but as I had made above 300 of it; I took the trouble of comparing each of them with the hypotheſis: and altho' it might have been expected, that, in ſo large a number, ſome great errors would have occurred; yet there are very few, only 11, that differ from the mean of theſe ſo much as 2″; and not one that differs ſo much as 3″. This ſurpriſing agreement, therefore, in ſo long a ſeries of obſervations, taken in all the various ſeaſons of the year, as well as in the different poſitions of the moon's nodes, seems to be a sufficient proof of the truth, both of *this* hypothesis, and also of *that* which I formerly advanced, relating to the aberrations of light; since the polar distance in this star may differ, in certain circumstances, almost a minute, *viz.* 56″ ½, if the corrections resulting from both these hypotheses are neglected; whereas, when those equations are rightly applied, the mean place of the star comes out the same, as nearly, as can be reasonably expected.

γ Draconis		South of ° ′ 38. 25	Precession.	Aberration.	Nutation.	Mean Dist.
		″	″	″	″	″
1727 September	3	70.5	— 0.4	+ 19.2	— 8.9	80.4
1728 March	18	108.7	— 0.8	— 19.0	— 8.6	80.3
September	6	70.2	— 1.2	+ 19.3	— 8.1	80.2
1729 March	6	108.3	— 1.6	— 19.3	— 7.4	80.0
September	8	69.4	— 2.1	+ 19.3	— 6.4	80.2
1730 September	8	68.0	— 2.9	+ 19.3	— 3.9	80.5
1731 September	8	66.0	— 3.8	+ 19.3	— 1.0	80.5
1732 September	6	64.3	— 4.6	+ 19.3	+ 2.0	81.0
1733 August	29	60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734 August	11	62.3	— 6.2	+ 16.9	+ 6.9	79.9
1735 September	10	60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736 September	9	59.3	— 8.0	+ 19.3	+ 9.0	79.6
1737 September	6	60.8	— 8.8	+ 19.3	+ 8.5	79.8
1738 September	13	62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739 September	2	66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740 September	5	70.8	— 11.3	+ 19.3	+ 1.9	80.7
1741 September	2	75.4	— 12.1	+ 19.2	— 1.1	81.4
1742 September	5	76.7	— 12.9	+ 19.3	— 4.0	79.1
1743 September	2	81.6	— 13.7	+ 19.1	— 6.4	80.6
1745 September	3	86.3	— 15.4	+ 19.2	— 8.9	81.2
1746 September	17	86.5	— 16.2	+ 19.2	— 8.7	80.8
1747 September	2	86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 observations of β *draconis*; which I find correspond as well with the hypothesis, as those of *γ*; but since the positions of both these stars, in respect to the solstitial colure, differ but little from each other; it will be needless to set down the observations of β. I shall therefore proceed to lay before your Lordship, some observations of a small star, that is almost opposite to *γ draconis* in R. ascension, being the 35th *Camelopardali Hevel.* in the *British* catalogue. Mr *Flamsteed*, indeed, has not given the R. ascension of this star; but *that* being necessary to be known, in order to compute the change of it's declination arising from the precession of the equinox; I compared the time of it's transit over the meridian, with that of some other stars near the same

ſame parallel; whereby I found, that it's R. aſcenſion was 85°. 54′. ½ at the beginning of the year 1737.

This ſmall ſtar was compared with the ſame point of the limb of my ſector, as *γ draconis*; and the ſecond column, in the following table, ſhews how many *ſeconds* it was found to be S. of that point, at the time of each reſpective obſervation. The other columns contain, as in the foregoing table, the equations that are neceſſary to find, what it's *mean* diſtance from the ſame point would have been on *March* 27th 1727, which is exhibited in the laſt column. The whole number of my obſervations of this ſtar did not much exceed 40; the greateſt part of which were made before 1730; in ſome of the following years none were taken; and only a ſingle one in any other, except in 1739. However, their correſpondency ſeems ſufficient to evince the truth of the hypotheſis: for if the mean of theſe, contain'd in the table, be taken, not one, among the reſt of the obſervations, will differ from it more than 2″.

35th *Camelopard. Hevelii.*			South of ° ′ 38. 25	Preceſ-ſion.	Aberra-tion.	Nutation	Mean Diſt South.
			″	″	″	″	″
1727	October	20	73.6	+ 0.9	− 6.7	+ 8.9	76.7
1728	January	12	60.8	1.2	+ 6.1	8.8	76.9
	March	1	57.8	1.4	+ 9.4	8.7	77.3
	September	26	75.2	2.3	− 8.8	8.1	76.8
1729	February	26	56.4	2.8	+ 9.4	7.6	76.2
1730	March	3	57.8	4.4	9.4	5.4	77.0
1731	February	5	59.1	5.6	8.5	+ 3.0	76.2
1733	January	31	64.1	8.7	8.2	− 2.9	78.1
1738	December	30	61.8	17.2	4.3	6.5	76.8
1739	February	4	56.9	17.3	8.5	6.3	76.4
1740	January	20	56.0	18.6	7.0	− 4.0	77.6
1747	February	27	32.3	28.5	9.4	+ 8.4	78.6

The obſervations of the foregoing ſtars are the moſt proper, to prove the change of the inclination of the earth's axis to the plane of the ecliptic; thoſe which follow, will ſhew in what manner the ſtars, that lie near the equinoctial colure, are affected, as well as others, that are differently ſituated, with reſpect to the cardinal points of the equator. Some of theſe ſtars are indeed more remote from the zenith, than I would have choſen, if there had been others, of equal luſtre, in more proper poſitions; becauſe experience has long ſince taught me, that the obſervations of ſuch ſtars, as lie near the zenith, do generally agree beſt with one another, and are therefore the fitteſt to prove the truth of any hypotheſis. I ſhall begin with thoſe near the vernal

equinox.

equinox. *α Caſſiopeæ* was compared with the point marked 34°. 55′; and at firſt was found to be more *ſoutherly*, but afterwards became more *northerly* than that point, as in the following table; the laſt column of which ſhews it's mean diſtance *S.* of that point on *March* 27, 1727. The obſervation of *Dec.* 23, 1738, differs 3″ from the *mean* of the others; as does alſo another, that was taken 5 days after this; neither of which being marked as uncertain, I judged it proper to inſert one of them; altho' they give the mean place of the ſtar near 2″ more *northerly* than any other, in a ſeries of above 100; *all* of which correſpond, with the *mean* of theſe here recited, within leſs than 2″; excepting *two*, that give the ſtars mean diſtance almoſt 3″ more *ſoutherly*; but theſe laſt mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with ſeveral others, that were made near the ſame time, from which they differ almoſt 2″.

α Caſſiopeæ.	South of ° ′ 34. 55	Preceſ-ſion.	Aberra-tion.	Nuta-tion.	Mean Diſt South
	″	″	″	″	″
1727 September 9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September 17	30.8	29.4	+ 4.6	5.2	70.0
1729 June 8	35.7	43.8	− 16.3	6.8	70.0
December 3	N. 9.4	53.5	+ 16.5	7.7	68.3
1730 June 11	S. 13.8	64.0	− 16.2	8.4	70.0
December 9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732 January 8	N. 49.2	95.4	12.9	8.9	68.0
1733 January 21	64.8	116.0	+ 10.0	7.9	69.1
1734 June 13	62.8	143.8	− 16.1	5.0	69.9
December 11	105.4	153.7	+ 16.2	+ 3.7	68.2
1738 December 23	176.3	234.0	+ 15.2	− 7.2	65.7
1740 June 2	169.1	262.8	− 16.5	− 8.9	68.3
1747 February 27	332.3	397.0	+ 0.2	+ 4.7	69.6

Altho' I have taken no obſervation of *τ Perſei* ſince *Jan.* 22, 1740; yet, as this ſtar is very near the zenith, and a ſufficient number were made about the times when the equation, reſulting from the hypotheſis, was at it's *maximum*; I judged it proper to inſert ſome of them in the next table; the laſt column of which ſhews, how much the ſtar's *mean* diſtance was *S.* of 38°. 20′. on *March* 27, 1727. Among near 60 obſervations I meet with 2 only, that differ from the mean of theſe ſo much as 2″; and thoſe differ almoſt as much from the mean of others, that were taken near the ſame time: ſo that the hypotheſis ſeems to correſpond, in general, with the obſervations of this ſtar as well, as with either of the foregoing.

τ Perſei.

τ Perſei.	South of ° ′ 38. 20	Preceſſion	Aberration.	Nutation.	Mean Diſt. South
	″	″	″	″	″
1727 September 16	60.1	+ 7.4	− 3.2	+ 6.7	71.0
December 29	39.7	11.9	+ 12.9	7.2	71.7
1728 December 21	22.5	27.2	12.8	8.7	71.2
1729 December 2	S. 9.2	42.0	11.5	9.0	71.7
1731 January 3	N. 8.2	59.0	12.8	8.3	71.9
1732 January 8	22.0	74.8	12.7	6.7	72.2
1733 January 21	34.6	91.0	11.7	+ 4.3	72.4
1738 December 23	117.0	183.4	12.8	− 9.0	70.2
1740 January 22	132.5	200.2	11.7	8.6	70.8

After the laſt recited obſervations, it may perhaps ſeem needleſs to add thoſe of *α Perſei*, which is farther from the zenith; but however, as this ſtar lies very nearly at an equal diſtance from the equinoctial and ſolſtitial colures, and the ſeries of obſervations of it is ſomewhat more complete, than that of *τ Perſei*; I ſhall inſert one at leaſt, for each year wherein it has been obſerved; whereby it may appear, that the hypotheſis ſolves the *phænomena* of ſtars in this ſituation, as exactly as in others: for if a *mean* be taken of the numbers in the laſt column of the following table, which expreſſes the *mean* diſtance of the ſtar *S.* of 41°. 5′. on *March* 27, 1727, it will agree within 2″ with every one of 80 obſervations, that have been made of this ſtar.

α Perſei	South of ° ′ 41. 5	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728 April 7	87.5	14.3	− 0.8	8.2	109.2
July 5	94.6	17.7	− 11.4	8.5	109.4
December 12	65.7	23.8	+ 10.6	8.8	108.9
1729 December 3	53.4	37.2	9.7	8.9	109.2
1731 January 3	38.6	52.3	11.4	7.8	110.1
1732 January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July 11	S. 21.3	101.0	− 11.4	− 1.1	109.8
1738 December 24	N. 56.3	162.6	+ 11.2	0.0	108.5
1740 January 21	71.8	177.4	10.0	− 8.2	108.3
1747 February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given examples of ſtars, lying near both the ſolſtices and the *vernal* equinox; I ſhall now add the obſervations of *one*, that is not far from the *autumnal* equinox, *viz. η urſæ majoris*, the brighteſt ſtar in that part of the heavens, which approaches the zenith of *Wanſted* within a degree; and which, by reaſon of it's luſtre and poſition, gave me me the opportunity of making my ſeries of obſervations of *it*, more complete than of many others. This ſtar was compared with the point marked 39°. 15′. and was *S.* of it as in the following table; wherein your Lordſhip will ſee, that the obſervations of the years 1740 and 1741 give the polar diſtances 3″ greater, than the *mean* of the other years. Had there been only a ſingle obſervation taken in either of thoſe years, part of this apparent difference might have been ſuppoſed to ariſe from their uncertainty; but as there were 8 obſervations taken within a week, either before or after *June* 3, 1740, which agree well with each other; and three were made within 20 days in *Sept.* 1741, which likewiſe correſponded with each other; I am inclined to think, that the 'forementiond differences muſt be owing to ſomething elſe, beſides the error of the obſervations. This *phænomenon* therefore may deſerve the conſideration of thoſe gentlemen, who have employed their time in making computations relating to the quantity of the effects, which the power of gravity may, on various occaſions, produce. For I ſuſpect, that the poſition of the moon's apogee, as well as of her nodes, has ſome relation to the apparent motions of the ſtars that I am now ſpeaking of.

My ſeries of obſervations of ſeveral ſtars abound, of late years, with ſo many and long interruptions; that I cannot pretend to *determine* this point; but probably the differences before taken notice of in the obſervations of α *Caſſiopeæ*, and ſome others that I have found likewiſe among the obſervations of *other* ſtars, that are not here recited, may be owing to ſuch a cauſe; which, altho' it ſhould not have any large ſhare of influence, may yet, in certain circumſtances, diſcover a defect in a hypotheſis, that pays no regard at all to *it*. But whether theſe differences do ariſe from the cauſe already hinted at; or whether they proceed from any defect of the hypotheſis itſelf in any other reſpect; it will not be very *material* in point of practice; ſince *that* hypotheſis, as it was before laid down, appears to be ſufficient to ſolve all the *phænomena*, to as great a degree of exactneſs, as we can in general *hope* or *expect* to make obſervations. For if I take the *mean* of all the numbers in the laſt column of the following table for η *urſæ majoris*, and compare it with any one of 164 obſervations that were taken of it, the difference will not exceed 3″.

η *Urſæ*

η *Ursæ Majoris*			South of ° ′ 39. 15	Precession.	Aberration.	Nutation.	Mean Dist. South.
			″	″	″	″	″
1727	October	17	153.3	− 10.2	+ 1.0	− 5.2	138.9
1728	January	24	176.4	15.2	− 17.6	5.8	137.8
	July	17	150.8	23.9	+ 17.8	6.9	137.8
	October	11	170.6	28.2	+ 2.6	7.3	137.7
1729	January	16	196.6	33.1	− 17.8	7.8	137.9
	July	21	170.4	42.4	+ 17.8	8.4	137.4
1730	July	19	189.6	60.6	+ 17.8	9.0	137.8
	December	28	232.4	68.7	− 16.7	8.9	138.1
1731	September	18	218.1	81.9	+ 9.4	8.4	137.2
1732	January	10	250.7	87.7	− 17.7	8.0	137.3
	April	13	238.7	92.3	− 0.8	7.7	137.9
1734	July	11	255.7	133.3	+ 17.6	− 2.3	137.7
1735	September	10	280.8	154.6	+ 11.4	+ 1.2	138.8
1736	September	8	294.7	172.8	11.6	4.1	137.6
1737	July	3	303.0	187.8	17.2	6.1	138.5
1738	June	29	319.0	205.8	16.8	7.9	137.9
1739	April	25	348.0	220.8	2.5	8.8	138.5
1740	June	3	360.3	241.1	12.8	8.9	140.9
1741	September	23	390.9	265.0	7.9	+ 7.4	141.2
1745	September	5	466.7	337.1	12.4	− 3.3	138.7
1746	September	20	492.0	356.2	8.8	5.9	138.7
1747	September	2	507.2	373.5	13.2	7.8	139.1

You may perceive, by inspecting the tables which contain the observations of α *Cassiopeæ* and η *ursæ majoris*; that the greatest differences that occur therein may be diminished, by supposing the *true* pole of the equator to move round the point *P*, in an *ellipsis*, instead of a circle. For if the transverse axis, lying in the direction *AC*, be 18″, and the conjugate, as *DB*, be about 16″; the equations, resulting from such an hypothesis, will make the numbers in the last columns agree with each other, nearer than as they now stand. But since this would not entirely remove the inequalities, in all the positions of the moon's nodes; I shall refer the more accurate determination of the *locus* of the *true* pole to theory; and at present only give the equations for the precession of the equinoctial points, and the obliquity of the ecliptic, as also the real quantity of the annual precession, to every 5th degree of the place of the moon's ascending node, in the following tables; just as they result from the hypothesis, as at first laid down; it appearing, from what has already been remark'd, that these will be sufficiently exact for practice in all cases.

The Equation of the Equinoct. Points.				
☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Subst / Add
°	″	″	″	°
0	0.0	1.3	19.6	30
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subst. / Add	Sig. V / Sig. X	IV / X	III / IX	☽'s ☊ from ♈

The Equation of the Obliquity of the Ecliptick				
☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Add / Subst
°	″	″	″	°
0	9.0	7.8	4.5	30
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add / Subst.	Sig. V / Sig. XI	IV / X	III / IX	☽'s ☊ from ♈

	The Annual Precession of the Equinoctial Points.						
☽'s ☊ from ♈	Sig. O	I	II	III	IV	V	
°	″	″	″	″	″	″	°
0	58.0	57.0	54.2	50.3	46.5	43.7	30
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	☽'s ☊ from ♈

Sir *I. Newton*, in determining the quantity of the annual preceſſion from the theory of gravity, upon ſuppoſition that the equatorial is to the polar diameter of the earth as 230 is to 229, finds the ſun's action ſufficient to produce a preceſſion of $9''\frac{1}{8}$ only; and, collecting from the tides the proportion between the ſun's force and the moon's to be as 1 to $4\frac{1}{2}$, he ſettles the mean preceſſion, reſulting from their joint actions, at $50''$. But ſince the difference between the polar and equatorial diameter is found, by the late obſervations of the gentlemen of the *Academy of Sciences*, to be greater than what Sir *Iſaac* had computed it to be; the preceſſion, ariſing from the ſun's action, muſt likewiſe be greater than what he has ſtated it at, nearly in the ſame proportion. From whence it will follow, that the moon's force muſt bear a leſs proportion to the ſun's than $4\frac{1}{2}$ to 1; and perhaps the *phænomena*, which I have now been giving an account of, will ſupply the beſt *data* for ſettling this matter.

As I apprehend, that the obſervations already ſet down will be judged ſufficient, to prove in general the truth of the hypotheſis before advanced; I ſhall not trouble your Lordſhip with the recital of more, that I made of ſtars lying at greater diſtances from the zenith; thoſe not being ſo proper, for the reaſon before-mentioned, to eſtabliſh the point that I had chiefly in view. But as it may perhaps be of ſome uſe to future Aſtronomers, to know what were the *mean* differences of declination, at a given time, between ſome ſtars, that lie nearly oppoſite to one another in right aſcenſion, and not far from either of the *Colures*; I ſhall ſet down the reſult of the compariſon of a few, that differ ſo little in declination, that I could determine the quantity of that difference with great certainty.

By the *mean* of 64 obſervations, that were made of α *Caſſiopeæ* before the end of 1728, I collect, after allowing for the preceſſion, aberration and nutation as in the foregoing tables; that the *mean* diſtance of this ſtar was 68″.7 *S.* of 34°. 55′, on *March* 27, 1727. By a like compariſon of 40 obſervations, taken of γ *urſæ majoris* during the ſame interval of time, I find this ſtar was, at the ſame time, 39″.6 *S.* of 34°. 45′. I carefully meaſured, with the ſkrew of the micrometer, the diſtance between the points, with which theſe ſtars were compared; and found them to be 9′. 59″ from each other, or one ſecond leſs than they ought to have been. Hence it follows, that the *mean* difference of declination between theſe two ſtars, was 10′. 28″.1, on *March* 27, 1727.

By the *mean* of 65 obſervations, that were taken of β *Caſſiopeæ*, before the end of the year 1728, this ſtar was 25″.8 *N.* of 32°. 20′, on the 27th day of *March* 1727: and by the *mean* of 52 obſervations, ε *urſæ majoris* was 87″.6 *S.* of 32°. 30′ at the ſame time. The diſtance between theſe points was found to be 9′. 59″.3; from whence it follows, that the *mean* difference of declination between theſe two ſtars was 11′. 52″.7 on *March* 27, 1727.

By the *mean* of 100 obſervations, taken before the end of the year 1728, the *mean* diſtance of γ *draconis* was 79″.8 *S.* of 38°. 25′ on *March* 27, 1727; and by the *mean* of 35 obſervations, the 35th *camelopard. Hevel.* was *S.* of the ſame ſpot 76″.4. So that the mean polar diſtance of γ *draconis* was only 3″.4 greater, than that of 35th *camelopard. Hevel.* But as the equation for the nutation, in both theſe ſtars, was then near the *maximum*, and to be applied with contrary ſigns; the *apparent* polar diſtance of γ *draconis* was 21″.4 greater, on *March* 27, 1727.

The differences of the polar diſtances of the ſtars, as here ſet down, may be preſumed, both on account of the radius of the inſtrument and the number of obſervations, to be very exactly determined, to the time when the moon's aſcending node was at the beginning of *Aries*; and if a like compariſon be hereafter made, of obſervations taken of the ſame ſtars, near the ſame poſition of the moon's nodes; future Aſtronomers may be enabled, to ſettle the quantity of the mean preceſſion of the equinox, ſo far as it affects the declination of theſe ſtars, with great certainty: and they may likewiſe diſcover, by means of the ſtars, near the ſolſtitial colure, from what cauſe the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminiſh gradually.

The forementioned points indeed can be ſettled only on the ſuppoſition, that the angular diſtances of theſe ſtars do continue always the ſame, or that they have no real motion in themſelves; but are at reſt in abſolute ſpace. A ſuppoſition, which though uſually made by Aſtronomers, nevertheleſs ſeems to be founded on too uncertain principles, to be admitted in all caſes. For if a judgment may be formed, with regard to this matter, from the reſult of the compariſon of our beſt modern obſervations, with ſuch as were formerly made with any tolerable degree of exactneſs; there appears to have been a real change in the poſition of ſome of the fixed ſtars, with reſpect to each other; and ſuch, as ſeems independent of any motion in our own ſyſtem, and can only be referred to ſome motion in the ſtars themſelves. *Arcturus* affords a ſtrong proof of this: for if it's preſent declination be compared with it's place, as determined either by *Tycho* or *Flamſteed*; the difference will be found to be much greater, than what can be ſuſpected to ariſe from the uncertainty of their obſervations.

It is reaſonable to expect, that other inſtances of the like kind muſt alſo occur among the great number of the viſible ſtars: becauſe their relative poſitions may be altered by various means. For if our own ſolar ſyſtem be conceived to change it's place, with reſpect to abſolute ſpace; this might, in proceſs of time, occaſion an apparent change in the angular diſtances of the fixed ſtars; and in ſuch a caſe, the places of the neareſt ſtars being more affected, than of thoſe that are very remote; their relative poſitions might ſeem to alter; tho' the ſtars themſelves were really immoveable. And on the other hand, if our own ſyſtem be at reſt, and any of the ſtars really in motion, this might likewiſe vary their apparent poſitions; and the more ſo, the nearer they are to us, or the ſwifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since then the relative places of the ſtars may be changed from ſuch a variety of cauſes, conſidering that amazing diſtance at which it is certain ſome of them are placed, it may require the obſervations of many ages, to determine the laws of the apparent changes, even of a ſingle ſtar: much more difficult therefore muſt it be, to ſettle the laws relating to all the moſt remarkable ſtars.

When the cauſes, which affect the places of all the ſtars in general are known; ſuch as the preceſſion, aberration, and nutation; it may be of ſingular uſe, to examine nicely the relative ſituations of particular ſtars: and eſpecially of thoſe of the greateſt luſtre, which, it may be preſumed lie neareſt to us, and may therefore be ſubject to more ſenſible changes; either

either from their own motion, or from that of our ſyſtem. And if at the ſame time that the brighter ſtars are compared with each other, we likewiſe determine the relative poſitions of ſome of the *ſmalleſt* that appear near them, whoſe places can be aſcertained with ſufficient exactneſs; we may perhaps be able to judge to what cauſe the change, if any be obſervable, is owing. The uncertainty that we are at preſent under, with reſpect to the degree of accuracy wherewith former Aſtronomers could obſerve, makes us unable to determine ſeveral things, relating to the ſubject that I am now ſpeaking of: but the improvements, which have of late years been made in the methods of taking the places of the heavenly bodies, are ſo great, that a few years may hereafter be ſufficient, to ſettle ſome points; which cannot now be ſettled, by comparing even the earlieſt obſervations with thoſe of the preſent age.

It were to be wiſhed therefore, that ſuch perſons as are provided with proper inſtruments, would attempt to determine, with great care, the preſent relative poſitions of ſeveral of the principal ſtars, in various parts of the heavens; eſpecially of thoſe, that are leaſt affected by refraction: *that* Cauſe having many times ſo uncertain an influence on the places of objects, that are very remote from the zenith; that wherever *It* is concerned, the concluſions, deduced from obſervations that are *much* affected by it, will always remain doubtful, and too precarious, in many caſes, to be relied upon.

The advantages, ariſing from different perſons attempting to ſettle the ſame points of Aſtronomy near the ſame time, are ſo much the greater; as a concurrence in the reſult, would remove all ſuſpicion of incorrectneſs in the inſtruments made uſe of. For which reaſon, I eſteem the curious *apparatus* at *Shirburn Caſtle*, and the obſervations there taken, as a moſt valuable *criterion*, whereby I may judge of the accuracy of thoſe, that are made at the *Royal Obſervatory*: and as a lover of ſcience I cannot but wiſh, that our nation abounded with more frequent examples, of perſons of like rank and ability with your Lordſhip, equally deſirous of promoting *This*, as well as every other branch of natural knowledge, that tends to the honour and benefit of our country.

Greenwich, Dec. 31, 1747.

Declinations of ſome Southern Stars *of the 1ſt and 2d magnitude, in* June 1738, *with the method of finding the time at ſea in the night, by the aſpect of the* Southern Croſs, *by* M. de la Conda-

II. Theſe declinations are taken from various obſervations, made with a quadrant of 3 feet, in *June* 1737, 1738, *&c.* at *Quito* in *America*, in 0° 13′ 16″ *S.* Lat. in a place 11″ more to the *S.* and the place of obſervation of the ſolſtices, *Dec.* 1736 and *June* 1737, the latitude of which I had already determined in my diſcourſe concerning the diſtance of the tropicks. In calculating theſe declinations, I made uſe of *M. Bouguer*'s table of refractions for the height of *Quito*, inſerted in the *Memoires de l'Acad.* 1738.

	° ′ ″
In the ſhip *Argo* α *Canopus*, a ſtar of the 1ſt magnitude, and the greateſt of the fixed ſtars, excepting *Sirius*.	52 34 16

One

naine, *R. S.* Lond. & *A cad. R. Sc.* Par. Soc. No 402 p. 139. *April, &c* 1749. *Read* May 11, 1749.

	°	′	″
One in the preceding or occidental arm of the *southern cross* (*Bayero* γ) of a middle magnitude between the 2d and 3d.	57	17	32
ζ a star in the foot of the *cross*, is the nearest to the pole of the 4 stars which constitute the *cross*, being viewed by a telescope, appears double, but to the naked eye single, and of the 1st magnitude.	61	38	57
ϵ the most northern in the top of the *cross*, of the 2d magnitude, ξ in the following arm of the 2d magnitude.	58	15	5
In the preceding or occidental foot of the *centaur* γ of the 1st magnitude.	59	5	35
In the following foot of the *centaur* α of the 1st magnitude.	59	44	56

When we observed at *Panama*, in *Jan.* 1736, we found by repeated trials, that about 2′ are to be added to the declination of the star *canopus* in the *British Catalogue*, to make the latitude of the place taken by the observations of that star to agree with that taken from the altitudes: and this remark was confirmed by all the following observations, and particularly by those on which the above declination of *canopus* depends: whence it appears to be greater by 2′ 2″ than in the *British Catalogue*. All the stars above-mentioned are very bright, and the most visible of any in the *S.* hemisphere, that are not seen in *Europe*.

In most planispheres the southern cross is variously represented: in some it's situation is from *N.* to *S.* in others from *N. E.* to *S. W.* *Pardie*'s celestial chart of the southern part of the heavens, gives two schemes of the southern cross, one in the former, the other in the latter direction, the first of which is the true one. The southern cross therefore, when it is in the meridian, appears upright, that is, perpendicular to the horizon, and therefore may serve mariners to find out the hour without any sensible error, the difference of time being known between it and the sun's passing the meridian, may be easily accommodated to practice by the following method.

From repeated observations reduced to the present year 1749, I gather, that there are about 4′ 30″ between the mediation of the stars ζ and ϵ in the foot and head of the southern cross, and that the first reaches the meridian about 13′ after it has culminated the first point of *Aries* in the northern hemisphere. Therefore from the table of mediation of the first point of *Aries* in the *Connoissance des Tems*, the true hour at night by sea will be easily obtained by viewing the southern cross, and observing at what hour it shall appear upright and perpendicular to the horizon, or rather when the time will permit, by observing with a plumb line held in the hand the very moment when the stars ζ in the foot, and α in the head of the *southern cross*, shall appear equally distant from the perpendicular; the latter on the east side, and the former on the west. For at the point of time when this position of the line shall happen, there will scarce be an error of 1′ the true hour, if you add 15′ to the hour of mediation of the

the firſt point of *Aries*, which will be determined by the abovementioned table, the difference of the meridians of the calculator and obſerver being amended.

The ſtar ζ therefore in the foot of the croſs appears the greateſt of the 4, becauſe when ſeen by the naked eye it unites with another ſmall one, which comes to the meridian 4″ or 5″ after it, and when obſerved by a teleſcope is 1′ 31″ more to the S; the diſtance being meaſured by a micrometer.

The following or eaſtern foot of the *centaur* α, a ſtar alſo of the firſt magnitude, which ſeems to equal or perhaps exceed *capella* in brightneſs and magnitude, is alſo double, and conſiſts of two ſtars, the ſmaller of which is ſcarce diſcovered to emerge from the greater by a good teleſcope of 3 feet. The latter is alſo more northern, and the other a little more ſouthern.

Feuilleé, who obſerved them both with a teleſcope of 16 feet, determines the greater to be of the third and the leſs of the fourth magnitude; which I have not been able to confirm by my own obſervations. But the ſame author erroneouſly calls the foot of the *centaur*, in which theſe united ſtars appear, the northern. The declination of the ſame ſtar was obſerved by *Feuilleé*, *Feb.* 26. 1710. in the city of *Conception* in *Chili*, and was determined by him to be greater than the declination of the other foot by 39′.

III. The chief thing required in the preſent calculation, is to meaſure arches of parallel circles in a ſphere, by degrees and minutes of a great circle. It is paſt all controverſy, that the circumferences of circles are in a *ratio* of their diameters and ſemidiameters. Let the ſemidiameter of a great circle be the whole ſine, and the ſemidiameter of a parallel circle the coſine of declination: it will be eaſy to determine how many ſeconds of the great circle are contained in a degree of the parallel circle, its declination being determined. For as *radius* to the number of ſeconds of one degree in the great circle, ſo 3600, or the coſine of declination, to the number of ſeconds contained in one degree of the parallel circle. On repeating the calculation, we have found, that the arcs of one degree of parallel circles, proceeding from one degree of declination to 29, are equal to the following numbers:

A new method of calculating Eclipſes *of the* Sun; *or any* Occultations *of the* Stars *by the* Moon; *by* Chriſtian Lewis Gerſten, *F. R. S. and Prof. Math.* Gieſen. No. 473 p. 22. May, &c. 1744. *Preſented* May 10. 1744.

Introduction.

Sect. 1.

Deg. of Decl.	Arcs of par. Circ.			Deg. of Decl.	Arcs of par. Circ.			Deg. of Decl.	Arcs of par. Circ.		
	′	″	‴		′	″	‴		′	″	‴
1	59.	59.	27.	11	58.	53.	51.	21	56.	0	53
2	59.	57.	48.	12	58.	41.	19	22	55.	37.	51.
3	59.	55.	3.	13	58.	27.	43	23	55.	13.	49.
4	59.	51.	13.	14	58.	13.	3.	24	54.	48.	45.
5	59.	46.	18	15	57.	57.	19.	25	54.	22.	42.
6	59.	40.	16.	16	57.	40.	32.	26	53.	55.	39.
7	59.	33.	9.	17	57.	22.	41.	27	53.	27.	37.
8	59.	24.	57	18	57.	3.	48.	28	52.	58.	36.
9	59.	15.	40.	19	56.	43.	51.	29	52.	28.	37.
10	59.	5.	18.	20	56.	22.	53.				

From

From these, by simple addition, and cutting off the fourths, we composed a table of the reduction of parallel arcs to minutes, seconds, &c. of a great circle, to every degree of declination from 1 to 29: by the help of which, we may reduce any arcs in parallel circles less than one degree, to minutes and seconds of a great circle, the intermediate declination of which, and their values also are found without much difficulty by the help of an additional table. We have retained the thirds in the table, that when they amount to above 50, a second may be put in their room. By way of example we shall give a part of the table, namely of a parallel circle, the declination of which is 18 degrees.

Arcs of par. Cir.	Parts of a great Circle.			Arcs of par. Cir.	Parts of a great Circle.			Arcs of par. Cir.	Parts of a great Circle.		
′	′	″	‴	′	′	″	‴	′	′	″	‴
″	″	‴	⁗	″	″	‴	⁗	″	″	‴	⁗
1	0	57	3	21	19	58	19	41	38	59	35
2	1	54	7	22	20	55	23	42	39	56	39
3	2	51	11	23	21	52	27	43	40	53	43
4	3	58	15	24	22	49	31	44	41	50	47
5	4	45	19	25	23	46	35	45	42	47	51
6	5	42	22	26	24	43	38	46	43	44	54
7	6	39	26	27	25	40	42	47	44	41	58
8	7	36	30	28	26	37	46	48	45	39	2
9	8	33	34	29	27	34	50	49	46	36	6
10	9	30	38	30	28	31	54	50	47	33	10
11	10	27	41	31	29	28	57	51	48	30	13
12	11	24	45	32	30	26	1	52	42	27	17
13	12	21	49	33	31	23	5	53	50	24	21
14	13	18	53	34	32	20	9	54	51	21	25
15	14	15	57	35	33	17	13	55	52	18	29
16	15	13	0	36	34	14	16	56	53	15	32
17	16	10	4	37	35	11	20	57	54	12	36
18	17	7	8	38	36	8	24	58	55	9	40
19	18	4	12	39	37	5	28	59	56	6	44
20	19	1	16	40	38	2	32	60	57	3	48

Example. Let $53' \ 47''$ of this parallel circle be converted into parts of a great circle: - - $53' = 50' \ 24'' \ 21'''$
$45'' = 42 \ 47$

The Sum 51 7 will be the value sought.

Sect. 2. Small portions of circles parallel to the equator, when they may be safely taken for right ones, are cut by circles of declinations at right angles. Wherefore a small spherical triangle, one side of which is a portion of a circle of declination, another a portion of a parallel circle, may be be taken for a plain rectangular triangle, and its hypothenuſe may be ſafely determined by the Pythagoric theorem, or other rules of plain Trigonometry. But when this hypothenuſe is the diagonal of any ſpherical quadrilineal, which is effected by the ſection of two circles of declination, by two parallel to the equator, the greater of the parallel arcs, and more remote from the pole, is to be choſen for the baſe of the rectangular triangle, when the queſtion is to find the hypothenuſe.

Tables of the parallaxes of the altitude of the moon are conſtructed two ways; according to *Streete*'s 12th precept, prefixed to the Caroline tables, and by his 13th precept alſo. For the diſtance of the moon from the earth, the *ratio* of this diſtance to the ſemidiameter of the earth, which is immediately known by the horizontal parallax, is ſufficient. The firſt way determines the parallaxes to the altitudes ſeen above the ſenſible horizon. For the eclipſes of the ſun, and appulſes of the moon to the ſtars, the firſt way is to be choſen, and not the latter, which would introduce very great errors into our calculation. As I judged an accurate table of the altitude of parallaxes to be a thing of the greateſt moment, I conſtructed a new one for my own uſe to the altitude of 70°. with which however I afterwards found the Lanſbergian table, *p.* 48, *& ſeq.* to agree well enough. But that which is extant in the Ludovician tables, N°. XXV, regards the altitudes ſeen, but not the true, and therefore is not fit for theſe uſes without reduction. Let it be obſerved, that the parallaxes of the ſame true altitude, but of different diſtances of the moon from the earth, are proportional to the diſtances themſelves, and conſequently to the horizontal parallaxes. Sect. 3.

The following tables exhibit the parallaxes of altitude, both according to our table, and that of *Lanſbergius*; and the numbers being either augmented or diminiſhed in proportion to the other horizontal parallaxes are ſufficient for any caſes whatſoever.

True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.		True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.		True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.	
1	60	0	59	59	17	57	41	57	41	33	50	48	50	48
2	59	59	59	59	18	57	23	57	23	34	50	14	50	14
3	59	58	59	57	19	57	4	57	3	35	49	39	49	40
4	59	56	59	54	20	56	44	56	43	36	49	3	49	4
5	59	52	59	50	21	56	23	56	22	37	48	26	48	27
6	59	47	59	46	22	56	0	56	0	38	47	48	47	49
7	59	41	59	40	23	55	37	55	36	39	47	9	47	10
8	59	34	59	33	24	55	12	55	11	40	46	30	46	31
9	59	26	59	24	25	54	47	54	46	41	45	49	45	51
10	59	17	59	14	26	54	21	54	20	42	45	7	45	9
11	59	6	59	4	27	53	54	53	53	43	44	25	44	26
12	58	55	58	53	28	53	25	53	25	44	43	42	43	42
13	58	42	58	41	29	52	56	52	56	45	42	58	42	58
14	58	28	58	28	30	52	26	52	25	46	42	13	42	13
15	58	14	58	14	31	51	54	51	53	47	41	28	41	27
16	57	58	57	58	32	51	22	51	21	48	40	41	40	37

True Alt.	Parall. Alt. our Table.		Parall. Lansberg.		True Alt.	Parall. Alt. our Table.		Parall. Lansberg.		True Alt.	Parall. Alt. our Table.		Parall. Lansberg.	
49	39	54	39	54	57	33	10	33	10	64	26	44	26	44
50	39	6	39	7	58	32	17	32	16	65	25	47	25	47
51	38	17	38	18	59	31	23	31	22	66	24	49	24	49
52	37	28	37	28	60	30	28	30	28	67	23	50	23	50
53	36	38	36	37	61	29	33	29	33	68	22	51	22	51
54	35	47	35	46	62	28	37	28	37	69	21	52	21	52
55	34	55	34	55	63	27	41	27	41	70	20	52	20	52
56.	34	3	34	3										

Sect 4.

The longitude and latitude of a ſtar being given, its right aſcenſion and declination is given by trigonometrical rules. But as that requires a tireſome analyſis of triangles, it is better to make uſe of tables conſtructed on purpoſe. We have in *Flamſted*'s *Hiſt. Cœleſtis*, two of *Abraham Sharp*'s, by which there is a converſion made not only from right aſcenſion and declination into longitude and latitude, but alſo from longitude and latitude into right aſcenſion and declination. Thoſe which are laſt in order *, lead the ſhorteſt way of all, and therefore we have hitherto made uſe of them in this our calculation.

Precepts of the calculation.

1. When it is known by the uſual methods that there will be an eclipſe of the ſun, let the time of conjuction, the longitude and latitude of the moon, the true horary motion thereof, the parallax, and the horizontal diameter, and the horary motion of the ſun and its diameter, be found by theoretical tables.

2. By the help of tables, from the given longitude and latitude, let the right aſcenſion and declination of the ſun and moon be determined.

3. The mean time being converted into apparent, if the point of conjuction happens before noon; then an hour beforehand find out by the horary motion reduced to the ecliptic, the longitudes of the ſun and moon, the latitude of the moon, and the right aſcenſions and declinations of each of the points. If it happens after noon, then the ſame muſt be done an hour after the conjuction.

4. Let the time of conjunction, and that alſo one hour diminiſhed be ſubtracted from 24 hours, when that happens, that there may be an interval of time from the moment of conjunction, or from an hour before the conjunction to noon. In the afternoon hours the time itſelf gives the interval.

5. Let the diſcovered intervals of time be converted into degrees and minutes of the equator; and thus we have the angles of the circle of declination paſſing through the center of the ſun with the meridian of the place.

6. The right aſcenſion of the moon may at any time be either greater or leſs than the right aſcenſion of the ſun. In the morning hours, if it is leſs, then the difference between the right aſcenſions of the ſun and moon is to be ſubtracted from the angle of the circle of declination found in the pre-

* Page 34 and 74, Tom. III.

preceding number: if it is greater, then the difference must be added to the same angle, and then we have the angle of the circle of declination passing through the centre of the moon with the meridian of the place. In the afternoon hours the contrary is to be done.

7. From the angles found by the preceding number, the declinations of the sun and moon by the second number, and the latitude of the place by the rules of spherical Trigonometry, the true altitudes of the sun and moon in both cases may be computed; and then also,

8. The angles of the circles of declination, passing through the centre of the moon in both cases with the vertical circles. The seconds are slighted in this and the preceding number.

9. The true altitudes of the moon being found by number 7, it's horizontal parallax by number 1, the parallaxes of the altitude of the moon are found by the tables of parallaxes of altitude. As we may with *Flamsted* allot to the sun a horizontal parallax of 10″, the horizontal parallax of the moon must first be diminished by this quantity.

10. As *radius* to number of seconds contained in parallax of altitude found by the preceding number, so sine of the angle found in num. 8. to the fourth proportional number, shewn by the calculus, I call it the *parallax of the right ascension in a parallel circle.*

11. To proceed, as *radius* to the same number of seconds contained in the parallax of altitude; so the co-sine of the angle found in num. 8. to the fourth proportional, which is the *parallax of the declination of the moon.* In both cases, that is, at the very time of conjunction, and an hour before or after this computation is to be made.

12. Let the right ascensions of the sun and moon in both cases be disposed according to the natural order of the numbers. Let the difference between the right ascensions of the sun be added to the first right ascension of the moon, and we shall have the first right ascension of the sun; there will then remain two right ascensions of the moon, and one of the sun.

13. The declinations of the sun either increase or decrease. In the first case, let the difference of them be added to that declination of the moon, which agrees with the least right ascension. In the other, let it be subtracted, and the mutual distance of the luminaries will be as if the sun without moving, looked upon the progressive moon for the entire space of an hour.

14. Let each right ascension be subtracted, the least from the biggest, and let the differences be carefully noted.

15. Let the parallaxes of declination be subtracted from the declinations of the moon, if they are northern; but added if they are southern. Thus are found the visible declinations of the moon.

16. Let the differences found in num. 14. which are now conceived to be in a parallel circle, be reduced by the table of reduction *, to minutes

* Introd. §. 1.

nutes and seconds of a great circle. The declination of the parallel is the same with the visible declination of the moon or sun. This it quite to be abstracted from the number and distance of the points of right ascension from the beginning of *Aries*: for that is not the concern at present, but only the position and distance of the luminaries from each other.

17. If the moon comes on before noon, then let the parallaxes of right ascension found in the parallel circle, num. 10. be added to the competent places of the moon. But if it happens after noon, subtract instead of adding. When this is done, the positions of the luminaries are determined, and their visible places, at the time of the true conjunction, and an hour before and after it, whence what follow may be made out with little or no difficulty. For,

18. In every case from what has been found arises a rectangular triangle, the base of which is the distances of the apparent places of the moon in the parallel circle; the *cathetus* the difference of it's visible declinations; the hypothenuse gives the orbit seen; and the position of the sun, whether it falls within or without the triangle, will be also sufficiently determined. The triangle itself never arises to such a magnitude, as to hinder it's being taken for a plain and rectilineal one. Hence by a most easy and simple construction, with the help of a pair of compasses and a scale, may be determined the least distance of the centres and points in the orbit, the greatest darkness and the end so exactly, if a proper scale is made use of, as hardly to err above a second. But this, and all the rest may be performed by the rules of plain Trigonometry.

19. When the sum of the apparent semidiameters of the sun and moon falls without the bounds of the hypothenuse of this triangle, then it must be continued till it meets; and the rest must be performed after the usual manner, to obtain the time of the beginning and end of the eclipse. But then, when the points of meeting are too far distant from the points of the triangle already determined, the *calculus* will want correction, if the time of the beginning and end is exactly required. For the apparent path of the moon in a right line, and also the equable motion seen, of which neither is strictly true, tho' the way seen for the space of an hour generally diverges so little from rectitude in eclipses, that it may be taken for a right line without any conspicuous error. But the same cannot be said of the equality of swiftness. There must therefore be made a *calculus* of correction, which will be explained better by an example than by rules.

Example. *May* 12, 1706. there happened an eclipse of the sun. The quantity, beginning, greatest darkness, and end, are sought to the longitude and latitude of the observatory of *Paris*. According to the *Ludovician* tables, a conjunction of the sun and moon happened *May* 11, $21^h. 49'. 13''.$ mean time. To this time, according to the same tables,

1. The

		°	′	″
1. The true place of ☉ and ☽ in the ecliptick	—	51	6	48
Longitude of ☽ in the orbit	— — —	51	8	22
Place of ☊	— — —	44	14	59
Argument of latitude	— — —	6	53	23
N. Latitude of ☽	— — —		36	7
Horary motion of ☉	— — —		2	25
Semidiameter of ☉	— — —		15	54
Horary motion of ☽	— — —		37	13
Horary motion of ☽ reduced to the ecliptick	—		37	5
Horizontal semidiameter of ☽	— — —		16	31
Horizontal parallax of ☽	— — —		60	29
According to *Sharp*'s table.				
Right ascension of ☉	— — —	48	37	57
N. declination of ☉	— — —	18	3	32
Right ascension of ☽	— — —	47	53	27
N. declination of ☽	— — —	18	25	58

The equation of time, according to the *Ludovician* tables, is 8′ 18″. It must be added to the mean, to make it apparent. Therefore the true time of conjunction is 21^h 57′ 31″.

2. At 1^h before conjunction longit. ☉ = 51° 4′ 23″. Longit. ☽ = 50° 29′ 43″ N. Lat. ☽ = 32′ 53″, consequently the increase of Latitude in the space of one hour = 3′ 15″. Right ascension of ☉ by *Sharp*'s tables = 48° 40′ 24″. Declination of ☉ = 18° 4′ 10″. Right ascension of ☽ = 48° 30′ 21″. Declination of ☽ = 18° 38′ 59″.

3. The interval from the moment of conjunction 21^h 57′ 31″ to noon is = 2^h 2′ 29″, which being converted into arcs of the equator is = 30° 37′ 15″. From 1^h before ☌ to noon there are 3^h 2′ 29″ to which the arc of the equator 45° 37′ 15″ answers. Therefore, according to rule 5, we have the angles of the circles of declination passing thro' the centre of ☉, with the meridian of the place in both cases.

4. The R. asc. of ☉ precedes the R. asc. of ☽ in these two cases: therefore by prec. 6. the differences are to be substracted from the angles found; namely, in ☌ the difference of the R. asc. of ☽ from the R. asc. of ☉ is 10′ 3″. One hour before ☌ the difference is = 43′ 38″. Therefore when these arcs are subducted, there remains for the angle of the circle of declination passing thro' the centre of ☽ in ☌, 30° 27′ 12″, and 1^h before ☌, 44° 53′ 37″.

5. From these angles, the elevation of the pole of the observatory at *Paris* = 48° 50′, and the declinations of ☽, follow the altitudes of ☽. Particularly in ☌, alt. ☽ = 51° 5′. 1^h before ☌ alt. ☽ = 42° 52′. The angles also of the circles of declination with the vertical ones at ☌, 32° 4′ at 1^h before ☌, 39° 19′.

6. According to our table *, to the horizontal parallax 60′ 29″, parall.

* Introd. §. 3.

alt.

alt. ☽ in ☌ = 38′. 31″ not being ſubtracted from the horizontal parall. of ☉, which in this example we have purpoſely omitted. The parall. of R. aſc. in the parallel circle = 20′ 27″. The parall. of declination is = 32′ 38″ by prec. 10 and 11. But at 1^h before ☌, the parall. of altitude = 44′ 53″, parall. R. aſc. in parall. circ. = 28′ 26″, parall. declin. = 34′ 43″.

7. Now follows, by prec. 12, the diſpoſition and ſubtraction of R. aſcenſions and declinations competent to R. aſcenſions.

		R. Aſc. ° ′ ″	Comp. Decl. ° ′ ″
At 1^h before ☌ —	☽	47 53 35	18 26 0
At the very ☌ —	☽	48 30 21	18 38 59
At 1^h before ☌ —	☉	48 37 57	18 3 32
At the very ☌ —	☉	48 40 24	18 4 10
Diff. between R. Aſc. ☉		2 27.	Between declin. ☉ 38

		R. Aſc. ° ′ ″	Declin. ° ′ ″
At 1^h before ☌ —	☽	47 56 2	18 26 38
In the very ☌ —	☽	48 30 21	18 38 59
Of ☉ unmoved		48 40 24	18 4 10
	Diff. *a*	34 19	Parall. Declin. { 34 43
	Diff. *b*	44 22	{ 32 38

Declin. ſeen { ☽ 17 51 55
☽ 18 6 21
☉ 18 4 10

8. According to prec. 16, the difference *a* reduced to parts of a great circle is = 32′ 39″; the difference *b* = 42′ 13″. The firſt is the diſtance of the places of the moon in both caſes, the latter the diſtance of the ſun unmoved, from the firſt place of the moon in a parallel circle, the declination of which is 17° 51′ 55″; or, which is not very different, 17° 52′.

9. The parallax of R. aſc. in a parallel circle in ☌ = 20′ 27″ (numb. 6.) being added, by prec. 17. to the ſecond place of the moon, 32′ 39″ makes 53′ 6″. Therefore the firſt place of the moon = parall. R. aſc. at 1^h before ☌. Therefore, in the parallel circle, the places of the luminaries ſeen are the following.

		′ ″
At 1^h before ☌	☽	28 26 = A
☉ unmoved		42 13 = B
In the very ☌	☽	53 6 = C

Diff. between A and B = 13 47
A and C = 24 40

If

If the last declination is subtracted from the declinations seen, in this case ☽ 17° 51′ 55″ remains for ☉ 12′ 15″; for ☽ in ☌ 14′ 26″.

10. Now let *b c* be a portion of a circle parallel to the declination 17° 51′ 55″, and therein the point *c* the centre of ☽ at 1^h before ☌, *d* the place of ☉, *b* the place of ☽ in ☌; then *d c* = 13′ 47″; *b c* = 24′ 40″. From the points *d* and *b*, erect the perpendiculars *a f* and *a b*; of which the former = 12′ 15″, the difference of the last declination; the latter, = 14′ 26″ of the greatest, *f* will be the centre of the sun unmoved, *a* the centre of the moon in the very ☌, the right line *a c* the visible path of the moon at the distance of one hour. Fig 5.

11. From the point *f* to *ac* a perpendicular *gf* being let fall determines the quantity of the eclipse, and the point *g* the greatest darkness. Moreover, if we take with a pair of compasses, the space *nf* and *fm* = to the sum of the apparent semidiameters of ☉ and ☽, and if from the same point *f* be cut the hypothenuse produced *mn* of the triangle *abc*, we shall have the determination of the points *n* and *m*, in which the beginning and end of the eclipse happens.

12. By the trigonometrical calculation we have *cg* = 18′ 4″; *gf* = 3′ 37″; *ac* = 28′ 34″. Now if we say as *ac* to *gc*, so the time by *ac* = 1^h to the time by *gc*, there results 37′ 37″; this time being added to 20^h 57′ 31″ (1^h before ☌) makes the point of greatest obscuration 21^h 35′ 26″.

13. The horizontal semidiameter of ☽ is = 10′ 31″ (num. 1.); but corrected by *de la Hire*, tab. 24. is = 16′ 43″. The semidiameter of ☉ is = 15′ 54″. The sum of the semidiameters of ☉ and ☽ = 32′ 37″; *gf* being subtracted from this sum, there remains the deficient part = 29′ 0″, this being reduced to ecliptical digits gives the quantity of the eclipse 10° 56′.

14. To determine the beginning and the end, from *gf*, *fn* and *fm*, are to be sought *gn* and *gm*. I make *fn* equal to the sum of the apparent semidiameters (num. 13.) diminished by one or two seconds, but *fm* equal to the same augmented by one or two seconds; and so *fn* = 32′ 35″; *fm* = 32′ 39″. Wherefore *gn* = 32′ 22″; *gm* = 32′ 25″; the time by *gn* = 1^h 7′ 58″; which being subtracted from the point of greatest darkness, shews the beginning of the eclipse, 20^h 27′ 28″; the time by *gm* = 1^h 8′ 5″; which being added to the greatest obscuration gives the end 22^h 43′ 31″.

15. One hour before ☌ = 20^h 57′ 31″; the time of the beginning = 20^h 27′ 28″; therefore the beginning differs from 1^h before ☌ 30′ 3″. To this difference of time answers the motion of ☽ in long. 18′ 34″; increm. lat. ☉ 1′ 37″; motion of ☉ in long. 1′ 12″: these being subducted from long. and lat. at 1^h before ☌, there is left at the time of the beginning, long. ☉ = 51° 3′ 11″; long. ☽ 50° 11′ 9″; lat. ☽ 31′ 16″ R. asc. ☉ 48° 36′ 44″; decl. ☉ 18° 3′ 13″; R. asc. ☽ 47° 35′ 10″; declin. ☽ 18° 19′ 28″: diff. between R. asc. ☉ and ☽ = 1° 1′ Correction of the beginning.

34″;

34″; the interval of time between the moment of beginning and noon $= 3^h\ 32'\ 32''$; which being converted into arches of the equator gives $53^\circ\ 8'\ 0''$. Now because R. asc. ☽ is less than R. asc. ☉, the difference of the R. asc. of ☉ and ☽ to be subtracted from this arc, remains $52^\circ\ 6'\ 26''$, namely the angle of the circle of declination passing thro' the centre of ☽ with the meridian of the place. The altitude of ☽ $= 38^\circ\ 20'$. The angle of the circle of declination with the vertical $= 41^\circ\ 28'$. Parallax of altitude $= 47'\ 58''$. Parall. R. asc. in parallel circ. $= 31'\ 45''$. Parall. declin. $= 35'\ 56''$.

16. The disposition and reduction of the R. ascensions according to prec. 12. now becomes thus:

		R. Asc.		Comp. Declin.
		° ′ ″		° ′ ″
At $20^h\ 27'\ 28''$	☽	47 35 10	— — — —	18 19 28
1 before ☌	☽	47 53 35	— — — —	18 26 0
20 27 28	☉	48 36 44	— — — —	18 3 13
1 before ☌	☉	48 37 57	— — — —	18 3 32
Diff. of R. asc.	☉	1 13	Diff. of Dec. ☉	19
	☽	47 36 23	— — — —	18 19 47
	☽	47 53 35	— — — —	18 26 0
Of the unmoved	☉	48 37 57	— — — —	18 3 32
Diff. *a*		17 12	Parall. of Declin.	35 56
Diff. *b*		1 1 34		34 43
Diff. *a* reduced		16 24	Declin. seen ☽	17 43 51
Diff. *b* reduced		58 39	☽	17 51 17
			☽	18 3 32
Parall. of R. Asc. at 1^h before ☌		28 26		
at $20^h\ 27'\ 28''$		31 45	Diff. *c*	7 26
			Diff. *d*	19 41
	☽	31 45		
	☽	54 50		
	☉	58 39		
Diff. *e*		13 5		
Diff. *f*		26 54		

Fig. 4. 17. From the differences *e*, *f*, *c*, *d*, is constructed the type and correction after the following method; let the diff. $e = 13'\ 5''$ be $= ac$; and diff. $f = 26'\ 54'' = ad$; let the perpendicular bc be $=$ diff. c or $7'\ 26''$; let the perpendicular fd be $= 19'\ 41'' =$ diff. d; and $20^h\ 27'\ 28''$ will be the centre of ☽ in a; but $4'$ before ☌ in b; the centre of the sun unmoved in f. The seen orbit of the moon is determined by the points a and b; because it passes thro' them. But if fm is equal to the sum of the

the apparent diameters = 32′ 35″, it cuts the part *ma* from the hypothenuse *b a*, which being converted into time, gives the quantity of correction.

18. If the thing is to be performed by calculation, let *b a* be continued, and from *f* let fall the perpendicular *fg*. In the present case $ab =$ 15′ 2″, $ae =$ 30′ 55″, $ge =$ 2′ 10″: therefore $ga =$ 33′ 5″, $gf =$ 3′ 50″, $fm =$ 32′ 35″; therefore $gm =$ 32′ 21″; and $ga - gm = ma =$ 44″; which quantity being converted into time, is = 1′ 27″. But when ☽ is moved from *a* towards *b*, and the centre of the moon is placed in *a* 20° 27′ 28″, it is plain that this time must be added to the time of the beginning found above, that the beginning of the eclipse may be found true and correct; 20ʰ 28′ 55″.

19 To shew the exactness of this calculation, let us investigate the distance of the centres of ☉ and ☽ to this corrected time of the beginning. For if these are equal to the sum of the apparent semidiameters, it is necessarily the true point of the beginning; if otherwise, it is false. The time that passes between this point of the corrected beginning and the time of ☌ is = 1ʰ 48′ 36″. With this agrees the motion of ☽ in the ecliptick 54′ 46″ increment. lat. ☽ 4′ 48″; motion of ☉ in longitude 3′ 34″; therefore at the time of corrected beginning, long. ☽ 50° 12′ 12″, N. lat. ☽ 31′ 19″; long. ☉ 51° 3′ 14″; R. asc. ☉ = 48° 30′ 47″; declin. ☉ = 18° 3′ 14″; R. asc. ☽ 47° 36′ 4″; declin. ☽ 18° 19′ 46″; diff. between R. asc. ☉ and ☽ 1° 0′ 43″; diff. between time of corrected beginning and noon 3ʰ 31′ 6″; arc of the equator agreeing with this time = 52° 46′ 30″ = ang. circ. declin. passing thro' the centre of ☉ with the meridian of the place. The difference between R. asc. ☉ and ☽ being subtracted from this, there remains for ang. circ. declin. passing thro' the centre of ☽ with the meridian = 51° 45′ 47″. Corresponding alt. ☽ = 38′ 33″: ang. circ. declin. with the vertical = 41′ 11″; parall. alt. = 47′ 50″, parall. declin. = 36′ 0″; parall. R. asc. in parall. circle = 31′ 29″; seen declin. ☽ = 17° 43′ 46″; diff. between seen declin. ☽ and declin. ☉ = 19′ 28″; diff. between R. asc. ☉ and R. asc. ☽, reduced to parts of a great circle, allowing the declin. of the parall. 17° 44′ = 57′ 34″; parallax R. asc. = 31′ 29″: therefore the distance of the places of ☉ and ☽ in this parallel circle = 26′ 5″. If therefore from 26′ 5″ as a base, and from 19′ 28″ as a *cathetus*, a rectangular triangle be constructed, the hypothenuse of this triangle will be the distance of the centres of ☉ and ☽; but 26′ 5″ = 1565″; the square of which is 2449225: and 19′ 28″ = 1168″, the square of which is 1364224; and the sum of the squares = 3813449, the square root of which is = 1953″, 2″ only less than the sum of the apparent semidiameters. Proof of the correction.

20. The point of this, as determined above, numb. 14, is 22ʰ 43′ 31″. Time of ☌ 21ʰ 57′ 31″; difference, 46′ 0″. To this difference, the motion of ☽ in longit. is 28′ 25″; increment of lat. = 2′ 19″; motion of ☉ in longit. = 1′ 51″; wherefore to 22ʰ 43′ 31″ longit. ☽ = 51° 35′ 13″; latit. ☽ = 38′ 36″; longit. ☉ = 51° 8′ 39″; R. asc. Correction for the end.

☉ = 48° 42′ 17″; declin. ☉ = 18° 4′ 39″; R. asc. ☽ = 48° 58′ 38″; declin. ☽ = 18° 48′ 49″.

21. The diff. of time between the end of the eclipse and noon, is 1^h 16′ 29″; which being converted into an arc of the equator, is = 19° 7′ 15″. Diff. between R. asc. ☉ and ☽ = 16′ 21″; R. asc. ☽ precedes R. asc. ☉; therefore this diff. is to be added, to make 19° 23′ 36″, the angle of the circle of declination passing thro' the centre of ☽ with the meridian. This angle with lat. of the Observatory of *Paris* and declin. ☽ produces alt. ☽ = 56° 8′; and the angle of the circle of declin. with the vertical = 23° 4′. Hence follows parallax of alt. = 34′ 12″; parall. declin. 31′ 27″; and parall. R. asc. in a parallel circle = 13′ 24″.

22. The reduction therefore and disposition of R. ascensions and declinations is as follows.

		R. Asc.		Comp. Decl.
		° ′ ″		° ′ ″
	In 6 ☽	48 30 21		18 38 59
h ′ ″	In 6 ☉	48 40 24		18 4 10
At 22 43 31	☉	48 42 17		18 4 39
At 22 43 31	☽	48 58 38		18 48 49
Diff between R. asc. ☉		1 53	Diff. between decl. ☉	29
	In 6 ☽	48 32 14		18 39 28
	Unmoved ☉	48 42 14		18 4 39
h ′ ″				
At 22 42 31	☽	48 58 39		18 48 39
	Diff. *a*	10 3	Parall.	32 38
	Diff *b*	26 24	declin.	31 27
Diff. *a* reduced		9 33	Decl. seen	18 6 50 ☽
Diff. *b* reduced		25 5		18 4 39 ☉
				18 17 12 ☽

23. Diff. *a* is the distance of ☉ unmoved from the first place of ☽, and diff. *b* the distance of the 2d place of ☽ from the 1st in the parallel circle, the decl. of which is 18° 7′. By the parallaxes of R. asc. the two places of ☽ are now changed into the following, and so by adding the parallaxes, the distances will be of the

	′ ″
☉ unmoved =	9 33
☽ in 6 =	20 27
☽ in the end =	38 29

Now if from these numbers we subtract the least, 9′ 33″, there is left for the distance of the place of ☽ in 6 from ☉ unmoved, 10′ 54″; for the distance of ☽ in the end of the eclipse from ☉ 28′ 56″; the differences of the declinations seen, from the least seen, are 2′ 11″, and 12′ 33″.

24. Let

24 Let $q\,f$ be the portion of a circle parallel to the declination 18° 7′; Fig. 5.
therein let f be the centre of ☉ unmoved; r the place of ☽ in ☌; q the place of ☽ in the end of the eclipse: wherefore $r\,f = 10' \, 54''$; $q\,f = 28' \, 56''$. At the points r and q raise the perpendiculars $a\,r$ and $q\,v$; so that $a\,r$ may be $= 2' \, 11''$, and $q\,v = 12' \, 33''$. The right line $m\,v\,a\,g$ being drawn thro' the points v and a, will shew the orbit of ☽ seen. But if the aperture of the circle is equal to the sum of the apparent semidiameters, in this case $= 32' \, 39''$, this will cut off from f a portion of the orbit $m\,v$, which being converted into time, and added to the time of the end found above, gives the end corrected.

25. If this is to be done by numbers alone, first, let the perpendicular $a\,r$ be subtracted from the perpendicular $v\,a$, to obtain $v\,z$. Let the orbit $v\,a$ be produced, and from f let fall the perpendicular $f\,g$; hence arise 3 similar triangles; $a\,z\,v$, $a\,r\,n$, and $f\,n\,g$. On making the calculation, there comes out for $v\,a$, $20' \, 48''$; for $a\,n$ $4' \, 22''$; for $n\,g$ $6' \, 10''$: consequently $v\,g = 31' \, 20''$, and $g\,f = 3' \, 32''$. But as $m\,f$ is $= 32' \, 39''$, $m\,g$ will be $= 32' \, 27''$, therefore $m\,v = m\,g - v\,g = 1' \, 7''$: which quantity being changed into time, is $= 2' \, 28''$: this time being added to the time of the end found above $22^h \, 43' \, 31''$, gives at last the end of the eclipse corrected $22^h \, 45' \, 59''$.

I made choice of this example, because it is the same with that by Note.
which *de la Hire* illustrated the precepts of his calculation: it will therefore not be amiss to shew it's agreement with the present. In *de la Hire*'s calculation, the point of conjunction is supposed to be according to true time $21^h \, 57' \, 15''$; which however is not exact: for according to the *Ludovician* tables, it happens at $21^h \, 57' \, 31''$; as we settled above. After this small error is corrected, the point of greatest obscuration, according to *de la Hire*'s calculation, agrees with ours even in seconds, $21^h \, 35' \, 26''$; but there is some little difference in the beginning, end, and quantity, of the eclipse *. In that calculation, the perpendicular $L\,T$ produces at the true time of conjunction 211; and so the quantity of the eclipse is = 10 dig. 49′. The beginning happens at $20^h \, 27' \, 29''$, the end, $22^h \, 43' \, 23''$. By *de la Hire*'s precept, that beginning needs no correction; which however is true, if an error of 1′ or 1 $\frac{1}{2}$ may be slighted. But if not, as the thing requires, and the proof of my correction sufficiently shews, the labour of correcting is also to be undertaken in *de la Hire*'s calculation. In mine, the beginning found at first would agree exactly enough; but because of different altitudes of the moon in the end and beginning, I have assumed diverse apparent semidiameters, which *de la Hire* did not do; and therefore to make all things equal, let the apparent semidiameter of ☽ be set at $16' \, 43''$ in the beginning and end; in which case the beginning of my calculation not corrected will be brought back to $20^h \, 27' \, 23''$, the end, to $22^h \, 43' \, 29''$; therefore my beginning is 6″ before *de la Hire*'s; and the end follows it at the same distance; and the quantity of the eclipse, as we have determined above, exceeds *de la Hire*'s 7′.

* *Vid.* Tab. *Ludovic.* Edit. *Paris.* 1727. p. 48. *in usu Tabularum.*

 When

When the apparent orbits of ☽, or rather the feigned ones in the present calculation, and in that of *de la Hire*, are not right but curves, in this difference that the convexity in *de la Hire*'s may be objected to the point L *, and in the present the concavity to the point f †, it is evident that the perpendicular $L\,T$, on the length of which the quantity of the eclipse depends, is greater than it ought to be in *de la Hire*'s calculation; as in mine the same perpendicular which is designed by $f\,g$ is less than it ought to be: therefore if the greatest exactness was to be used, the quantity of the eclipse ought to be reckoned between them both.

The Sun's Eclipse of July 14, 1748. observed at Marlborough House, *with the 12 foot refracting Telescope, fix'd as a finder to the tube of the great 12 foot reflector; by* John Bevis *M. D.* N°. 489. p. 521. Oct. *&c.* 1748. *Read* Nov. 10. 1748.

Apparent Time.

	d	h	′	″	
IV. 1. *July*	14.	9.	3.	50.	The beginning, which perhaps might be 2″ or 3″ sooner.
	14.	9.	39.	42.	The first little spot in the western cluster, quite covered.
			52.	00.	The biggest of that cluster quite cover'd, yet somewhat doubtful for flying clouds.
		10.	12.	08.	The middle one of three considerable spots towards the eastern limb half cover'd.
					The end could not be precisely observed for flying clouds; at 12. 09. 15. it was not quite over; but at 12. 09. 35. the sun was clear, and nothing of the eclipse left.

N. B. *The wind was so boisterous, that no phases could be measured with a Micrometer.*

—— by Mr Mark Day, *dated* Luffwick *near* Thrapston, Northamptonshire, Oct. 21. 1748. *Ibid.* p. 523.

2. The beginning 9ʰ 1′ 0″ *a. m.* The end 0. 5. 25. *p. m.* at 10ʰ 32′ 10″ *a. m.* 10° 18′ were dark, which I take to be the greatest with us. These are apparent times, from a well adjusted clock (by a meridian drawn *June* 10, on a plate of metal), and corrected to the time of observation. Our latitude is 52° 27′ 30″.

—— At the Observatory Royal at Berlin, *by* Augustine Nathanael Grischow, *Memb. of the R. Acad. of Sc. at* Berlin, *&c. ibid.* p. 526.

3. 1748. *July* 25. *N. S.* The beginning of the eclipse was not observed, the sun having been covered with clouds.

	h	′	″	
The *annulus* was completed at	11	52	51	*ante merid.*
—— —— —— broken	11	54	13	
The end of the eclipse	1	25	9	*post. merid.*

The diameter of the sun was 31′ 43″.

* *Vide* Alleg. p. 48. in Tab. *Ludov.* † Fig. 3.

This

This eclipſe was likewiſe obſerved annular at *Francfort* upon the *Oder*, but not ſo exactly as at *Berlin*.

4. Theſe obſervations were made at *Aberdour* caſtle, belonging to the ſaid Earl, whoſe lat. is 56° 4′ N. Mr *le Monnier* having come over from *France* to go to *Scotland*, to obſerve the annular eclipſe of the ſun, *July* 14. 1748. I was deſirous to contribute all that lay in my power to aſſiſt him, and therefore reſolved to go to *Scotland* with the Earl of *Morton*, who was ſo good as to permit us the honour of accompanying him. We arrived at *Edinburgh July* 4. and immediately went to the College, to enquire what preparations were made there, in conſequence of letters we had writ before we left *London*; when Mr *Alex. Monro*, Prof. Anat. informed us, that, upon receipt of ours, he had writ circular letters to all his friends in different parts of the country, to prepare, in the beſt manner they could, for the moſt exact obſervation of this eclipſe.

—— *obſerved by the R. Hon.* James *E. of* Morton, *Mr* le Monnier, *R Aſtron and Memb. of the* R. Acad. *of* Sc. *at* Paris, *and Mr* Ja. Short, *Fellows of the* R. Soc N°. 490 p. 582. Dec. 1748. *Read* Dec. 8. 1748.

We found that the meridian mark, which had been ſettled from obſervations, by the late worthy Mr *Mac Laurin*, was loſt, by the taking down of a chimney, upon which it was fixed; and Mr *Matthew Stewart*, the preſent Profeſſor, having no proper inſtruments, had not as yet re-eſtabliſhed it; which we hoped to do by an inſtrument, which we every day expected from *London*; and Mr *Stewart* having promiſed to make the beſt obſervation he could, we reſolved to ſet out for *Aberdour*, a ſeat of the E. of *Morton*'s, which he readily offered to us, and did us the honour to accompany us thither himſelf, having the ſame deſire and curioſity to do whatever lay in his power to contribute to an exact obſervation.

Aberdour is about 8 miles almoſt N. W. of *Edinburgh*. We choſe this place, as being, by the computations of this eclipſe, at or very near the ſouthern limit of the *annulus*.

In the caſtle of *Aberdour*, 25″ of time weſt of the college of *Edinburgh*, we ſet up a clock, *July* 9. and the weather being cloudy, and our equal-altitude inſtrument and *tranſit* not being yet arrived, we on the 11th made uſe of an equatorial teleſcope of Lord *Morton*'s, to find correſponding altitudes of the ſun, and at the ſame time put up a *gnomon* of 15 feet high.

Being uneaſy that our inſtruments were not come to hand, and reſolving to have a communication with the college of *Edinburgh*, where they had a *tranſit* inſtrument; Lord *Morton* propoſed that two cannon ſhould be fired from the caſtle of *Edinburgh*, one preciſely at 12, and the other at 5 after 12 on the day of the eclipſe; and the different obſervers in different parts of the country to be advertiſed of this, and to mark down the preciſe time of ſeeing the flaſh, or hearing the ſound of the cannon; ſo that, after having made a geographical map of theſe different parts of the country, and having found the exact meridian of one place, we ſhould be enabled to ſettle the times of all the reſt by the difference of meridians found by this map. This was ſettled

and

and agreed to on the 12th, and an express sent over to *Edinburgh* with a letter from Lord *Morton* to the Lord Justice Clerk, to desire this favour of General *Bland*, who very readily granted it.

The 13th being a clear day, we took equal altitudes with the equatorial telescope, and found our clock gained 1′ 46″ in two days, and that the sun passed the meridian at 12^h 7′ 6″ by the clock.

July 14th was an exceeding bad morning both for wind and rain; but about 8^h in the morning, the clouds dispersed, and we had a very clear sun.

In order to observe the eclipse, Lord *Morton* made use of a reflecting telescope, 12 inches focal length, magnifying about 40 times. I made use of a reflecting telescope 4 feet *focus*, magnifying about 120 times; both belonging to Lord *Morton*. Mr *le Monnier* made use of a refracting telescope, about 9 feet *focus*, which he brought with him from *France*, armed with a micrometer, made after the method of Mr *G. Graham*, by the late Mr *Sisson* at *London*. Mr *le Monnier* took his station in the garden, under the window of the room where the clock was placed; Lord *Morton* was in the room next that where the clock stood; and I was at the window next the clock.

Clock. h ′ ″	True Time. h ′ ″	
8 55 0	8 47 5	The eclipse not yet begun. Clouds come on.
8 59 13	8 51 18	Beginning of the eclipse, found by the following chord.
9 0 42	8 52 47	First view of the eclipse, then considerably advanced.
9 2 30	8 54 35	Measured the chord of the part eclipsed; which was found equal to the field of the great reflector.
10 6 10	9 58 12	The illuminated part of the sun, measured by the micrometer, and found = 7′ 37″ ½.
10 45 0	10 37 0	Again measured, and found = 7′ 37″ ½.
		L. *Morton* judged the middle of the eclipse, or nearest approach to an *annulus*, at 10^h 17′ 54″ apparent time.
11 52 43	11 44 40	The same phase or chord observed as at the beginning, and measured both in the telescope, as at first, and by the micrometer, and found = 8′ 25″ of a great circle, as verified by a base after the eclipse was over, which gives the end as exact as the beginning.
11 56 21	11 48 18	End of the eclipse by the preceding chord.

Mr *le Monnier* measur'd with the micrometer the apparent equatorial diameter of the moon, when she was upon the sun; which he found

= 29′

$= 29' \, 47'' \frac{1}{2}$. He measured also the apparent vertical diameter of the sun at noon; which he found $= 31' \, 40''$. The micrometer, with which he measured these diameters, was afterwards verified, by a base of 2570 feet, and two marks, placed at right angles to it's extremity, at the distance of 22 feet from one another.

The flash of the first cannon fired from the castle was seen at $12^h \, 3' \, 4''$ by the clock; and the flash of the second cannon also by the clock at $12^h \, 8' \, 4''$. The eclipse was so nearly annular, that, at the nearest approach, the cusps seemed to want about $\frac{1}{4}$ of the moon's circumference to be joined; yet a brown light was plainly observed, both by L. *Morton* and myself, to proceed or stretch along the circumference of the moon, from each of the cusps, about $\frac{1}{3}$ of the whole distance of the cusps from each cusp; and there remained about $\frac{1}{3}$ of the whole distance of the cusps not enlightned by this brown light; so that we were for some time in suspense whether or not we were to have the eclipse annular with us. I observed, at the extremity of this brown light, which came from the western cusp, a larger quantity of light, than in any other place, which at first surprized me; but afterwards I imagined it must have proceeded from some cavity or valley made by two adjoining mountains on the edge or limb of the moon. I had often formerly observed mountains on the circumference of the moon, more or less every-where round it, but never saw them so plain as during the time of this eclipse; for we had the air exceeding clear, and free of all agitation, notwithstanding it blew a perfect hurricane of wind, which began about the middle of the eclipse; and I remember, in the annular eclipse of the sun in the year 1737, it did the same. The mountainous inequalities on the southern limb of the moon were particularly remarkable; in some parts mountains and valleys alternately; others extended a considerable way along the circumference, and ended almost perpendicularly like a precipice. L. *Morton* was able to see them very easily thro' his small reflector.

A little after the middle of the eclipse, some clouds, that seemed stationary below the sun, appeared tinged on their upper extremities with all the colours of the rainbow.

During the greatest darkness, some people, who were in the garden adjoining to the castle, saw a star to the east of the sun; which, when they afterwards told us, and pointed to the place where they had seen it, we found must have been the planet *Venus*. This star, we were afterwards told, was seen also at *Edinburgh*, and other places, by a great number of people; but I did not hear of any other stars being seen. The darkness was not great, but the sky appeared of a faint languid colour. What is pretty remarkable, is, Mr *le Monnier* assured us, that when he looked at the sun with his naked eyes during the middle of the eclipse, he could observe nothing upon the sun, but saw the sun full, tho' faint in his light. This, I am apt to imagine, may be owing to his being short-sighted. I observed also, about the

the middle of the eclipse, a remarkable large spot of light, of an irregular figure, and of a considerable brightness, about 7′ or 8′ within the limb of the moon next the western cusp. I thought I lost this light several times; but whether this was owing to my shutting my eyes, in order to relieve them, or not, I cannot tell. I am told, that the rev. Mr *Irwin* at *Elgin* observed the same. When I first perceived it, I called to Lord *Moreton*, who was in the next room, but he could not see it.

Before the eclipse began, and during the whole time of the eclipse, the air, as I said before, being exceeding clear, I saw thro' the 4 foot reflector the surface of the sun cover'd with something which I had never observed before; it seemed to be all irregularly overspread with light, and a faint shade, especially towards his equatorial diameter. This appearance was so odd, that it is difficult to describe it, so as to give an adequate idea of what I saw; but if I may be allowed the expression, it seemed as it were, curdled with a bright and more dusky light or colour. This appearance was permanent, and regularly the same; and if in any degree seen before, may have given rise to *faculæ* having been seen in the sun; but to me the whole sun's body seemed to be more or less covered with it. I looked with all the attention possible, to see if I could observe the body or limb of the moon before she touched the sun, and also after she left it, and was intirely off the sun, but could see nothing at all of any such appearance. I mention it to satisfy Mr *de Lisle*, who publickly desired this might be attended to.

The barometer had been falling for several days before the eclipse, and even that morning, when it was at 29.2 inches. But during the eclipse it began to rise.

		h	′		Divisions.
July 11.	at	11^h		in the morning the thermometer stood at -	54
	at	12	0	or noon at - - - - - -	56
	at	4	0	*p. m.* at - - - - - -	60
July 12.	at	11	0	*a. m.* it stood at - - - - -	57
	at	12	0	or noon at - - - - -	58
July 13.	at	8	30	*a. m.* it stood at - - - - -	55½
	at	1	0	*p. m.* at - - - - - -	57½
July 14.	at	8	0	*a. m.* at - - - - - -	56
	at	8	53	at - - - - - - -	57
	at	9	7	at - - - - - - - -	57½
	at	9	20	at - - - - - - - -	57¼
	at	10	8	at - - - - - - - -	57
	at	10	26	at - - - - - - - -	56¼

All these observations of the thermometer were taken when it stood in the shade; and the times are by the clock. Immediately after the middle of the eclipse, the thermometer, when exposed to the sun for the space of 10′ of time, rose only ½ a division.

Thermometer

	Divisions
Thermometer ſtill expoſed to the Sun,	
at $10^h 46' 00''$, ſtood at - - -	- $58\frac{1}{2}$
at 10 51 30 at - - - -	- 62
at 10 57 30 at - - - -	- $63\frac{1}{2}$
at 11 4 00 at - - - -	- 66
at 11 10 00 at - - - -	- 70
at 11 34 00 at - - - -	- $75\frac{1}{4}$
Thermometer replaced in the ſhade after this laſt obſervation,	
at $12^h 54'$ ſtood at - - -	- $60\frac{1}{2}$
at 1 28′ at - - - -	- $61\frac{1}{4}$
at 5 50 at - - - -	- 59
at 7 30 at - - - -	- $58\frac{1}{4}$
July 15. Thermometer at 8^h *a. m.* ſtood at - -	- 56
at 9 at - - -	- 57
at 10 at - - -	- 60

Theſe obſervations were made with a thermometer of *Fahrenheit*'s ſcale, the diviſions of which were very ſenſible. We did not at all perceive or feel any greater degree of cold, during the eclipſe, than we felt before it began.

The weather being very bad at *Edinburgh*, Mr *M. Stewart*, Prof. Math. could make no obſervations of the Eclipſe; he only ſaw the end at $11^h 50' 34''$ true time; and even then the ſun was ſomewhat cloudy: he took however the ſun's *tranſit* over the meridian (as then ſuppoſed) at $12^h 4' 42''$ by his clock, and heard the ſecond cannon fired from the caſtle at $12^h 7' 48''$ by the clock. We afterwards, in a few days, examined his meridian mark with a very exact equal altitude inſtrument by 3 ſeveral correſpondent obſervations; and found his mark $3' 22''$ of time to the W. of the true meridian. The college is about 2500 feet diſtant from the caſtle Eaſtward.

The Rev. Mr *Bryce*, at *Aldiſton*, about 6 miles to the W. of *Edinburgh*, lat. $55° 55'\frac{1}{2}$ N, obſerved with a reflecting teleſcope, 9 inches *focus*,

	h	′	″
The beginning of the eclipſe at - - -	8	52	30
Upper horn or cuſp vertical, at - - -	9	5	0
Hitherto the weſtern cuſp lower than the eaſtern.			
The two cuſps horizontal at - - -	10	13	10
The weſtern cuſp aſcends very faſt at - -	10	14	10
The weſtern cuſp vertical at - - - -	10	16	15
The cuſp which was juſt now vertical, now becomes Eaſt, and about 30° from the zenith to the Eaſt at -	10	17	10
The middle of the eclipſe as near as he could judge at -	10	17	40
The lower cuſp at the nadir, and very ragged and uneven at - - - - -	10	24	45

The

	h ′ ″
The ſame cuſp ſtill in the ſame poſition at - - - - -	10 32 5
The ſame cuſp ſeems to begin to move towards the W. at	10 43 35
The motion of this cuſp ſcarce ſenſible at - - - - - -	10 55 45
The other cuſp middle between the *zenith* and the *nadir* towards the E. at - - - - - - - - - - -	11 0 25
End of the eclipſe, the ſun being quite clear at - - -	11 48 40

I ſhall ſet down the following obſervations of this eclipſe juſt as they came to my hand when in *Scotland*, without making any other remark, than that, from the diſagreement among themſelves, they do not all of them ſeem to have been made with due accuracy and attention; for want, I ſuppoſe, of ſufficient practice in this kind of obſervations.

William Crow, Eſq; at his houſe of *Netherbyres* near *Haymouth*, lat. 55° 51′ N. ſays,

	h ′ ″
The eclipſe began at - - - - - - - - - - - - -	8 55 0
Half of the ſun eclipſed at - - - - - - - - - - -	9 50 0
Middle of the eclipſe, $\frac{5}{6}$ of the ſun's limb cover'd by the moon at	10 25 0
End of the eclipſe at - - - - - - - - - - - -	11 55 0

Mr *John Mair*, at *Air*, lat. 55° 30′ N. ſays, the eclipſe began 8^h 45′; but that, by reaſon of clouds, he could make no other particular obſervation; only that, by a view he had of the ſun ſome little time before the end, he thinks the end of the eclipſe might be about 11^h 48′.

Mr *Mark*, teacher of Math. at *Dundee*, lat. 56° 25′ N. obſerved,

	h ′ ″
The beginning of the annular appearance at - - - - -	10 16 44
End of the annular appearance at - - - - - - - -	10 23 8

He ſays, the beſt obſervations make the *annulus* a ſmall matter narrower on the upper than lower ſide; by which it appears the centre of the eclipſe was to the N. of *Dundee*.

Mr *John Stewart*, Prof. Math. at *Aberdeen*, writes, that by an obſervation made at *Monroſs*, lat. 56° 41′,

	h ′ ″
The annular appearance began at - - - -	10 20 0
Annulus ended at - - - - - -	10 24 30
End of the eclipſe at - - - - - -	11 52 45

And that, by an obſervation made at a place about 18 miles S. W. of *Aberdeen*.

	h	′	″
The eclipſe began at - - - - - - - - - - - - - -	8	52	0
Middle at - - - - - - - - - - - - - - - - - -	10	21	0
End at - - - - - - - - - - - - - - - - - - -	11	52	0

And that at *Aberdeen*, lat. 57° 11′ N.	h	′	″
The eclipſe began at - - - - - - - - - - - - - -	8	55	33
Middle of the eclipſe, and annular appearance, as near as he could judge, at	10	23	3
End of the annular appearance at - - - - - - - - - -	10	24	48

He writes alſo, that he received an account from Mr *Reid*, Miniſter at *New Macchar*, about 7 Miles N. W. of *Aberdeen*, who obſerved

	h	′	″
The beginning of the annular appearance at - - - - - -	10	18	28
And the end of the eclipſe at - - - - - - - - - -	11	49	3

Mr *Stewart* ſays, that, by comparing his obſervation at *Aberdeen* with this of Mr *Reid*'s, he apprehends he is in a miſtake as to his judging of the middle of the eclipſe, and annular appearance; and reckons, that the annular appearance began at *Aberdeen* at 10ʰ 19′, and ended as above. By which the total duration of the *annulus* was 5′ 48″: and the end of the eclipſe at *Aberdeen* was at 11ʰ 49′ 33.

The Rev. Mr *Irwin*, at *Elgin*, lat. 57° 34′, ſays, the eaſtern limb of the moon touched or entered on the weſtern limb of the ſun at 8ʰ 57′; tho' he ſuſpects it began a little ſooner (another having taken the teleſcope out of his hand); for when he looked, the moon was a little advanced on the diſc of the ſun about 30° from the *zenith* of the ſun towards the W.

	h	′	″
The eaſtern cuſp in the *zenith* of the ſun at - - - - -	9	6	10
Eaſtern limb of the moon reached the centre of the ſun at	9	39	0
The *annulus* began about 30° from the *zenith* of the ſun weſtward at - - - - - - - - - - - - - - -	10	20	0
The *annulus* appeared moſt perfect at - - - - - - - -	10	22	45

Tho', as nearly as he could diſcern, he thought it a little narrower on the S. W. limb of the ſun, than it was on the oppoſite ſide. From hence it ſhould appear, that the centre of the eclipſe was to the ſouthward of *Elgin*.

The *annulus* was obſerved to break on the S. E. limb of the ſun, about 30° from the *nadir*, at 10ʰ 25′ 30″.

Before the joining of the cuſps of the ſun, as alſo at the breaking of the *annulus*, he ſays, he obſerved a quick tremulous motion, and ſeveral irregular

irregular bright spots between the cusps, which disappeared in a few moments; and he thought the moon's body passed quicker about the time of the *annulus* (especially as it was forming), than at any other time during the eclipse.

Before the western limb of the moon reached the centre of the sun's disc, the sun was hid under a cloud, and continued so, till within some little time of the end of the eclipse, which happened at $11^h\ 50'$.

There was no cloud all the time of the formation of the *annulus*, or the duration of it; and he thinks he is pretty right, as to the time of its continuance; for both the formation and breaking were very sensibly to be observed, and passed in a moment; affording a very pleasant sight, by the irregular tremulous spots of the sun.

He says, the darkness, during the *annulus*, was not so great as a little before and after; and, when greatest, was only somewhat duskish, but observable. Some saw a star to the east of the sun; but he saw it not, nor any present with him. He was told of it after his observation was over.

He says, that by an observation taken of the sun that day at noon, he found that his clock was somewhat less than $1'$ faster than the sun. He says also, that he observed this eclipse with a telescope 3 feet long, and that he had a very good burning-glass; but that it had little force, during the *annulus*, and some short time before and after.

Mr *Duncan Frazer* writes to Mr *Monro*, Prof. Anat. *Edin.* that he went to the house of *Culloden*, lat. $57°\ 29'$ N. on purpose to observe the eclipse; it having been said, that the centre of the eclipse would pass there; and after having adjusted his clock by the regulator-clock of a watch-maker at *Inverness*, he observed the eclipse with a telescope 5 feet long, and found

	h	′	″
The beginning precisely at - - -	8	37	36
Beginning of the *annulus* at - - -	10	0	10
End of the *annulus* at - - -	10	5	10
End of the eclipse at - - - -	11	29	30

By comparing his observation with that sent him by Mr *Irwine* at *Elgin*, he imagines his clock was not set to true time, since there is so great a difference, and more than the difference of longitude between the two places will allow; it being no more than 26 computed miles, and nearly in the same parallel of latitude.

Mr *Murdock Mackenzie* (who has for some years past been making a survey of the islands of *Orkney*, and whose abilities for such an undertaking give us hopes he will for the future, free navigators of a great many melancholy disasters, which formerly happened in those seas, thro' the want of true charts) made the following observation at *Kirkwall* in the island of *Pomona* in *Orkney* the latitude of which is $58°\ 58'$ N.

Beginning

	h	′
Beginning of the eclipſe about - - -	8	40
End of the eclipſe about - - -	11	37

He ſays, that by reaſon of clouds, he could not be perfectly exact, as to the preciſe time of beginning or ending; but adds, that the beginning cannot be more than 4′ wrong, nor the end more that 2′. He ſays, he is ſure he did not ſee it annular, but that there remained about ¼ or ⅕ of the ſun's circumference intercepted at the middle of the eclipſe.

P. S. It having been an opinion pretty generally received, that the darker parts of the moon's ſurface are water, I take this opportunity to remark, that though thoſe leſs lucid ſpaces are for the moſt part, to appearance, evenly extended ſurfaces, when teleſcopes of ſmall magnifying powers are made uſe of, yet, when they are examined with larger magnifiers, it is eaſy to diſcern on them many protuberances in a longitudinal direction; and that theſe riſings are really elevated above the common plane ſurface, is paſt all queſtion, from their projecting ſhadows, always oppoſite to the ſun: Moreover, they are of the very ſame colour as the plane they ariſe from, of the like ſmooth ſurfaces, without any ſenſible aſperities; and invariably the ſame, under the like poſitions of the ſun to the moon, at leaſt as far as I have been able to diſcover in 12 or 15 years frequent obſervations of them.

5 Being prepared for the obſervation of this eclipſe with a reflecting teleſcope about 2 feet long, and being ſufficiently acquainted with the motion of an aſtronomical pendulum, which I had uſed in my voyage to *Peru*, in making ſeveral obſervations. I obſerved the beginning of the eclipſe to have happened in true time about - $8^h\ 49'\ 6''$

——*at* Madrid, *by* Don Antonius de Ulloa, S. S. R. N° 491, p. 10, Jan. &c. 1749. *Preſented* Jan. 26, 1748-9. Fig. 7.

The ſpot *a b* in the diſk of the ſun between it's eaſtern and ſouthern parts, which could then be eaſily diſcerned, becauſe there was no other near it, began to be immerged - - - - 10 23 44½.

The total immerſion of this ſpot - - - 24 46½.

I was not able to obſerve either the emerſion of this ſpot, or the end of the whole eclipſe; for the ſun, being in it's greater altitude above the horizon, made the uſe of the teleſcope inconvenient. Nor could the particular number of the digits be conveniently determined for ſeveral reaſons.

About the beginning of the eclipſe, part of the lunar diſk had a colour inclining to red, which afterwards increaſed with the increaſe of the eclipſe. The day was clear, and the atmoſphere free from clouds, and ſo it continued till evening. When the eclipſe was in the middle, there was ſome diminution of light; and it's reflexion was obſerved to be ſomething weaker; and the air was obſerved to have loſt ſomething of it's uſual heat; which alteration continued from ½ an hour after the beginning of the eclipſe to the end.

Fig. 6. *S* the ſun, *L* the moon, *a* the limb of the ſun at which the immerſion began, *b* the limb of the moon perceived before the beginning of the eclipſe.

Fig. 7.

Fig. 7. *NOSL* the solar disk; *L* the east, *O* the west, *N* the northern part of the disk, *S* the southern part, *a b* the spot observed in the disk of the sun; *c* another between the N, and the W. to which the eclipse did not reach; *d* other numerous spots in the middle of the solar disk.

Solar Eclipse, Jan 8. 1750. N. S. *at* Rome, *by Mr* Christopher Maire. N° 484. p. 322. Jan. &c. 1750. *Read* Feb. 1. 1749.

	h	′	″
V. 1. Beginning by a reflector of Mr *Short*, *Jan.* 7.	20	34	35
The first spot covered - - -	20	49	50
The rest could not be observed for the clouds -			
Quantity of the eclipse 7 dig. 48 min. - -	21	49	4
Again more exactly - 7 - 43 - -	21	51	28
The sun appears for a moment; horns nearly horizontal -	21	56	15
Two digits remain eclipsed - - -	22	55	37
One digit exactly - - - -	23	3	42
End of the eclipse - - -	23	11	32

The observation was made with a 7 foot tube, 2610 parts of the micrometer just clasping the sun's diameter. The place of observation was in lat. 41° 54′ 0″, and 4″ of time E. of *St Peter's*.

——*at the Observatory at* Berlin, *by M.* Grischow, *jun. and M.* Kies. *Translated from the* French. ibid. p. 339. *Read* Feb. 15. 1749-50.

	h	′	″	
2. The beginning was at	8	59	19½	true time.
The end of the eclipse at	11	20	5½	
The whole duration	2	20	46	

The observations were made with the greatest exactness, the weather being as favourable as could be wished, the whole time.

M. *Euler* observed in his own house, which stands a little to the W. of the S. W. of the Observatory, at the distance of 190 *Rhinland* yards (*verges*) in a strait line, that

	h	′	″	
The beginning was at	8	58	30	true time.
And the end at -	11	19	50	
The whole duration	2	21	0	

That is, 34″ more than at the Observatory.

The diameter of the umbra was 6½ *Rhinland* inches.

Letter from Mr Richard Dunthorne, *to the Rev. Mr* Charles Mason, *F. R. S. and* Woodwardian *Professor of* Nat. Hist. *at* Cambridge, *concerning the* Moon's *Motion*. N°482. p. 412. Jan. &c. 1747. *dated* Cambridge, Nov. 4. 1746. *Read* Feb. 5. 1747.

VI. In the preface to my lunar tables, I hinted, that one use of publishing those tables would be, the assisting of persons desirous farther to rectify the lunar Astronomy, by enabling them more readily to compare the *Newtonian* theory with observations. Since the publishing those tables, I have spent some time myself in that comparison; and here send you the result, that you may communicate it to the *R. Soc.* if you think it deserves to be made publick.

As the motion of every secondary planet must partake of the errors in the theory of its primary, I thought proper, before I undertook the examination of the lunar numbers, to compare those of the sun with observations. I compared several sets of Mr *Flamstead's* observations, after the method he himself teaches *, which, for many reasons, I think the best method hitherto used; and, with the concurrence of a gentleman well skilled in these matters, determined the mean motion of the sun at *Greenwich*,

* Prolegom. Hist. Cœlest. p 133, *& seq.*

wich the laſt day of *Dec.* at noon, 1700, *O. S.* ♑ 20° 43′ 40″ of its apogee, ♒ 7° 30′ 0″, and the greateſt equation of the ſun's centre 1° 55′ 40″; which, I am fully perſwaded, are very near the truth.

The theory of the ſun being thus ſettled, I proceeded to examine the elements of the lunar Aſtronomy. I began with obſervations of lunar eclipſes about the equinoxes, when the apogee of the moon was in the ſun's quadratures; becauſe at thoſe times I could conceive the moon's motion affected with no inequality: but the annual one, called by *Newton* the firſt equation, and the elliptic one, called *proſthaphæreſis:* From a compariſon of ſuch obſervations I obtained the moon's mean longitude, which came out 1′, at leaſt, greater than in the tables, and very nearly as *Newton* has it in the laſt edition of his *Principia.*

I went on to examine the place and motion of the apogee, and theory of the increaſe and decreaſe of the eccentricity, as well as the greateſt and leaſt eccentricities themſelves (from the beſt obſervations, and beſt ſituated that I could procure) all which agreed ſo well with the tables, about the ſun's mean diſtances, that I dare venture to make no alteration therein: indeed I think the 6th equation does not ſo well account for the variation of the motion of the apogee, and change of the eccentricity, according to the greater or leſſer diſtance of the ſun from the earth; and therefore I ſet myſelf to compute what change this difference of the ſun's action upon the lunar orbit would introduce in the moon's place in every ſituation of the ſun and lunar orbit; and found, after many tedious computations, that the ſun being in apogee, this change, where greateſt, would amount to about 4′, and to 4′ 16″, when the ſun is in perigee. In other diſtances of the ſun from the earth, this greateſt change is proportional to the difference of the cubes of the mean and preſent diſtances; and in every Situation of the moon, and of her orbit, the preſent is to the greateſt equation nearly as the ſine of the exceſs of the moon's mean anomaly above twice the annual argument to *radius*. It increaſes the moon's longitude, when the ſun is in his

$\left\{\begin{array}{l}\textit{Apogeon}\\ \textit{Perigeon}\end{array}\right\}$ Semicircle, and that exceſs $\left\{\begin{array}{c}\text{leſs}\\ \text{greater}\end{array}\right\}$

than 180°; and diminiſhes it when otherwiſe *.

In fine, I compared the theory of the moon, as to her longitude, with ſeveral obſervations, as well in the octants and ſemi-octants, as in the ſyzigies and quadratures, and found ſuch an agreement when the above corrections were made, as ſeemed rather to be wiſhed than hoped for, conſidering the many inequalities wherewith the ſun's action diſturbs the motion of the moon, and the defects to which the beſt obſervations I have hitherto met withal are liable.

I have compared 100 obſerved longitudes of the moon with the tables; *viz.* 25 eclipſes of the moon, all, except the firſt, taken from *Flamſtead*'s

Hiſtoria

* If this equation be increaſed and diminiſhed in a direct *ratio* of the moon's horizontal parallax, it will become more exact. And I think, if it were always diminiſhed by $\frac{1}{4}$ or perhaps $\frac{1}{5}$ part, it would agree better with obſervations.

Historia Cœlestis, the *Philos. Transf.* and the *Mem.* of the *R. Acad.* of *Sc.* the 2 great eclipses of the sun in 1706 and 1715: 25 select places of the moon from *Flamstead's Historia Cœlestis*, and 48 of those longitudes of the moon computed from *Flamstead's* observations (by Dr *Halley* as I suppose) printed in the first edition of the *Historia Cœlestis*. They are as follow:

25 Eclipses of the Moon, and 2 of the Sun, compared with the Tables corrected as above.

	Place of Observation.	A.D.	Apparent Time at Greenwich.	☽'s true Place observed.	M. Anom. ⊙	M. Anom. ☽	Ann. Arg^t.	☽'s Place computed.	Diff. from Observat.
			h ′ ″	s ° ′ ″	s °	s °	s °	s ° ′ ″	′ ″
a	Dantzick	1652	Septem. 7. 6 21 35	11 25 25 51	2 20	6 2	0 3	1 25 26 10	+ 0 19
		1670	Septem. 18. 14 36 48	0 6 13 29	3 1	6 0	0 0½	0 6 16 37	+ 3 8
	Greenwich	1678	Octob. 19. 8 21 54	1 6 47 9	4 1	7 16½	2 2	1 6 46 38	− 0 31
		1682	Febr. 11. 10 58 52	5 3 47 3	7 25	6 28½	1 14	5 3 46 14	− 0 49
	Paris	1684	Decem. 11. 10 47 6	3 1 11 20	5 24	1 8	7 16	3 1 13 7	+ 1 47
	Dantzick	1685	Novem. 30 10 35 30	2 19 40 0	5 13	11 26½	5 25	2 19 39 46	− 0 14
	Dublin	1686	Novem. 19. 11 13 11	2 8 11 57	5 2	10 12½	4 4½	2 8 11 28	− 0 29
	Greenwich	1689	Septem. 18. 14 30 37	0 6 32 8	3 1	4 24	10 8	0 6 34 16	+ 2 8
		1690	March 14. 9 56 30	6 4 37 11	8 25	9 18	3 15½	6 4 37 28	+ 0 17
		1696	May 6. 12 2 0	7 26 53 14	10 18	3 1½	8 28	7 26 53 40	+ 0 26
	Paris	1699	March 5. 7 13 44	5 25 32 27	8 16	8 27	3 1	5 25 32 14	− 0 13
⊙	New England	1703	Decem. 11. 18 29 8	3 0 49 56	5 23	11 24½	5 23	3 0 48 51	− 1 5
	Marseilles	1706	May 10. 21 13 57	1 20 46 44	10 12	6 22	7 6	1 20 46 32	− 0 12
	Greenwich		Octob. 10. 7 10 0	0 28 0 20	3 22	5 27	11 25	0 27 58 47	− 1 33
		1707	April 5. 13 39 0	6 26 18 33	9 17	11 9	5 3	6 26 19 0	+ 0 36
		1708	Septem. 18. 9 10 25	0 6 39 23	3 1	2 12	8 7	0 6 39 19	− 0 4
		1712	January 12. 7 34 0	4 2 57 47	6 24	1 17½	7 25½	4 2 58 37	+ 0 50
	Paris	1713	Novem. 20. 15 27 30	2 9 58 21	5 3	9 21	3 17	2 9 56 56	− 1 25
⊙	London	1715	April 22. 21 11 5	1 12 0 22	10 3	6 12½	6 22	1 11 59 36	− 0 46
	Paris	1717	March 15. 15 7 4	6 6 23 50	8 26	8 22½	2 27	6 6 23 31	− 0 19
		1719	August 18. 8 22 46	11 5 42 8	1 29	10 28	4 20	11 5 42 39	+ 0 31
		1722	June 17. 13 46 10	9 6 47 43	11 29	5 8½	10 25	9 6 47 33	− 0 10
		1724	Octob. 20. 15 40 40	1 9 0 0	4 2	5 25½	11 22	1 8 58 50	− 1 10
		1729	February 2. 8 42 55	4 25 13 39	7 16	3 25	9 14	4 25 14 40	+ 1 1
			July 28. 13 0 0	10 16 15 28	1 9	8 3	2 15	10 16 16 26	+ 0 58
		1731	June 8. 13 47 51	8 28 9 58	11 19	4 27	10 11	8 28 8 45	− 1 13
		1732	Novem. 20. 9 40 25	2 10 3 54	5 3	7 8	1 24	2 10 0 56	− 2 58

25 Places of the Moon, computed by myself from *Flamstead*'s Observations, compared with the Tables.

A D.	Apparent Time at Greenwich		h ′ ″	☽'s true Place observed. s ° ′ ″	M. Anom. ☉ s °	M. Anom. ☽ s °	Ann. Argt. s °	☽'s Place computed. s ° ′ ″	Diff. from Observat. ′ ″
1684	March	13.	8 9 8	2 28 48 40	8 25	2 25	11 19	2 28 50 44	+ 2 4
1693	March	6.	7 22 48	3 16 43 12	8 17	3 11½	11 7	3 16 44 0	+ 0 48
	Octob.	11.	18 12 34	3 28 34 2	3 24	2 27	5 15	3 28 38 21	+ 4 19
1694	February	27.	10 29 16	4 27 27 31	8 10	3 12	9 20	4 27 26 48	— 0 43
	August	23.	11 13 54	11 0 19 11	2 5	8 2	2 21	11 0 21 41	+ 2 30
1694	Septem.	15.	5 34 13	8 27 1 34	2 27	6 10	3 11	8 27 1 41	+ 0 7
	Septem.	21.	10 50 31	11 22 47 41	3 3	9 3½	3 17	11 22 49 16	+ 1 35
	Decem.	13.	6 2 53	0 6 28 43	5 25	8 26½	6 0	0 6 29 58	+ 1 15
1695	February	8.	3 55 22	1 5 14 3	7 21	9 7½	7 22	1 5 12 14	— 1 49
	July	9.	5 56 14	7 2 2 42	0 20	3 14	0 2	7 2 3 4	+ 0 22
	Septem.	8.	8 30 26	10 3 0 12	2 20	5 21	1 24	10 2 59 50	— 0 22
1696	January	16.	17 29 2	7 5 4 20	6 29	2 29	5 20½	7 5 6 11	+ 1 51
	March	4.	9 8 13	4 12 2 24	8 16	11 8	7 3	4 12 0 59	— 1 25
1697	February	18.	6 29 47	2 20 52 29	8 2	8 19	5 10	2 20 51 7	— 1 22
		19.	7 22 57	3 4 18 16	8 3	9 2½	5 11	3 4 17 54	— 0 22
	Septem.	15.	7 54 40	10 1 7 8	2 28	3 21½	11 9	10 1 5 38	— 1 30
1698	Septem.	8.	11 2 35	11 12 15 6	2 21	3 24	9 23	11 12 15 27	+ 0 21
	Novem.	27.	3 49 54	10 11 33 39	5 9	2 3½	0 3	10 11 31 13	— 2 26
1699	March	5.	12 8 23	5 28 15 59	8 16	8 29½	3 1	5 28 16 11	+ 0 12
1701	Septem.	28.	6 55 23	9 27 13 17	3 10	9 9	6 8	9 27 12 49	— 0 28
1702	Octob.	16.	6 16 49	10 3 25 41	3 28	8 11½	5 13	10 3 26 27	+ 0 46
1703	Septem.	13.	11 58 50	11 28 50 30	2 25	8 28	3 3	11 28 53 30	+ 3 0
	Octob.	6.	6 28 39	9 27 1 44	3 18	7 4	3 23	9 27 0 38	— 1 6
1706	Octob.	10.	11 11 10	1 1 11 15	3 22	6 0	11 25	1 1 10 17	— 0 58
1714	Septem.	6.	6 34 7	9 3 44 13	2 18	3 17	0 0	9 3 45 24	+ 1 11

48 Places

48 Places of the Moon, computed by Dr *Halley*, from *Flamstead*'s Observations, compared with the Tables.

A. D.		Apparent Time at Greenwich	☽'s true Place observed.	M. Anom. ☉.	M. Anom. ☽.	Ann. Arg.	☽'s Place computed.	Diff. from Observat.
		h ' "	s ° ' "	s °	s °	s °	s ° ' "	' "
1689	Novem.	16. 11 59 0	2 6 12 43	4 29	6 1	0 0	2 6 14 40	+ 1 57
	Decem.	9. 6 1 0	11 28 48 54	5 21½	3 21	0 20½	11 28 48 40	— 0 14
		10. 6 46 35	0 12 46 49	5 22½	4 4	0 21½	0 12 47 14	+ 0 25
		12. 8 26 33	1 12 13 8	5 24½	5 0½	0 23	1 12 10 39	— 2 29
		13. 9 24 30	1 27 38 36	5 25½	5 14	0 2½	1 27 36 16	— 2 20
		16. 12 42 0	3 15 14 54	5 28½	6 24	0 27	3 15 15 48	+ 0 54
1690	January	4. 3 3 46	11 11 12 59	6 17	2 24	1 14	11 11 13 8	+ 0 9
		6. 4 30 15	0 8 13 31	6 19	3 20½	1 16	0 8 14 36	+ 1 5
		10. 7 59 22	2 6 11 20	6 23	5 14½	1 19½	2 6 10 37	— 0 43
		12. 10 8 49	3 7 5 21	6 25	6 12½	1 21	3 7 3 59	— 1 22
		13. 11 14 0	3 22 36 28	6 26	6 25½	1 22	3 22 35 19	— 1 9
	February	2. 2 25 39	0 3 57 24	7 15½	3 5	2 10	0 3 56 9	— 1 15
		5. 4 51 10	1 16 31 33	7 18½	4 26	2 12½	1 16 34 15	+ 2 42
		7. 6 48 17	2 15 58 14	7 20½	5 24	2 14½	2 15 59 13	+ 0 59
		8. 7 51 54	3 0 56 20	7 21½	6 8	2 15½	3 0 56 31	+ 0 11
		10. 9 56 26	4 0 55 24	7 24	7 6	2 17	4 0 55 1	— 0 23
		11. 10 52 31	4 15 42 16	7 25	7 20	2 18	4 15 41 49	— 0 27
		14. 13 19 31	5 28 12 40	7 28	9 2	2 21	5 28 11 44	— 0 56
		19. 17 3 55	8 2 23 26	8 3	11 12	2 25½	8 2 21 17	— 2 9
		21. 18 45 37	8 27 1 25	8 5	0 10	2 27½	8 26 59 6	— 2 19

1690	February	22. 19 37 40	9 9 21 32	8 6	0 24	2 28	9 9 23 26	+ 1 54
	March	7. 5 50 38	2 26 22 50	8 18	6 12	3 9	2 26 25 20	+ 2 30
		11. 9 43 23	4 24 31 45	8 22	8 8	3 13	4 24 33 15	+ 1 30
		12. 10 33 9	5 8 31 54	8 23	8 22	3 14	5 8 33 11	+ 1 17
		13. 11 18 30	5 22 16 37	8 24	9 5½	3 15	5 22 17 26	+ 0 49
		14. 12 2 18	6 5 44 39	8 25	9 19½	3 15½	6 5 45 18	+ 0 39
		25. 20 54 54	10 24 54 20	9 6½	2 21	3 25½	10 24 53 20	− 1 0
b	April	7. 7 48 46	4 20 8 0	9 19	8 5	4 6	4 20 9 18	+ 1 18
		8. 8 36 30	5 3 59 40	9 20	8 19	4 7	5 4 0 34	+ 0 54
		9. 9 22 0	5 17 34 42	9 21	9 2	4 8	5 17 35 40	+ 0 58
		10. 10 5 19	6 0 54 50	9 22	9 16	4 9	6 0 55 35	+ 0 45
		11. 10 47 56	6 14 0 8	9 23	9 29	4 10	6 14 2 13	+ 2 5
		12. 11 30 56	6 26 55 56	9 24	10 12½	4 11	6 26 56 55	+ 0 59
		13. 12 17 31	7 9 41 19	9 25	10 26	4 12	7 9 42 20	+ 1 1
		14. 13 4 0	7 22 16 0	9 26	11 9½	4 12½	7 22 17 34	+ 1 34
		15. 13 52 44	8 4 45 10	9 27	11 23	4 13½	8 4 44 47	− 0 23
		18. 16 26 47	9 11 43 52	10 0	1 3½	4 16	9 11 44 53	+ 1 1
		20. 18 5 47	10 6 30 55	10 2	2 0	4 18	10 6 33 2	+ 2 7
		22. 19 37 30	11 2 8 6	10 4	2 27	4 20	11 2 8 45	+ 0 39
		24. 21 6 38	11 29 14 33	10 6	3 23½	4 21½	11 29 14 30	− 0 3
	May	14. 13 30 30	8 25 6 53	10 25½	0 7	5 8	8 25 8 48	+ 1 55
b		22. 19 41 40	0 6 50 26	11 3½	3 23	5 15	0 6 49 43	− 0 43
	June	3. 6 2 13	5 22 31 45	11 15	8 19	5 25	5 22 32 6	+ 0 21
	Septem.	1. 6 48 53	9 2 27 42	2 13½	11 15	8 12	9 2 31 20	+ 3 38
		8. 12 16 42	0 1 44 51	2 21	2 22	8 18	0 1 44 18	− 0 33
1691	February	23. 3 55 10	1 19 39 56	8 6	3 16	1 18	1 19 40 45	+ 0 49
	March	11. 18 27 51	9 7 30 5	8 22½	10 23½	2 2	9 7 30 23	+ 0 18
	Septem.	22. 8 51 3	10 19 26 24	3 4	11 16	7 19	10 19 28 32	+ 2 8

a, The

a, the time of the middle of this eclipse here set down is from the beginning and end; but *Hevelius* says he could not observe the beginning exactly. Several intermediate *phases* compared together shew the middle to have been about 4′ sooner; to which the moon's place computed is $0^s\ 6^\circ\ 14'\ 3''$ and diff. $+\ 34''$.

b, *b*, *b*, the moon's places, observed on *Feb.* 2. *April* 7. and *May* 22. are computed by myself, from the observations; there being manifestly errors, either of the computation or press, in those printed in the *Hist. Cœlestis.*

Several observed latitudes of the moon, which I have compared with the tables, shew them to be very near the truth, both in the motion of the nodes, and also in the quantity and variation of the inclination.

Of the Acceleration *of the* Moon, *by the same.* N° 492. p. 162. April, &c. 1749. *dated* Cambridge, Feb. 28, 1748-9. *Read* June 1, 1749.

VII. After I had compared a good number of modern observations made in different situations of the moon and of her orbit in respect of the sun, with the *Newtonian* theory, I proceeded to examine the mean motion of the moon, of her apogee, and nodes, to see whether they were well represented by the tables for any considerable number of years, and whether I should be able to make out that acceleration of the moon's motion which Dr *Halley* suspected.

To this end I compared several eclipses of the moon observed by *Tycho Brahe*, as they are set down in his *Progymnasmata*, *p.* 114, with the tables *, and found them agree full as well as could be expected; considering the imperfection of his clocks, and the difficulty there must commonly have been in determining the middle of the eclipse from the facts observed, as published in his *Hist. Cœlest.* Indeed the small distance of time between *Tycho Brahe* and *Flamstead*, rendered *Tycho*'s observations but of little use in this enquiry.

The next observations that occurred to me were those of *Bernard Walther* and *Regiomontanus*, which being at double the distance of time from *Flamstead* that *Tycho*'s were, seemed to promise some assistance in this matter: upon comparing such of their eclipses of the moon whose circumstances are best related with the tables, I found the computed places of the moon were mostly 5′ too forward, and in some considerably more, which I could hardly perswade myself to throw upon the errors of observation; but concluded, that the moon's mean motion since that time, must have been something swifter than the tables represent it; though the disagreement of the observations between themselves is too great to infer any thing from them with certainty in so nice an affair.

Then I compared the four well-known eclipses observed by *Albategnius* with the tables, and found the computed places of the moon in three of them considerably too forward: this, if I could have depended upon the longitude

* My tables corrected as in my former letter; which is always to be understood of the tables mentioned in this.

longitude of *Aracta*, would very much have confirmed me in the opinion, that the moon's mean motion muſt have been ſwifter in ſome of the laſt centuries than the tables make it; though the differences between theſe obſervations, and the tables, are not uniform enough to be taken for a certain proof thereof.

I could meet with no obſervations of eclipſes to be at all depended upon between thoſe of *Regiomontanus* and *Albategnius*, except two of the ſun and one of the moon made at *Cairo* in *Egypt*, related in the *Prolegomena* to *Tycho Brahe*'s *Hiſt. Cœleſt. p.* 34; nor any between thoſe of *Albategnius* and *Ptolemy*, beſides the eclipſe of the ſun obſerved by *Theon* at *Alexandria*; notwithſtanding I carefully ſearched all the remains of antiquity I could find with that view. Theſe eclipſes of the ſun are the more valuable, becauſe they were obſerved in places the longitudes and latitudes whereof are determined by Monſieur *Chazelles* of the *R. Acad. Sc.* who was ſent by the *French* King in the year 1693, with proper inſtruments for that purpoſe. *

The ſolar eclipſe obſerved by *Theon* was in the 1112th year of *Nabonaſſar* the 24th day of *Thoth*, according to the *Egyptians*, but the 22d day of *Pauni*, according to the *Alexandrians*: he carefully obſerved the beginning of 2 temporal hours and 50′ afternoon, and the end at 4½ hours nearly afternoon at *Alexandria*. *Theonis Comment. in Ptol. mag. Conſtruct*, *p.* 322. This eclipſe was *June* 16, in the year of Chriſt 364: and the temporal hour at *Alexandria* being at that time to the equinoctial hour as 7 to 6, makes the beginning at 3 equinoctial hours and 18′ afternoon, and the end at 5 equinoctial hours 15′ nearly.

The eclipſes obſerved at *Grand Cairo* were as follow.

" Anno Hegiræ 367, die *Jovis*, qui erat 28, rabie poſterioris (is eſt " ordine menſis quartus, & incipit ille annus *Saracenicus* die 19 *Auguſti*, " anno *Chriſtiano* 977) obſervatum fuit *Cahiræ* in *Ægypti* metropoli ini- " tium eclipſis ſolaris, cum altitudo ſolis eſſet 15° 43′. quantitas obſcu- " rationis 8 digit. Ea finita ſol elevabatur 33½ *gr.* *Ex Schickardo in* " *MS.*"——This eclipſe was *Decem.* 13, in the year of Chriſt 977, the beginning at 8^h 25′, and the end at 10^h 45′, apparent time in the morning.

" Anno eodem die Sabbathi, videlicit 29 menſis *Sywal* (numero decimi, " qui Paſchalis eſt eorum) eclipſis ſolis occupavit digitos 7½. In prin- " cipio ſol altus ſere 56°. In fine ſol occiduus elevabatur gradibus 26. " *Ex Schickardo in MS.*"——This eclipſe was *June* 8, in the year of Chriſt 978. The beginning at 2^h 31′, and the end at 4^h 50′ apparent time afternoon.

" Anno Hegiræ 368 (qui incœpit die 9 *Auguſti*, anno *Chriſtiano* 978) " die *Jovis*, 14 *Sywal*, luna fuit orta cum defectu, qui at 5½ digitos ac- " crevit; cum extaret ſupra horizontem gradibus etiam 26 ſubaudio fi- " nem tunc accidiſſe). *Schickardus.*"——This eclipſe was *May* 14, in the

* *Du Hamel, Hiſt. Acad.* p. 309. 395.

year

year of Christ 979; but as the middle cannot be known from what was observed of it, I made no use thereof in this enquiry. The account concludes with the following paragraph:

"Hæ tres observationes habitæ sunt ab *Ibn-Junis*, qui jussu regis *Abu* "*Haly Almanzor*, sapientis, *Ægypto* tunc imperantis, rebus vacabat cœ-"lestibus. Hujus authoris tabulas habet *Jac. Golius* professor *Lugdun.* "(qui mihi inde communicavit ista) in quibus plures aliæ, sui & superioris "ævi observationes extant. Locus observationis propinquus urbi *Cahiro.* "*Schickardus.*"

That the before-mentioned solar eclipses might be applied to the examination of the lunar motions, I contrived the following method; which I think renders eclipses of the sun as useful at least as those of the moon are in that business.

Let ABC represent half the earth's enlightned disk, AEC a portion of the ecliptick projected thereon, FGH the path of the moon's shadow over the disk, EI the universal meridian, α the situation of the place at the beginning of the eclipse, β it's situation at the end thereof, δ the centre of the shade at the beginning, and ϵ its centre at the end of the eclipse. Draw EG, $\alpha\zeta$, and $\beta\eta$, perpendicular to the path of the shadow, $\beta\gamma$ parallel thereto; join $\alpha\delta$ and $\beta\epsilon$, and through α draw $\theta\alpha\iota$ perpendicular to AC.

Then (computing the true places of the sun and moon at the observed times of the beginning and end of the eclipse) we shall have given $\delta\epsilon$ the motion of the moon from the sun in her orbit during the time of the eclipse, and $\alpha\delta = \beta\epsilon$ the semidiameter of the *penumbra*; which are to be reduced into such parts as the semidiameter of the disk contains 10000: The angles BEI and BEG, being found by methods commonly known, GEI their sum or difference will be likewise given. Also $E\alpha$ and $E\beta$ will be sines of the sun's altitude at the beginning and end of the eclipse respectively; $IE\alpha$ and $IE\beta$, are the angles at the sun betweeen the vertex of the place and the pole of those times; which being found, the angle $\alpha E\beta$, their difference will be known, from whence the line $\alpha\beta$ and the angle $E\alpha\beta$ may be computed.

The angle $GE\alpha$ is the sum or difference of the known angles GEI and $IE\alpha$: In the figure before us, the complement of this to a semicircle is $E\alpha\gamma$; which being subtracted from $E\alpha\beta$ leaves the angle $\gamma\alpha\beta$, from whence and the line $\alpha\beta$, $\alpha\gamma$, and $\gamma\beta = \zeta\eta$ may be found.

Let $a = \delta\epsilon - \zeta\eta$, $b = \alpha\delta = \beta\epsilon$, $c = \alpha\gamma$, and $x = \beta\eta = \gamma\zeta$.

Then $\sqrt{bb - xx} = \eta\epsilon$, and $\sqrt{bb - cc - 2cx - xx} = \delta\zeta$, by *Eucl.* 1. 47.

Consequently $a - \sqrt{bb - xx} = \sqrt{bb - cc - 2cx} = xx$ which being reduced, gives us the quadratic equation $xx + cx = \dfrac{4a^2b^2 - a^4 - 2a^2c^2}{4aa + 4cc}$.

This equation solved, gives us the value of x, from which $\delta\zeta$ and $\eta\epsilon$ will be likewise had. In the triangle $\alpha\zeta\theta$ we have $\alpha\zeta$ and the angle $\zeta\alpha\theta =$ GEB

GEB given, whence $\kappa\theta$ and $\zeta\theta$ may be found: consequently $\delta\theta$ will be known; and from the observed time of the beginning of the eclipse, and hourly motion of the moon from the sun, the time when the centre of the shade is at θ will be had. Lastly, in the triangle $E\iota\kappa$, we have given the side $E\alpha$, and the angle $E\kappa\iota = BE\alpha$ (the sum or difference of the angles BEI and $IE\alpha$); therefore the sides $E\iota$ and $\kappa\iota$ may be found. But $E\iota$ is the distance of the moon from the sun in the ecliptic, and $\alpha\iota \pm \alpha\theta$ the moon's latitude at the time when the centre of the shade is at θ; which may be compared with the computation from the tables for that time.

By this means I compared the aforesaid solar eclipses with the tables, and found the difference in longitude and latitude, as follows.

A.D.	Apparent time at Greenwich.	Dist. ☽ a ☉ from *E*.	Lat. ☽ from *E*.	☽ a ☉ by Tab.	Lat. ☽ by Tab.	Diff. from Obser. in Long.	Diff. from Obser. in Lat.	Diff. in Lat. from Digits observed.
	h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
364	June 16. 2 4 20	39 41 in conseq.	34 47 N.	35 25	37 26 N.	−4 16	+2 49	
977	Dec. 12. 19 12 30	43 39 in antec.	30 23 N.	36 3	31 50 N.	+7 36	+1 27	− 2 36
978	June 8. 1 16 10	29 3 in conseq.	8 24 S.	37 48	3 21 S.	+8 45	−5 3	+ 3 38

The agreement there is between the two last of these differences in longitude, shews that the tables represent the mean motion of the moon's apogee very well for above 700 years, the moon being very near her perigee at the time of one of those eclipses, and near her apogee at the time of the other.

By the same method I also compared the sun's eclipse, *July* 29, 1478. (which appears, from what is related of it, to have been carefully observed by *Bernard Walther* at *Nuremberg*), with the tables, and found the difference in long. to be + 10′ 29″, and in lat. + 9′ 12″. This wide difference in lat. from the tables, that agree so well with the former ancient observations, confirmed me in the opinion, that the *Nuremberg* observations are too inaccurate to determine any thing from them in this affair.

The eclipses recorded by *Ptolemy* in his *Almagest*, are most of them so loosely described, that, if they shew us the moon's mean motion has been accelerated in the long interval of time since they happened, they are wholly incapable of shewing us, how much that acceleration has been. There are indeed two or three of them attended with such lucky circumstances as not only plainly prove, that there has been such an acceleration, but also help us to guess at its quantity. One of these is the eclipse, said by *Hipparchus* to have been observed at *Babylon*, in the 366th year of *Nabonassar*, the night between the 26th and 27th days of *Thoth*, when a small part of the moon's disk was eclipsed from the N. E. half an hour before the end of the night, and the moon set eclipsed. This was in the year before Christ 383, *Decemb.* 22. The middle of this eclipse at *Babylon* (supposing with *Ptolemy* the meridian of that place to be 50′ in time E. of the meridian of *Alexandria*), by my tables was *Dec.* 22, 21^h 4′ apparent time; the duration was 1^h 37′, *Ptolemy* makes it 1^h 30′ nearly; whence

whence the beginning ſhould have been about $8^h\ 15'$ after midnight: according to *Ptolemy*, the night at *Babylon* was at that time $14^h\ 24'$ long, and therefore ſun riſe at $7^h\ 12'$ after midnight; and as the moon had then S. lat. and was not quite come to the ſun's oppoſition, her apparent ſetting muſt have been ſomething ſooner, *i. e.* more than an hour before the beginning of the eclipſe, according to the tables; whereas the moon was ſeen eclipſed ſome time before her ſetting; which, I think, demonſtrates, that the moon's place muſt have been forwarder, and conſequently her motion ſince that time leſs than the tables make it by about $40'$ or $50'$. But the computed place of the moon in each of the before-mentioned ſolar eclipſes obſerved at *Grand Cairo*, being about $8'$ before her place, from obſervation ſhews us, that the mean motion of this luminary has been ſomething greater in the laſt 700 years than the tables ſuppoſe it, and therefore muſt have been accelerated.

This acceleration is further confirmed by the eclipſe, which *Hiparchus* ſays was obſerved at *Alexandria*, in the 54th year of the ſecond *Calippic* period, the 16th day of *Meſſori*, when (he ſays) the moon began to be eclipſed half an hour before her riſing, and was wholly clear again in the middle of the third hour of the night. This was in the year before Chriſt 201. *Sep.* 22. The middle of this eclipſe at *Alexandria* by the tables was *Sept.* 22. $7^h\ 44'$ apparent time; and the duration $3^h\ 4'$, which makes the beginning at $6^h\ 12'$ apparent time, that is, about $10'$ after the riſing of the moon at *Alexandria*, or $40'$ later than the beginning from obſervation. This difference in time makes a difference of near $20'$ in the moon's place.

The moſt antient eclipſe of which we have any account remaining, namely that related by *Ptolemy*, to have been obſerved at *Babylon* the firſt year of *Mardokempad*, in the night between the 29th and 30th days of *Thoth*, in which the moon began to be eclipſed when one hour after her riſing was fully paſt; if, by reaſon of the lat. of the expreſſion, it be not a direct proof of the acceleration, it may nevertheleſs help to limit it's quantity. This eclipſe was in the year before Chriſt 721. *March* 19. The middle whereof at *Babylon*, by the tables, was *March* 19. $10^h\ 26'$ apparent time; and the beginning at $8^h\ 32'$ the apparent riſing of the moon at that place was about $5^h\ 46'$ afternoon; ſo that the obſerved beginning of the eclipſe was at leaſt $6^h\ 46'$ afternoon, *i. e.* not above $1\frac{3}{4}^h$ before the beginning, by the tables: wherefore the moon's true place could precede her place by computation but little more than $50'$ at that time.

If we take this acceleration to be uniform, as the obſervations whereupon it is grounded are not ſufficient to prove the contrary, the aggregate of it will be as the ſquare of the time: and if we ſuppoſe it to be $10''$ in 100 years, and that the tables truly repreſent the moon's place about *A. D.* 700. it will beſt agree with the before-mentioned obſervations;

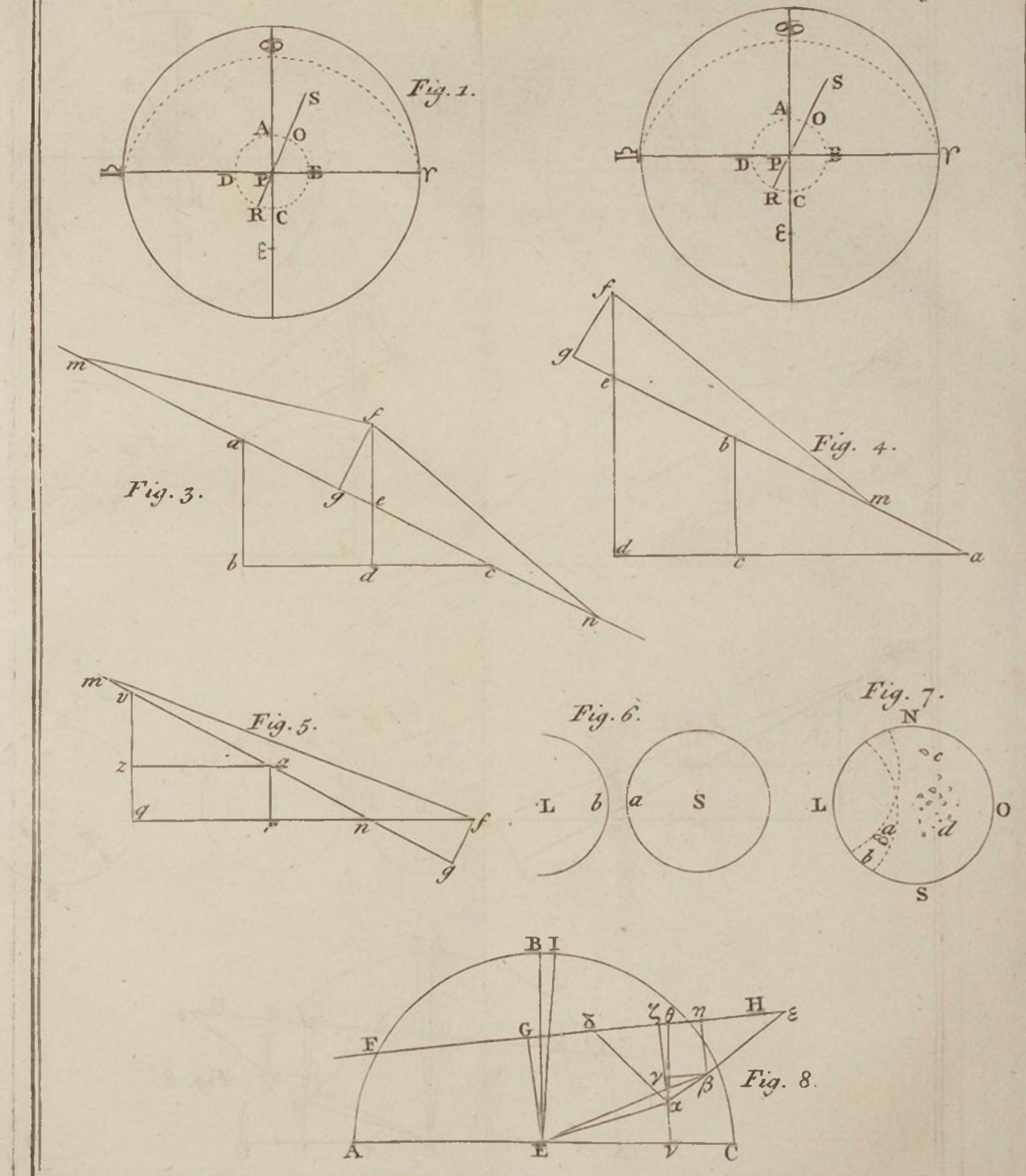
Fig. 1.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.

vations; and the difference between the moon's place by the tables and her place in the heavens, will be as follows.

Years before Chriſt	Error of Tab.		Years before Chriſt.	Error of Tab.		Years of Chriſt.	Error of Tab.		Years of Chriſt.	Error of Tab.		Years of Chriſt.	Error of Tab.	
	′	″		′	″		′	″		′	″		′	″
700	— 56	0	200	— 28	30	300	— 9	20	800	+ 1	30	1300	+ 4	0
600	— 49	50	100	— 24	0	400	— 6	30	900	+ 2	40	1400	+ 3	30
500	— 44	0	A.D.0	— 19	50	500	— 4	0	1000	+ 3	30	1500	+ 2	40
400	— 38	30	100	— 16	0	600	— 1	50	1100	+ 4	0	1600	+ 1	30
300	— 33	20	200	— 12	30	700	0	0	1200	+ 4	10	1700	0	0

	d	h	′	″	
VIII. 1. *July*	28.	10.	13.	28.	The *penumbra* diſcernible.
			06.	30.	The beginning, as moſt of the company judged.
			18.	38.	*Mare humorum* juſt touch'd.
			26.	24.	Began to touch *Tycho*.
			27.	51.	*Tycho* biſected.
			24.	09.	*Tycho* cover'd.
			29.	53.	Touch'd *Grimaldi*.
			30.	25.	*Mare humorum* cover'd.
			34.	14.	*Grimaldi* cover'd.
		12.	24.	30.	The End.
			27.	40.	The *penumbra* quite gone.

The Moon's Eclipſe *of* July 28, 1748. *obſerv'd at* Marlborough houſe, *by* J. Bevis, M. D. N°. 489. p. 522. Oct. *&c.* 1748. *Read* Nov. 10, 1748.

About the middle of the eclipſe, the moon's diameter, perpendicular to the equator, meaſur'd in a 5 foot teleſcope was 33′ 50″; perhaps 15″ or 20″ greater than it would have been found to be with a 12 foot tube.

2. I made uſe of the ſame teleſcope, with which I obſerved the eclipſe of the ſun mentioned above. The *phaſes* in this eclipſe being reduced to true time are as follows.

—— *at* Madrid, *by* D. Ant. de Ulloa. N°. 491. p. 12. Jan. *&c.* 1749. *Preſented* Jan. 26, 1748-9.

	h	′	″
The *penumbra* began to be perceived at - - - - - - -	9	45	42
The beginning of the eclipſe, not without ſome doubt -		50	0
Immerſion of *Capuanus* - - - - - - - - - - - - -	10	0	13
Beginning of the immerſion of *Mare humorum* - - - -		4	10
Tycho begins to enter the ſhadow - - - - - - - - -		11	54
Total immerſion of *Tycho* - - - - - - - - - -		14	14
Beginning of the immerſion of *Grimoaldus* - - - - - -		15	15
Total immerſion of *Mare humorum* - - - - - - - -		15	18
Total immerſion of *Grimoaldus* - - - - - - - - - -		20	51
Reinoldus enters the ſhadow - - - - - - - - - - -		28	40
Snellius and *Furnerius* touch it - - - - - - - - - -		44	40

	h	′	″
Snellius and *Furnerius* under the shadow - - - - - -		47	40
Fracastorius begins to be immerged - - - - - - - -		49	0
Grimoaldus begins to emerge - - - - - - - - - -		51	16
Mare nectaris begins to be immerged - - - - - -		52	20
Grimoaldus totally emerged - - - - - - - - - -		56	32
Beginning of the immersion of *Mare fœcunditatis* - - -	11	13	58
Mare humorum begins to emerge - - - - - - - -		19	11
totally emerged - - - - - - -		30	18
Total emersion of *Mare nubium* - - - - - - - - -		40	24
Total emersion of *Mare nectaris* - - - - - - - - -		45	16
Tycho begins to emerge - - - - - - - - - - -		47	35
Total emersion of *Tycho* - - - - - - - - - - - -		49	54
End of the shadow on the lunar disk - - - - - - - -	12	10	22
End of the stronger *penumbra* - - - - - - - - -		17	25
End of any *penumbra* whatsoever - - - - - - - -		22	12

The beginning of the eclipse was doubtful, because the shadow and the *penumbra* were not well discerned; and therefore they could not well have been determined, tho' the atmosphere had remained clear, and free from all impediments.

An Eclipse *of the* Moon, Dec. 12. 1749. *observed at Mr* Graham's *in* Fleet-street, *by* John Bevis, *M. D. and Mr* James Short, *F. R. S.* N° 493. p. 247. Oct. &c. 1749. *Read* Dec. 14. 1749.

1749.	By the Clock.			App. Time.			
				h	′	″	
IX. 1. *Dec.* 11.	23	56	15½				The sun passed the meridian.
12.	6	43	0	6	46	36	A sensible *penumbra.*
	6	47	20	6	50	56	Eclipse begins.
	7	1	26	7	5	1	Shadow touches *Tycho.*
	7	3	12	7	6	47	*Tycho* half covered.
	7	4	38	7	8	13	*Tycho* covered.
	8	33	37	8	37	11	*Tycho* begins to be uncovered.
	8	34	50	8	38	24	*Tycho* half uncovered.
	8	36	9	8	39	43	*Tycho* quite uncovered.
	9	9	5	9	12	38	Eclipse ends.
	9	13	30	9	17	3	*Penumbra* gone.
	12	5	53½				Moon's centre passed the meridian.
	12	20	2				*Sirius* passed, his mean right ascension being 98° 31′ 38″.
13.	23	56	46				The sun passed the meridian.

The appulses of the shadow to the spot *Tycho* were observed with a reflecting telescope, which magnified about 40 times, and may be serviceable for geographical purposes. The beginning and end of the eclipse were estimated by the bare eye, and a refracting telescope of a small magnifying power; larger powers being apt to dilate the shadow too much, and thereby render these phases more uncertain.

A com-

	Apparent Time.		
	h	′	″
A computation by Dr *Halley*'s tables gave the beginning -	6	52	0
end - - -	9	14	58

2. —*At* Earith, *near* St Ives, *in* Huntingdonſhire, *by Mr* Wm. Elſtobb, *jun.* *in a letter to* Martin Folkes, *Eſq;* *Pr. R. S.* ibid. p. 280. *Read* Dec. 21. 1749.

h	′	
7	0	The umbra came on the lower limb of the moon, almoſt directly under the ſpot called *Tycho*, in *Keil*'s map of the moon.
	$2\frac{3}{4}$	The penumbra overſpread *Tycho*.
	6	The umbra approached the lower part of *Mare humorum*, and *Tycho* immerged into the umbra.
	21	*Mare humorum* totally immerged into the umbra.
	41	The lower part of *Mare nectaris* immerged into the umbra.
	57	The N. E. limb began to evolve itſelf; and that part of the limb below the ſpot called *Grimaldus*, began to appear brighter than when the penumbra covered it.
8	9	The upper part of *Mare humorum* emerged from the umbra.
	21	*Mare humorum* totally emerged.
	$45\frac{1}{2}$	*Tycho* emerged from the umbra.
	$51\frac{1}{4}$	The penumbra left *Tycho*.
	$54\frac{1}{2}$	*Mare nectaris* emerged from the umbra.
9	0	The penumbra left *Mare nectaris*.
	$4\frac{3}{4}$	*Mare fœcunditatis* emerged from the umbra.
	16	The umbra left the moon a little below *Mare fœcunditatis*.
	18	The penumbra went off, and the eclipſe ended.

At the time of the greateſt obſcuration, the edge of the umbra paſſed below *Grimaldus*; approached the lower part of *Peninſula fulgurum*; paſſed over the upper part of *Mare nectaris*, and croſſed about the middle of *Mare fœcunditatis*. The edge of the umbra did not ſeem to make one regular curve, but looked like two curves, meeting in a very obtuſe angle near *Peninſula fulgurum*. And that part of the moon, immerſed in the umbra, was not viſible.

3. It was ſo boiſterous a day, that I deſpair'd of being able to ſee this eclipſe, and for that reaſon neglected to put my micrometer in order. My clock had likewiſe been altered without my knowledge, on which account I betook myſelf too late to the obſervation, as will appear by the following detail. The place of obſervation is in lat. 41° 54′ 0″. and 4″ of time eaſtward of *St Peter*'s. For I take it for granted that the *Thermæ Diocleſianæ* are, according to *Bianchini*'s determination, in lat. of 41° 54′ 27″. —*At* Rome, *by Mr* Chriſtopher Maire. N° 494. p. 321. *Read* Feb. 1. 1748 9.

	h	′	″
Chord of the part eclipſed 13′ as was deduc'd from the map of the moon - - - - - - - - - - - - -	7	47	18
Hence beginning of the eclipſe - - - - - - - - - -	7	40	53

The

	h ′ ″
The ſhade to *Tycho* and *Capuanus* - - - - - - - - - -	7 54 3
Tycho entirely covered - - - - - - - - - - - -	7 55 56
Shade to *Fracaſtorius* - - - - - - - - - - - - -	8 28 43
Fracaſtorius quite hid - - - - - - - - - - - - -	8 30 24
Tycho entirely diſengaged - - - - - - - - - - -	9 30 24
End of the eclipſe, as far as could be perceiv'd thro' a thin cloud	10 0 16

I judg'd the eclipſe to be ſomewhat leſs than 5 digits.

Eclipſe *of the* Moon, June 8, 1750. *obſerved in* Surry-ſtreet *in the* Strand; *by Mr* John Catlin *and Mr* James Short, *F. R. S.* No. 496. p. 523. Nov. *&c.* 1750. *Read* Nov. 1. 1750.

X. 1. We expected to have ſeen the moon riſe eclipſed before the ſetting of the ſun; but were prevented by clouds. About half an hour after 9, we ſaw the moon then totally eclipſed; tho' conſiderably brighter on the E. than on the W. ſide; by which we found that ſhe was then paſt the middle of the eclipſe.

	h ′ ″
Emerſion, or end of total darkneſs, at - - - - - - -	9 45 0
End of the eclipſe at - - - - - - - - - - - - - -	10 51 30

Here follows a computation of the ſame eclipſe by Mr *John Catlin* from Dr *Halley*'s tables, which he ſays was done in a hurry; however he knows of no error in the calculation.

	h ′ ″
Beginning at - - - - - - - - - - - - - - - - -	7 14 25
Immerſion at - - - - - - - - - - - - - - - - -	8 21 20
True oppoſition at - - - - - - - - - - - - - -	9 0 24
Emerſion at - - - - - - - - - - - - - - - - -	9 45 52
End at - - - - - - - - - - - - - - - - - - -	10 52 53

—— *at* Wittemberg, *by* G. M. Boſe, *Prof. of* Phyſicks. No. 496. p. 570. Nov. *&c.* 1750. *Read* Nov. 22, 1750.

2.

h ′ ″	alt. ☉ ° ′	h ′ ″	h ′ ″	h ′ ″		h ′ ″
8 11 4	38 34	3 47 40	23 58 44	11 59 22	hence noon	11 59 26
12 30	38 44	46 9	58 39	59 19½	corr. for dec. ☉	— 3
18 17	39 40	40 31	58 48	59 24	noon correct.	11 59 23
20 23	40 0	38 39	59 2	59 31	and correction	
23 30	40 25	35 17	58 47	59 23½	of the clock.	+ 37
27 36	41 0	31 23	58 59	59 29½		
29 4	41 15	30 2	59 6	59 33		

The eaſtern part of the ſky continued covered with clouds, at

length

Length time by the clock			true time			Moon visible. Emersion a good while past. Aristarchus already discovered. lucid parts rev.		that is	
h	′	″	h	′	″	rev.	″	′	″
10	50	0	10	50	37	3	220	9	49
	54	0		54	37	4	75	11	26
	58	13		58	50	4	192	12	19
11	0	22	11	0	59	5	37	13	52
	3	36		4	13	5	245	15	26
	6	3		6	40	5	349	16	13
	9	53		10	30	6	152	17	27
	12	1		12	38	6	321	18	43
	14	8		14	45	7	122	19	56
	16	9		16	46	7	265	21	1
	18	32		19	9	7	319	21	25
	20	40		21	17	8	55	22	9
	23	7		23	44				
	25	16		25	53				

I can hardly believe that the difference of meridians can be safely determined by lunar eclipses.

Time by the clock						
h	′	″	h	′	″	
11	40	0	11	40	37	end of the shadow according to me.
	40	40		45	17	end according to a friend.
				40	30	end by the projection of a friend.
				39	38	end by the corrected Kalendar of Leipsick.
				39	11	end by the *connoissance des temps.*
				39	46	end by the *ephemerides* of *Manfredi.*

Making use of the difference of meridians which I determined in 1743, by a transit of ☿ over ⊙, which is also used by the Acad. in their *Connoissance.*

	rev.	°		′	″		
Diam. of ☽ by a microm. =	11	142	that is	30	57		
Hence semidiam. of ☽ =	-	-	- -	15	28½		
The same according to Nicas. Grammat. =				15	25	See Rost's	Tab. 12.
J. Gauppius				15	2½	Astron.	Tab. 13.
Kalend. Leips.				15	32		

XI.

Total Eclipse *of the* Moon, *observed* Dec. 2, 1750. *in the morn. in the* Strand, London, *about* 3'' *of time W. of* St Paul's, *and* 27'' *W. of the* R. Observ. *at* Greenwich; *by Dr* Bevis *and Mr* James Short, *F. R. S.* ibid. p. 575. *Read* Dec. 13, 1750.

	h	′	″
XI. A sensible *penumbra* (*Dec.* 1,) at	16	32	0
The eclipse judged to begin at		36	50
Grimaldi covered		40	20
Shadow touches *Mare humorum*		45	26
at the middle of *Kepler*		48	40
at the middle of *Aristarchus*		50	7
touches *Copernicus*		55	24
Copernicus half-cover'd		56	56
——— quite covered		58	5
Timocharis half-covered		59	0
Shadow touches *Tycho*		59	20
at the middle of *Tycho*	17	0	0
covers *Tycho*		1	3
at the middle of *Menelaus*		14	42
touches *Goclenius*		24	29
covers *Goclenius*		25	17
at the middle of *Proclus*		27	20
touches *Mare Crisium*		28	44
at the middle of *Mare Crisium*		31	15
covers *Mare Crisium*		33	30
Total immersion at		36	5
The moon begins to emerge	19	14	33
Grimaldi begins to emerge		16	4
quite uncovered		18	10

The moon was now got so low, and day-light so far advanced, that no more phases could be observed with any degree of certainty.

These observations were made with a reflecting telescope, that magnified 40 times, and a refracting telescope, which magnified 12 times; and the times were the same through these two telescopes; for the air was exceeding clear, and the shadow well defined, the *penumbra* being scarce sensible.

Here follows a computation, made from Dr *Halley*'s tables, by Mr *John Catlin*, of *Guy*'s hospital; and sent to Mr *Short* the day before the eclipse.

Dec. 1. in the morning 1750.	h	′	″
Beginning of the moon's eclipse	16	44	31
Immersion at	17	42	45
Emersion at	19	20	37
End at	20	18	51

From hence it appears, that the eclipse began about 8′ sooner than the computation from Dr *Halley*'s tables gave it; but the computation which Mr *Brent* made and published some time before the eclipse happen-

ed,

ed, was within 1′ of the time obſerved; and this exactneſs he imputes to his leaving out 3 of the 7 equations of the moon, publiſhed by Sir *I. Newton* in his theory of the moon.

Occultation of Jupiter *by the* Moon, *obſerved at* London, *by* J. Bevis, *M. D.* No. 473, p. 65. May &c. 1744. *Read* June 7, 1744.

XII.

	d	h	′	″	By the clock
1744 *June*	6	11	13	58	Immerſion of the centre of Jupiter certainly enough.
			35	14	α *Serpentaria* culminates.
			43	15	Emerſion of the centre.
					With a tube of 12 feet.

N. B. The clock was too ſlow 1′ 25″

The Phænomena of Venus, *repreſented in an Orrery made by Mr* James Ferguſon, *agreeable to the obſervations of S.* Bianchini. No. 479, p. 127. March, &c. 1746. *Read* March 20, 1745-6. *here printed with alterations.*

XIII. In all the Orreries that I have ſeen, *Venus* is repreſented as having her *axis* perpendicular to the plane of the ecliptic, and her diurnal motion thereon equal to 23 hours of our terreſtrial time. Hence, as her annual motion is performed in about 225 of our days, it will contain 234 of her's; conſequently, to an eye placed in *Venus*, the ſun will always appear to go through a ſign of the zodiac in 19½ of her days; and as her *axis* has no inclination, ſhe muſt have a continual equality of her days and nights, without any variation of ſeaſons, and ſo her annual motion can be of no other uſe than to keep her from falling down to the ſun.

But *Bianchini* gives a very different account of her; which is, that her *axis* inclines 75° from a line ſuppoſed to be drawn perpendicular to the plane of the ecliptic (by which I ſuppoſe he means her own ecliptic, and not the earth's); and that her diurnal motion is performed in 24 days and 8 hours of our time; and this will cauſe her year, which is almoſt equal to 225 of our days, to contain only 9¼ of her days; and this odd quarter of a day in *Venus* will make every fourth year a leap year to her, as happens to us on earth, by the 6 hours that our year contains above 365 days: and to her the ſun will appear always to go thro' a ſign of the zodiac in little more than ¾ of her day, which is equal to 18 ¼ of our days; and in going round the ſun, her N. pole conſtantly leans towards the 20th degree of *aquarius*.

Thus, with regard to the abſolute length of *Venus*'s year, *Bianchini* agrees with *Caſſini* and other Aſtronomers: but differs widely in other very remarkable particulars, from which ariſe ſo many advantages, as to make that planet incomparably more fit for it's inhabitants, than we could poſſibly conceive it to be by a quick rotation on an *axis* perpendicular to it's annual path. For *Venus* is ſo much nearer the ſun than our earth is, that it is well known ſhe muſt have twice as much light and heat as our earth has; and then, was the ſun always perpendicularly above her equator, we cannot imagine but that her equatorial parts muſt be burnt up with heat, and her polar parts uninhabitable, by reaſon of the greatneſs of cold, occaſioned by the ſun-beams being parallel to, or making ſo very acute angles with, the horizon.

But,

But, by ſuch a motion as *Bianchini* deſcribes, and which I have exactly repreſented in my Orrery, theſe inconveniences are avoided; for there is no place in *Venus* but what will have the four ſeaſons every year, and the heated places will have time to cool; becauſe, to any place over which the ſun paſſes vertically on any given day, he will, on the next day, be 26° from the *vertex* thereof, even tho' the place be on the tropic; and if it be on the equator, one day's declination will remove him $37\frac{1}{2}$° from it.

I having conſidered in general what the effects of the ſun's quick and great declination would be in *Venus*, as occaſioned by the great inclination of her *axis*, with her ſlow diurnal and quick annual motion; and finding that her globe in the Orrery, by being not quite an inch in diameter, was inſufficient for ſolving her *phænomena* to any degree of exactneſs; I took the following method, by which I could do it mechanically, to ſerve my purpoſe.

Along the middle of a ſtrait narrow ſlip of parchment I drew a black line, and then meaſuring my parchment round a common globe of 9 inches diameter, cutting it ſo as when the ends were a little overlapp'd, it would become a girdle, and ſtick faſt on any great circle of the globe. Having thus fitted it, I took it off; and laying it flat on a table, I divided one ſide of the black line into $9\frac{1}{4}$ equal parts for the $9\frac{1}{4}$ days in *Venus*'s year, and then I ſubdivided each day into 24^h or equal parts, of which the odd quarter contained 6, and ſet the proper figures to them. The other ſide of the line I divided into 12 equal parts or ſigns, and each ſign into 30°: by this means I could eaſily ſee, at every day and hour in *Venus*, in what place of the ecliptic the ſun was: and putting this girdle round the globe, at an angle of 75° to the equator, croſſing it in two oppoſite points, it would, by repreſenting *Venus*'s ecliptic drawn on her globe, ſerve for the ſolution of problems concerning her, as the ecliptic on our terreſtrial globe does for thoſe relating to our earth: for, by bringing the ſun's place, at any day or hour, to the braſen meridian, I had thereby his declination for that time; which gave me both an eaſy and ſure way for drawing the ſpiral of the ſun's motion over the body of *Venus* on this globe; and then, by elevating it to different latitudes, I could immediately ſee where the ſpirals cut the horizon in any lat. and at what height or declination they croſs'd the meridian; as by the hour-circle I could eaſily perceive the times of the ſun's riſing and ſetting, and his amplitudes on the horizon; and I called that the firſt meridian, which paſſed thro' the northern tropic, in the place where the ſun touch'd it at his greateſt N. declination; reckoning the E. or W. longitudes on the equator from that meridian. But this meridian will only ſerve for one year; becauſe, as the odd quarter of a day in *Venus*, cauſes the ſun to croſs her equator 90° weſtward of the former place every year, the place of the ſun's greateſt declination at the N. tropic will be in a meridian 90° weſtward of the former alſo. Things being thus premiſed in general: I now proceed to give

give as good a deſcription as I can of the particular *phænomena* in *Venus*, confining myſelf chiefly to what happens in her northern hemiſphere; knowing that the ſame muſt happen, *mutatis mutandis*, in the ſouthern.

1. Her *axis* is inclined $51\frac{1}{2}°$ more than the *axis* of our earth, and therefore the variation of her ſeaſons will be much greater than of ours.

2. Becauſe her N. pole inclines toward *aquarius*, and ours to *cancer*; her northern parts will have ſummer in the ſigns where thoſe of our earth have winter; and *vice verſa*.

3. The artificial day at each of her poles (containing $4\frac{5}{8}$ apparent diurnal revolutions of the ſun) will be equal to $112\frac{1}{2}$ natural days on our earth.

4. The ſun's greateſt declination, on each ſide of her equator, amounts to 75°: therefore her tropics are only 15° from her poles, and her polar circles at the ſame diſtance from her equator. Conſequently, her tropics are between her polar circles and poles, contrary to what thoſe on our earth are.

5. The ſun, in one apparent diurnal revolution from the equator, and any meridian where he croſſes it, to the ſame meridian again, changes his declination at leaſt 14° more on *Venus*, than on our earth from the equinox to the ſolſtice.

6. Let us now ſuppoſe an inhabitant ſtanding on her N. pole, where the ſun's declination is always the ſame with his altitude, and looking toward that point of the horizon where the firſt meridian (above mentioned) cuts it; and let him call that point the S. ſo ſhall he have a meridian fixt, which will determine the other cardinal points on the horizon; tho' ſtrictly ſpeaking, every point of the horizon to him is S.: yet, for once, let us ſuppoſe him to have an horizontal plane, fixed with it's S. point in this meridian, and thence divided and numbered like the horizon of a globe: put a moveable ruler with ſights to turn round the centre of this plane, for obſerving the ſun's amplitude at riſing and ſetting; and a graduated quadrant to be fixed in the N. and S. line, with a moveable index, for taking the ſun's altitude, in paſſing over the meridian. The ſame degree or part of a degree, that gives him the ſun's altitude, will alſo give him it's declination, and he will have the following *phænomena*.

The ſun will riſe $22\frac{1}{2}°$ N. of the E. and going on $112\frac{1}{2}°$, as meaſured on the horizontal plane, he will croſs the meridian at an altitude of $12\frac{1}{2}°$; then, making an entire revolution without ſetting, he will croſs it again at an altitude of $48\frac{1}{2}°$, at the next revolution he will croſs it as he culminates, at the height of 75°, being only 15° from the *zenith*; and thence he will deſcend in the like ſpiral manner, croſſing the meridian firſt at an altitude of $48\frac{1}{2}°$; then, at an altitude of $12\frac{1}{2}°$, and going on thence $112\frac{1}{2}°$ he will ſet $22\frac{1}{2}°$ N. of the W. having been 4 revolutions and $\frac{5}{8}$ parts of one above the horizon.

7. If the ſpectator turns his inſtrument $22\frac{1}{2}°$ toward the E. and then ſuppoſes his quadrant in the plane a new meridian to him; the ſun will then riſe due E. and ſet in the N. W.; and his declination in the meridian will not be the ſame as before; for he will firſt croſs it at an altitude of $10°$: next of 46; then, of $74\frac{6}{8}$; and, at $1\frac{1}{2}^{h}$ after, he will come to his greateſt declination; from which, in his deſcent, he will not croſs the meridian in the ſame degrees of altitude, as in aſcending he did.

8. Now, let the ſpectator turn his inſtrument $90°$ ſtill more toward the E. and the ſun will riſe due S.; and from thence making a complete revolution, he will croſs the meridian at an altitude of $37\frac{1}{2}°$; making another revolution, he will croſs it at an altitude of $70\frac{3}{4}°$; and, going on $7\frac{1}{2}^{h}$ (or $112°$) he comes to his greateſt declination in the W. N. W.: thence deſcending, at the end of the third revolution he croſſes the meridian $58\frac{1}{3}°$ high; at the end of the 4th he croſſes it in $23\frac{1}{3}°$ of alt. and, going on thence $225°$, or $\frac{5}{8}$ of a revolution, he ſets in the N. E.

9. If the ſpectator will now turn his inſtrument juſt half round, ſhifting his meridian $180°$, the ſun will riſe in the N.; and, going on $180°$, or $\frac{1}{2}$ a revolution, he will croſs the meridian at an alt. of $19°$; then, making a complete revolution, he will croſs it at an alt. of $55°$; and going on thence $292\frac{1}{2}°$ he comes to his greateſt declination in the E. S. E. from which place he deſcends, croſſing the meridian in $73\frac{1}{2}°$ of alt.; and, in the next rev. he croſſes the meridian at an alt. of $41\frac{1}{2}°$: at the fourth rev. he croſſes it at an alt. of $5°$; and going on thence $45°$, or $\frac{1}{8}$ of a rev. he ſets in the S. W.

10. The ſun being thus for half a year together above each pole of *Venus* in it's turn, will cauſe the whole year at her poles, as well as at the poles of our earth, to contain only one day and one night: but there, the difference between the heat in ſummer and cold in winter (or of mid-day and mid-night) is greater than betwixt the ſame on any two places of our earth; becauſe, in *Venus*, the ſun is for half a year together above the horizon of one or other of the poles; and for at leaſt $\frac{1}{3}$ of a rev. (or about 16 of our days) within $20°$ of the *zenith*; and during the other half of the year, always below the horizon; and for a conſiderable part of that time, at leaſt $70°$ from it: whereas at the poles of our earth, tho' the ſun is for half a year together above the horizon, yet his alt. is never more than $23\frac{1}{2}°$ above it in ſummer, nor his depreſſion greater than that quantity below it in winter. When the ſun is in the equator, he is ſeen in the horizon of both poles; $\frac{1}{2}$ of his diſc above, and the other below: and deſcending quite below the horizon of one pole, he aſcends in a viſible ſpiral above that of the other, until he comes within $16°$ of the *zenith*, where he keeps the ſame alt. nearly for ſome time; then deſcends in the like ſpiral manner, till he gets below the horizon, where he continues inviſible for the other half of the year. This will occaſion to each pole one ſpring, one harveſt, a ſummer as long

long as them both, and one winter, equal in length to the other 3 seasons.

The sun's great distance below the horizon of *Venus*'s poles, will make her winters much more uncomfortable than at the poles of our earth, where they have twilight more than half the winter-time; unless she be surrounded with an atmosphere capable of occasioning a twilight, at least as long in proportion to her winter, as our twilight is to ours. But this can hardly be supposed; because always, when we see *Venus*, she appears with the same constant serenity; and therefore I am apt to believe she has a satellite, to supply, in some measure, the absence of the sun; as our moon does to our earth's poles, for one half of the winter constantly, without setting, from the first to the third quarter. 'Tis true, that we are inconveniently posited, with regard to *Venus* for seeing her satellite (if she has one); because, when her moon or satellite has it's enlighten'd side toward us, it may be too far distant to be seen, because *Venus* is then beyond the sun, and, consequently, furthest from us; and when she is betwixt us and the sun, or there abouts, her full moon would have it's dark side to us: and tho' *Venus* be then nearest the earth, yet her satellite could no more be seen by us, than we can see our own moon at her conjunction. When *Venus* is at her greatest elongation, we should have only one half of the enlighten'd side of her full moon turn'd towards us; and even then, perhaps, on account of it's smallness, it may be too far distant to be seen by our telescopes. But of this only by-the-bye.

11. At the tropics, the sun in summer will continue for about 15 of our weeks together above the horizon without setting, and as long below it in winter without rising. While he is more than 15° from the equator, he neither sets to the inhabitants of the nearest tropic, nor sets to those of the other; whereas, at our terrestrial tropics, he rises and sets every day in the year. But to let us know more particularly the *phænomena* of *Venus*'s tropics, we will suppose the inhabitant, who has seen the abovemetion'd appearances at the N. pole to have travell'd thence along the first meridian 15° to the northern tropic, carrying his engine or instrument along with him; and to have set it due N. and S. in the place where the said meridian intersects the tropic; and as the meridian of every place is in a great circle passing thro' the *zenith* of the place and both poles, he can now be at no loss how to settle his meridian, and observe as well the amplitude and azimuth, as the alt. of the sun; who will rise to him 10° N. of the E. with about 1° of N. declination: and going on 100° (to be measured on the horizontal plane) he will cross the meridian with $12\frac{1}{2}$° of N. declination, and $27\frac{1}{2}$ of alt.; then, making an intire rev. without setting, he will cross the meridian at $48\frac{1}{2}$° of decl. and $63\frac{1}{2}$ of alt.: at the end of the next rev. he will cross the meridian in the *zenith* at the greatest decl.; namely, 75°; and thence he descends in the like spiral, crossing the meridian at the same alt. as above, till, in his fifth rev. he sets 10° N. of the W.

 12. Let

12. Let our traveller now remove weſtward on the ſame tropic, to a meridian $97\frac{1}{2}^\circ$ diſtant from the firſt; and there he will have very great differences of the riſing, ſetting, and meridian alt. of the ſun; which will now riſe to him the firſt time, in the S. point of his horizon, at 12^h; at 1^h he will be about half a degree above the horizon, and will ſet at 2^h: ſo this ſhort artificial day in *Venus* (which is ſomewhat longer than two natural days on our earth) will have no forenoon at all. The ſun, after continuing almoſt 14 of *Venus*'s hours below the horizon, ſuppoſing each diurnal rotation to be divided into 24^h, will riſe a little before 4^h next morning, near the N. E.; and, going on 130°, he will then croſs the meridian with 22° of N. decl. and 37 of alt.: then going on without ſetting, he again croſſes the meridian at 57° of decl. and 72 of alt.; and advancing forward thence $17\frac{1}{2}^h$, or $262\frac{1}{2}^\circ$, he comes to his greateſt decl. $7\frac{1}{2}^\circ$ to the N. of the E.: from thence, completing his rev. to the meridian, he now croſſes it in $71\frac{1}{2}^\circ$ of decl. being only $3\frac{1}{2}^\circ$ from the *zenith*: at the next rev. he croſſes the meridian with $38\frac{1}{2}^\circ$ decl. and $53\frac{1}{2}$ of alt.: at the next, which is the fourth rev. he croſſes the meridian with $1\frac{1}{2}^\circ$ of decl. and $16\frac{1}{2}^\circ$ of alt.; and then goes on 65°, and ſets near the W. S. W.

13. Suppoſe now that our traveller removes ſtill further weſtward, on the ſame tropic, to a meridian 105° diſtant from this his ſecond ſtation; and then the ſun will firſt riſe to him in the S. E. about 9^h; and going on thence 45° he will croſs the meridian with 6° of S. decl. and 9 of alt. at 12^h: about 2^h he will be 1° higher; and, thence deſcending, he will ſet near the N. W. a little before 9^h: ſo the afternoon of this day is almoſt 6^h (about 6 natural days with us) longer than the forenoon; and it's night is but little more than 3^h long: for the ſun, after going a little below the horizon, riſes in the N. point thereof; and, making half a rev. he croſſes the meridian with 33° of decl. and 48 of alt. thence, making a whole rev. he croſſes the meridian at 66° of decl. and 81 of alt.: at the next rev. his decl. is 63° (having paſſed the greateſt 14^h before): at the next, it is 28° of decl.; and, going on thence about 146°, he ſets N. W. by N. about half an hour after 9; and continues inviſible till 3 quarters paſt 5 in the next morning, when he riſes about 4° N. of the E.; and, going thence forward 94°, he croſſes the meridian about 5° alt. and 10 of S. decl. having kept the ſame alt. very nearly for 3^h; then deſcending, he ſets in the S. S. W. about half an hour paſt 1; which makes the afternoon 5^h and about $12'$ ſhorter than the forenoon of the ſame day. The ſun now ſets for about 15 of our weeks to *Venus*'s northern tropic, and riſes to the ſouthern; in which the *phænomena* are the ſame: each tropic having the 4 ſeaſons once every year; the winters being longer than the ſummers, tho' not quite ſo long, in proportion, as at the poles.

14. Having ſaid ſo much concerning the N. pole and tropic, proceed we now to ſtation our inhabitant in a place of 45° of N. lat. where the firſt meridian cuts the parallel, and he will have the following *phænomena*.

The ſun will riſe 43° E. of the S. a little before 9^h; and, aſcending very quickly, he will, in little more than 3^h, croſs the meridian at an alt.

alt. of 19°, with 26° of S. decl.; then going on 62°, he will set near the W. S. W. about 5^h in the afternoon; by which means it is almost 2^h longer than the forenoon; each hour in *Venus* being equal in length to 24^h 20′ of our terrestrial time. The next day the sun will rise 3° N. of the E. about half an hour past 5 in the morning, and will cross the meridian with 12½° of N. decl. and 57½ of alt.; and will set in the N. W. by W. about half an hour past 7: so that the afternoon will be 2^h longer than the forenoon. The next day the sun rises 53° N. of the E. about 3^h; and will cross the meridian 3½° N. of the *zenith*; or with 86½° of N. alt. and 48½ of decl.: then he goes round without setting; and crosses the meridian 30° N. of the *zenith*, where he comes to his greatest decl.; from which he returns in the like spiral toward the equator, and beyond it; but will not rise and set at the same hours as before: for, having made a rev. without setting, in the next he sets 53° N. of the W. about 9^h: next morning he rises in the N. E. by E. about half an hour past 4; crosses the meridian with 12½° of decl. and sets 3° N. of the W. about half an hour past 6; and now the forenoon is 2^h longer than the afternoon. The next day the sun rises about 7^h 62° E. of the S.; passes over the meridian at an alt. of 19°, with 26° of S. decl.; and sets a little after 3; which makes the forenoon to be about 2^h at least longer than the afternoon: and now the sun will continue below the horizon at least 12 of our weeks without rising to this inhabitant of *Venus*.

15. In this place of *Venus* the hour and amplitude of the sun's rising, for one half of the year, are the same with those of his setting in the other half; which will also happen in all places under the first meridian, where he rises and sets: but, if our Spectator pleases to remove along the parallel of 45° lat. eastward 142°, the *phænomena* of things will then be very different to him; for the sun from once rising in the N. E. by E. will pass over the meridian with 3½° of N. decl. and set due N.; which will make the afternoon somewhat above 4^h longer than the forenoon; and the next morning the sun will rise at 2^h 21½° E. of the N. or about the N. N. E. As to what would happen on the other days concerning the sun's rising, and setting, I shall not take any further notice of it; but, if the inhabitant will travel eastward 37½°, still upon the same parallel of lat. he will see the sun, at making his first appearance from the southern tropic, rise due S. at 12^h; and, getting about half a degree above the horizon, when he has gone forward about 9°, he will then descend, and set about a quarter after 1: so there is only 1^h and a quarter in the first day of the sun's appearance; and the 2d day will be 11^h long; but the 3d day will be about 87^h long; for the sun will make 3 rev. and somewhat more than an half without setting: the fourth day will be 11^h long; and the fifth will only contain 1^h and a quarter; for the sun will rise about 18° E. of the S. and set in the S. point of the horizon.

16. We

16. We will now suppose that the spectator has travelled from 45° of N. Lat. to the equator, and has a mind to take a tour round the same, because the *phænomena* will be very different in different parts thereof; though the sun will rise and set to every part of it, in every apparent revolution; but we shall only consider in general what happens at two places thereof: the first place shall be that, where the first meridian crosses the equator; and the second, a place 112½° W. of the first. To each of these places the sun will always rise at 6, and set at 6, though sometimes his meridian alt. may be 11° more or less than his midnight depression; and in other places the difference will amount to 15 or 16°; so that, if the diurnal and nocturnal spirals of the sun's motion on the body of this planet were measured, the one would very much exceed the other. To the first of these two places the sun will rise 74° S. of the E. in coming from the southern tropic, and set 61½ S. of the W. having been 22° high at mid-day, and will be 32½ depressed below the horizon at midnight. The next day he will rise 44° S. of the E. and set 26° S. of the W. having been 55° high at noon, and will be 74½ depressed at midnight. The third day he will rise 7½° S. of the E. and crossing the equator at half an hour after 10, he will, in 7½h after, set 12° N. of the W. and so proceed, changing his rising and setting amplitude every day, in advancing toward the northern tropic, till he reaches it; and then his setting amplitude, in going from it, will be the same as his rising amplitude in coming toward it. In the second place, all I shall take notice of, is, that the sun, in coming from the southern to the northern tropic, will cross the equator at 9 at night; and, in going from the northern to the southern tropic, he will cross the equator at mid-day.

17. At the equator the sun's rays will be as oblique, when his declination is greatest, as they are at *London*, when he touches the tropic of *Capricorn* in *December*; because the tropics of *Venus* are as far from each side of her equator, as the tropic of *Capricorn* is from the parallel of *London* on our earth, therefore, at *Venus*'s equator, there will be two winters, two springs, two summers, and two autumns, every year: and because the sun stays for some time near the tropics, and passes so quickly over the equator, every winter there will be about twice as long as summer: but, because of the quick return of summers, and the general heat on the body of *Venus*, the winters there will be very mild; and so will make the equator, and all places thereabouts, very temperate, and fit for habitation.

18. Those parts of *Venus* which lie between the poles and tropics, and between the tropics and polar circles, and also between the polar circles and equator, will more or less participate of the *phænomena* of these circles, as they are more or less distant from them.

19. The places of the equinoxes and solstices on the body of *Venus* go backward, or from E. toward the W. 90° every year. This is not occasioned by any mutation of her *axis* from its parallelism; but by the sun's being ¼ of a day later in crossing the equator every year, than on the year before; and therefore he will cross it in a place 90° westward

of

of the former every year: ſo that to any place where he croſſes the equator at noon, he will, on the return of that day at noon in the next year be almoſt 10° S. of the equator, and will croſs it at 6 in the evening; ſuppoſing the year to begin when the ſun is on the equator, in paſſing from the ſouthern tropic to the northern. Hence, though the ſpiral, in which the ſun's apparent motion is performed, be of the ſame ſort every year, yet it will not be the very ſame; becauſe the ſun will paſs vertically over all the ſame places but once in every 4 years: and, in the above deſcription, I have only ſhewn what will happen in general, for 1 year, having only drawn the ſpiral of the ſun's motion for that time: and if a ſpectator, on any parallel of latitude, ſhould want to ſee the ſame appearances of the ſun's riſing and ſetting every year, and, conſequently to have the particular days thereof to be ſtill of the ſame length with thoſe of the year, he muſt travel weſtward every year 90° on the ſame parallel.

20. The inhabitants of *Venus* will be very careful in adding a day to ſome particular part of every 4th year, to keep ſtill the ſame ſeaſons to the ſame times; becauſe, as the great annual change of the equinoxes and ſolſtices will ſhift the ſeaſons forward $\frac{1}{4}$ of a day every year, they would, in 36 years, ſhift the ſeaſons forward through all the days of the year: But, by this intercalary day, every 4th year will be a leap-year; which will bring her time to an even reckoning, and keep her Kalendar right.

21. The great change of the ſun's declination every day, which cauſes his altitude, at noon, or any other hour, and his amplitude at riſing and ſetting, to be ſo very different in places lying under the ſame parallels of lat. will be of one ſingular uſe in *Venus*, the like whereof we ſhall never enjoy on the earth; and that is no leſs than the giving a ſure and eaſy method of finding the longitude. For, ſuppoſe to one place, at noon, the ſun's declination is 30°, and to another place, it is only 20° 35′ at noon, in the ſame revolutional ſpiral, going from the equator toward the northern tropic; the difference of theſe two declinations is 9° 25′: in the ſame ſpiral from the equator, where any meridian croſſes it, to the ſame meridian again, the declination changes from 0 to 37° 21′; and the ſun has gone 38° 55′ in the ecliptic. Theſe things being known, the proportion will be thus; As 75°, the greateſt declination, is to to the ſun's motion in that time, which is 3 ſigns, equal to $2\frac{5}{16}$ revolutions round *Venus*; ſo is 9° 25′ (the difference of declination at two given places) to 9° 44′, which is $\frac{1}{4}$ part of a revolution; and therefore the one place is $\frac{1}{4}$ part of a circle, or 90° of long. diſtant from the other: and, as the decl. was advancing from the equator toward the northern tropic, the place, in whoſe meridian it was 20° 35′ is eaſtward from the place in whoſe meridian it was 30°, ſuppoſing them both to be in the northern hemiſphere.

I ſhould be very glad to ſee this deſcription examined into, and put in a better form, by ſome whoſe abilities are much greater than mine: And although it ſeems ſtrange, at the firſt view, that the great inclination of *Venus*'s

Venus's axis, with her slow diurnal, and quick annual motion, should make such mighty differences of her *phænomena* from the earth's; yet I verily believe, that, was the spiral of the sun's motion for four years, which would contain 37 revolutions, nicely drawn on a large globe, and the times mentioned in which the sun would rise and set, with his different amplitudes, altitudes, and declinations, where the effects thereof would differ considerably in many particular parts of each spiral; and so occasion remarkable differences of the lengths of day and night, in the same revolutions, to places under the same parallels of latitude; a whole volume might be written in the description, if the author would descend to particulars.

A letter from John Bevis, *M. D. to* Martin Folkes, *Esq;* *P. R. S. &c.* *containing some* Observations *concerning* Mercury. No. 473. p. 48. May &c. 1744. *Presented* May 4. 1744.

XIV. The observations to which the inclosed computations are made by Mr *Morriss* from somewhat more correct elements than those in Dr *Halley*'s tables, were carefully taken by myself, with an excellent astronomical sector of 5 feet *radius*. You will perceive how far I am limited, by my friend's request; so must intreat you, if you think it worth while, to inform the *R. Soc.* that *Mercury*'s motion has not been at all disturb'd [by the late *Comet*].

	° ′ ″
1744, *May* 17. The apparent right ascension of ε *Geminorum*, according to Dr *Bradley*'s observations - - -	97 2 10
Declination - - - - - - - - - - - - - - - -	25 21 30

May.	Appt. time. h ′ ″	Long. ☿. ° ′ ″	Latitude. ° ′ ″	Comp. Rt. Asc. ° ′ ″
15	8 31 15	♊ 28 56 51	1 57 58 N.	88 50 7
17	8 26 6	♋ 1 1 16	1 44 45	91 7 41
18	9 4 40	1 59 30	1 36 40	92 11 53
19	8 41 00	2 51 38	1 28 7	93 9 12

May.	Comp. Declin. ° ′ ″	Obsd. Rt. Asc. ° ′ ″	Obsd Declin. ° ′ ″	Error Comput. Rt. Asc. ″	Error Comput. Declin. ″
15	25 26 12	88 49 20	25 26 20	+ 47	— 8
17	25 13 00	91 7 4	25 12 56	+ 37	+ 4
18	25 4 15	92 11 10	25 4 27	+ 43	— 12
19	24 54 43	93 8 20	24 54 56	+ 52	— 13

Transit of Mercury *over the* Sun, Nov. 5, 1743. *at the Observatory at* Giesen; *by* Christian Lewis Gersten, Math. *Prof. and F. R. S.* No. 482. p. 376. Jan. &c. 1747. *Read* Jan. 22, 1746-7.

XV. In the observation of this transit, I chiefly made use of 3 instruments; the first of which was a good telescope of 10 feet, to which I fitted an excellent micrometer made at *London*. The second was an oscillatory astronomical clock, made by Mr *Ellicot*, which the experience of many years gave me reason to depend upon. The third was of my own invention, and served to discover the true time of noon, instead of an astronomical quadrant, with which I was not provided.

A little

A little after 8^h in the morning, the clouds, which had totally obscured the heavens, began to break unexpectedly; and in a short space of time the sun began to shine clear through the opening. I applied my telescope immediately; but not being able to see any thing of *Mercury* or of any spot whatsoever, I endeavoured to take the horizontal diameter of the sun by repeated observations, and though the rapidity of the motion made it not easy to do this, yet I thought I had pretty justly found the semidiameter to be $21\,\frac{22}{72}$ rev. of the micrometer. I afterwards found the vertical semidiameter at about $11^h\,20'$ exactly $21\,\frac{18}{72}$ rev. How much these numbers make in the parts of a great circle will be shewn below.

When I had taken the horizontal semidiameter of the sun, it was hid again by very thick clouds; but at $9^h\,6'\,25''$ on a sudden I saw *Mercury* on it's disk, being wholly entered, if I rightly remember, but yet adhering to the edge. But going to look at the clock, in the absence of my assistant, on my return, I found the sun covered with clouds; so that I dare not affirm what was the exact time of the contact.

The following observations were made in the intervals of the clouds. I was favoured by the calm state of the air, and by the absence of many spectators. The body of ☿ appeared round and black with a determinate edge, and without any signs of an atmosphere, but so minute, as to appear to the naked eye not above twice as thick as the hair of a micrometer. About $1^h\,10'$ *p. m.* till the egression, the clouds were very distinct; but by that time the sun caused such an undulation of the limb as I could not by any means remove.

The 1st col. of the following table shews the time by the clock. The 2d, the true corrected time. The 3d, the spaces of time from the appulse of the limbs of the sun to the appulse of ☿ to the horary thread, reduced into seconds of a great circle for the declination of the sun $15^\circ\,39'\,18''$. The 4th, the observations. The 5th, the distances of ☿ from the lower limb of ☉ in parts of the micrometer. The 6th, the parts of the micrometer reduced to seconds of a great circle. The basis of the reduction is; 23 entire rev. give $17'\,33''\frac{1}{2}$ which I found to be such by the transit of ☉ and some of the fixed stars.

Time by the clock before noon.	True time corrected.	Dist. ☿ in R.A.	Observations.	Rev. Micr.	Dist. ☿ in Decl.
h ′ ″	h ′ ″				
9 6 25	9 5 55	00	1. I saw ☿ wholly entered or certainly the greatest part.		
			2.		″
9 44 7	9 43 37	694	☿ to the horary.	$5\frac{11}{72}$	237
0 0 55	0 44 25		Following limb of ⊙ to the horary.		
			3.		
9 49 5	9 48 35	722	☿ to the horary.	$5\frac{31}{72}$	251
0 0 55	0 49 25		Following limb of ⊙ to the horary.		
			4.		
9 56 30	9 56 0	766	☿ to the horary.	$5\frac{66}{72}$	271
0 57 23	0 0 53		Following limb of ⊙ to the horary observed thro' a thin cloud.		
			5.		
10 2 43	10 2 13	794	☿ to the horary.	$6\frac{21}{72}$	288
0 3 38	0 3 8		Following limb of ⊙ to the horary.		
			6.		
10 44 35	10 44 5	996	☿ to the horary.	$8\frac{42}{72}$	393
0 45 44	0 45 14		Following limb of ⊙ to the horary.		
			7.		
10 48 14	10 47 44	1028	☿ to the horary.	$8\frac{60}{72}$	404
0 49 25	0 48 55		Following limb of ⊙ to the horary.		
			8.		
10 53 26	10 52 55	1054	☿ to the horary.	$9\frac{9}{72}$	418
0 54 39	0 54 9		Following limb of ⊙ to the horary		
			9.		
11 33 52	11 33 22	1272	☿ to the horary.	$11\frac{30}{72}$	522
0 35 20	0 34 50		Following limb of ⊙ to the horary.		
			10.		
11 40 54	11 40 24	1310	☿ to the horary.	$11\frac{54}{72}$	538
0 42 25	0 41 55		Following limb of ⊙ to the horary.		
			11.		
11 43 52	11 43 22	621	Preceding limb of ⊙ to the horary.		
0 44 35	0 44 5		☿ to the horary.	$12\frac{6}{72}$	553
0 46 7	0 45 37	1329	Following limb of ⊙ to the horary.		
A. M.	P. M.		12.		
11 57 42	11 57 12	549	Preceding limb of ⊙ to the horary.		
0 58 20	0 57 50		☿ to the horary.	$12\frac{49}{72}$	580
0 59 57	0 59 27	1401	Following limb of ⊙ to the horary.		
P. M.			13.		
12 3 27	12 2 57	520	Preceding limb of ⊙ to the horary.		
0 4 3	0 3 33		☿ to the horary.	$13\frac{8}{72}$	600
0 5 42	0 5 12	1430	Following limb of ⊙ to the horary.		
			14.		
12 46 56	12 46 26	289	Preceding limb of ⊙ to the horary.		
0 47 16	0 46 46		☿ to the horary.	$15\frac{44}{72}$	715
0 49 11	0 48 41	1661	Following limb of ⊙ to the horary.		

Time

Time by the clock after noon.	True time corrected.	Dist. ☿ in R.A.	Observations.	Rev. Micr.	Dist. ☿ in Decl.
h ′ ″	h ′ ″		15.		″
1 16 56	1 16 28		Preceding limb of ⊙ to the horary.		
0 17 8	0 0 38	144	☿ to the horary.	$17\frac{16}{72}$	788
			The limb of ⊙ began to tremble.		
			16.		
1 19 27	1 18 57		Preceding limb of ⊙ to the horary.		
0 0 35	0 19 5		☿ to the horary.	$17\frac{24}{72}$	793
0 21 41	0 21 11		Following limb of ⊙ to the horary.		
			17.		
1 36 15	1 35 45		It seemed to touch the inner edge,		
0 0 20	0 35 50		it certainly either touched it or went		
0 38 19	0 37 49		a little beyond it, the great undulation and trembling of the limb quite went off.		
			Obs. 16.		
			On account of the trepidation of the solar limb, I think it should be thus corrected according to the analogy of the rest.		
1 19 $26\frac{1}{2}$	1 18 $56\frac{1}{2}$	122	Preceding limb of ⊙ to the horary.		
0 0 35	0 19 5	1828	☿ to the horary.	$17\frac{24}{72}$	793;
0 21 $41\frac{1}{2}$	0 21 $11\frac{1}{2}$		Following limb of ⊙ to the horary.		

I now proceed to the corollaries to be drawn from these observations. In the first place, the diameter of ⊙ is to be determined: in order to this, we must have it's decl. and alt. at the time when the vertical diameter was measured. The decl. of ⊙ is easily computed from it's longitude. I found it's long. by the *Ludovician* tables at 11^h 20′ 39″ true time (near the middle of it's transit) for merid. 25′ 10″ of time E. of *Paris* to be ♏ 12° 37′ 37″. To this long. answers S. decl. 15° 39′ 18″. The distance of time from the appulse of the preceding limb of ⊙ to the appulse of the following, by obs. 11, 12, 13 and 14, is 2′ 15″, which time being converted into arcs of the equator, gives 33′ 45″. Therefore, if this arc for the decl. be reduced according to the rules of the spherical doctrine into parts of a great circle, the diameter of the sun will be 32′ 30″.

By the astron. obs. of *Philip* of *Butisbach*, *Landgrave* of *Hesse*, the latitude of the city of *Butisbach*, which is not above 4 hours journey distant from *Giesen*, is 50° 28′. Wherefore I take 50° 30′ for the latitude of *Giesen*. Hence the alt. of ⊙ when it's vertical diameter was measured, is between the 23d and 24th degree more or less. The semidiameter of ⊙ in parts of the micrometer was 21 $\frac{18}{72}$ rev. which according to my table, is = 16′ 13″ of a great circle. Hence, the vertical diameter at that time 32′ 26″. But becauſe of the refraction it ought to appear leſs than the truth, and by *de la Hire*, tab. v. that defect is 4″. This being added, we have again 32′ 30″. But if we make uſe of a newer table of refractions conſtructed from *Taylor*'s *Hypotheſes*, which *Halley* * publiſhed, and preferred before the reſt, the defect will be leſs only by ſome thirds.

I found the horizontal ſemidiameter, as I ſaid above, to be in parts of the microm. 21 $\frac{22}{72}$ rev. the duple of this quantity gives according to my table, 32′ 31″ of a great circle. Therefore theſe 3 obſervations agree very well together, and make the diam. of ☉ 32′ 30″.

I proceed now to the angle ſeen of the apparent path of ☿ with the ecliptick. I followed the method of *Manfredi* in the tranſit of 1736 †, which alſo I have made uſe of. I drew a ſcale with great exactneſs, and found, that if the mean place is ſought between the places of obſ. 15 and 16 arithmetically corrected, and then through this, and alſo through that, which obſ. 5 determines, a right line be drawn; that it ſhews it's true apparent path in the diſk of ☉ as near as poſſible. This principle being laid down, I applied the numbers. The mean diſt. between obſ. 15 and 16 corr. from the following limb of ☉ is 1817″ of a great circle. The mean diſt of ☿ between the ſame obſervations from the inferior limb of ☉ 790 $\frac{1}{2}$″. The diſt. of the place of obſ. 5, from the following limb, is 794″. Diſt. from the inferior limb, 288″. Theſe differences therefore form a rectangular triangle, the firſt of which is the baſe, and the other the *cathetus*. The calculus being made, the angle at the baſe is 26° 9′, to which the angle of the path, with the circle parallel to the equator, is equal. After the ſame manner I ſought the angles of the ſeveral places; beginning with obſ. 7, with the place of obſ. 5, and they came out as follows.

		°	′
By obſ.	5 and 7 the angle is	26	33
	5 and 8 - - -	26	33
	5 and 9 - - -	26	5
	5 and 10 - - -	26	51
	5 and 11 - - -	26	21
	5 and 12 - - -	25	41
	5 and 13 - - -	26	7
	5 and 14 - - -	26	13
	5 and 15 - - -	26	17
	Mean	26	11

Wherefore, when in the former caſe, the angle at the baſe is 26° 9′, and in this, 26° 11′, I take the mean 26° 10′ for the angle ſeen of the

* See Vol. VI. p. 167. where Mr *Eames* has by miſtake omitted in the margin the name of Dr *Halley*, who was author of the paper.

The table here mentioned was not Dr *Brook Taylor*'s; but Sir *I. Newton*'s.

† See Vol. VI. p. 195.

path

path with the parallel circle. Hence, the angle of the apparent path with the horary, 116° 10′. But to the place of ☉, 12° 37′. ♏ answers by *de la Hire*'s tab. to the angle of the ecliptick with the merid. 107° 43′. Therefore the angle of the apparent path of ☿ with the ecliptick, is 8° 26′.

For the least distance of the centres, I chose two observations, the middle way between which was shewn by the type to be taken, nor were they much distant from the very path, namely the 7th and 10th. From the distances of ☿ from the inferior limb, I subtracted 8″ allowing 5½″ for the semidiameter of ☿ *, and the rest for ½ the thickness of the parallel thread: for the distances are to be taken from the centre of the tube, not from the edge of the thread. And then from the distance of ☿ from the following limb in obs. 7. Semid. ☉, the angle of the path with the parallel circle being found, I discovered, by the analysis of the triangles, the last distance of the path, or of the centres of ☉ and ☿ to be 9′ 2″. By obs. 10 the distance is 9′ 7″; therefore I take the mean 9′ 4½″ for the true dist. of the path from the centre of ☉. From these premises, I drew the following conclusions by a trigonometrical calculation.

	′	″
Long. of path seen in the disk of ☉ - - - - - - - - -	26	57
Lat. seen of ☿ in conjunction - - - - - - - - - - -	9	10
Lat. of ☿ in the ingress - - - - - - - - - - - -	10	57
Lat. of ☿ in the egress - - - - - - - - - - - - -	6	59
Diff. between lat. in egress and ingress - - - - - - -	3	58
Portion of the path between ☌ and middle of the transit -	1	20

The time of the conjunction, the position of the node, and the inclination of the orbit, cannot hence be immediately discovered; for there is still required an exact determination of the stay of the centre of ☿ on the disk of ☉, which I cannot safely determine from my observations. But by comparing the intervals of the times with the distances of many places in the path, I found the horary motion to be about 5′ 56″, and therefore that the whole stay of the centre of ☿ on the disk, is pretty near 4ʰ 33′. And as an error of 1′ or 2′ of time in this case, makes but a small difference in the lower node, in the inclination of the orbit, I shall briefly set down what is produced by this hypothetical calculus. And because, by probable reasoning, the trepidation of the limb anticipated the contact of ☿ with the inner edge, and consequently the egress, let us set down

	h	′	″
The true time of the egress of the centre of ☿ on the disk of ☉ at *Giesen* - - - - - - - - - - - - - - - -	1	37	0
Half the stay on the disk - - - - - - - - - - - -	2	16	30
The middle of the transit will be - - - - *Nov.* 4.	23	20	30

* Dr *Bradley* determined the diam. of ☿ to be 10″ 45‴, by a microm. applied to the *Hugenian* telesc. of above 120 foot long. See Vol. VIII. p. 254.

By

	h ′ ″
By the horary motion and portion of the path, between ☌ and middle, the time of the tranſit through that portion will be - - - - - - - - - - - - - - - -	0 13 28
Therefore true time of ☌ at *Gieſen* - - - - - *Nov.* 4.	23 7 2
Let us therefore lay down the diſt. of merid. between *Gieſen* and the Obſ. at *Paris*, rejecting ſeconds - - - - -	0 25 0
The true time of ☌ at the Obſervatory at *Paris* will be -	22 42 2
Equation of time by the *Ludovician* tables - - - - -	0 20 24
Mean time of ☌ at the Obſervatory at *Paris* - - - - -	22 21 38
	° ′ ″
To this time the place of the ſun by the *Ludovician* tables- - - - - - - - - - - - - - - - - - ♏	12 37 0
	h ′ ″
By the diff. between the lat. in the ingreſs, egreſs, and ☌, and by the ſtay of the centre on the diſk $4^h\ 33'$ the time reſults which ☿ finiſhes from ☌ to ☊ - - - - - - - -	10 31 25
	° ′ ″
By tab. *Ludovic.* from which the *Caroline* here ſcarcely differ, in this ſpace of time ☿ proceeds heliocentrically in the ecliptick - - - - - - - - - - - - - - - - -	2 39 13
Therefore place of the node by theſe hypotheſes - - ♉	15 16 13
But if the ſtay of the centre of ☿ be ſuppoſed $4^h\ 32'$ then ☊ will be - - - - - - - - - - - - - - - - ♉	15 15 38
But if the ſtay of the centre of ☿ be ſuppoſed to be $4^h\ 34'$, then ☊ will be - - - - - - - - - - - - - ♉	15 16 47
	h ′ ″
But if we ſuppoſe the diſt. of ☿ from the earth, to be as 676 to 313, as the Great *Halley* defines it *, then the inclination of ☿ in ☌ will be - - - - - - - - - - - -	0 19 47
From this arc and the diſt of ☿ in the ecliptick from ☊ follows at length the inclination of the orbit, and in the firſt caſe. where the ſtay of the centre is ſuppoſed to be $4^h\ 33'$ -	0 7 5
But if the ſtay of the centre of ☿ on the diſk is ſuppoſed to be $4^h\ 32'$, the inclination of the orbit will be - - - -	0 7 6
But if $4^h\ 34'$ - - - - - - - - - - - - - - - -	0 7 5

Occultation of Cor Leonis *by the* Moon, *on* Thurſday, March 12. 1747, *in* Surrey ſtreet *in the* Strand, London, *with a reflecting teleſcope, made by Mr* Short, *F. R.S. which magni-*

XVI. Apparent Time.

	d	h	′	″	
1747, *Mar.*	12	8	24	19	The ſtar immerg'd into the dark limb.
		9	27	4	It emerg'd from the enlighten'd limb a ſmall matter to the W. of the moon's zenith.
			44	$4\frac{1}{2}$	The moon's preceding limb paſs'd the meridian in the tranſitory.
			44	21	The ſtar paſſed the meridian.

Mr *Short*, another gentleman, and myſelf, agreed to a ſingle ſecond in the immerſion, with different teleſcopes; but I ſaw and pronounced

* See Vol VI. p. 253.

the

the emerſion 2 or 3″ before them.—— There had been an exact obſervation of the ſun's tranſit at noon; and the clock gain'd about $\frac{1}{2}$ a ſecond a day.

We reckon *Surrey-Street* 27″ in time W. of the R. Obſ. at *Greenwich.*

Mr *John Catlin* had a few days before deliver'd me a computation of this eclipſe, corrected from two places of the moon, obſerv'd the 28th of *February* and the 2d of *March* 1729, correſponding pretty nearly with her preſent ſituation; as likewiſe from the ſtar's poſition, as I had rectify'd it from ſeveral late obſervations; and this gave the

	h	′
Immerſion at - - - - - - - - - - - - - - - - -	8	$26\frac{1}{2}$
Emerſion - - - - - - - - - - - - - - - - - -	9	30

fied about 100 *times; communicated to the* Royal Society *by* J. Bevis, *M. D.* N° 483. p. 455. March &c. 1747. *Read* March 19. 1746-7.

XVII. The comet which appeared towards the end of laſt *Dec.* and in the following months *Jan.* and *Feb.* 1744. was firſt ſeen in *England*, at the Obſervatory of the Earl of *Macclesfield*, *Dec.* 23. between 5 and 6 in the evening. It formed, at that time, an obtuſe-angled triangle, with α of *andromedæ*, and γ *pegaſi*, the comet being at the obtuſe angle; and it's paſſage over the meridian was obſerved at $5^h\ 32'$, mean *Oxford* time. His Lordſhip's obſerver could not then take it's diſtance from the *vertex* accurately, the comet's *tranſit* being unexpected; however, by an obſervation made at *Paris* the ſame evening by Mr *Monnier* we have the diſtance very nearly.

A letter from the Rev. Mr Joſ. Betts, *M. A. Fellow of* Univerſ. Coll. Oxon. *to* M. Folkes, *Eſq; Pr. R. S. containing obſervations on the late* Comet, *made at* Sherborn *and* Oxford; *with the* Elements *for computing its motions.* N° 474. p. 91. June &c. 1744. *Read* June 14. 1744.

His Lordſhip the next day acquainting the Rev. Mr Profeſſor *Bliſs* with this diſcovery, gave us an opportunity of looking after it at *Oxford*; but, unfortunately, bad weather, and a continued ſucceſſion of cloudy evenings prevented our obſerving it, till *Dec.* 31. but the weather proving more favourable at *Sherborn*, it's R. aſc. and decl. were taken by his Lordſhip, the reſult of which obſervations is as follows:

Note, That the equal time is made uſe of in the following obſervations, and that the comet's *tranſits* (reduced to the meridian of *Oxford*) are only given to the neareſt half minute, as being ſufficient for computing it's places.

Dec. $23^d\ 5^h\ 32'$) the R. aſc. of the comet by the *tranſit* inſtrument at *Sherborn* was found to be $5^\circ\ 48'\ 1''$; and it's polar diſtance by Mr *Monnier* $68^\circ\ 18'\ 35''$.

Dec. $27^d\ 5^h\ 7'\frac{1}{2}$) the R. aſc. of the comet, obſerved at *Sherborn*, was $3^\circ\ 41'\ 7''$; and it's decl. $21^\circ\ 7'\ 13''$ N.

Dec. $28^d\ 5^h\ 1'\frac{1}{2}$) the obſerved R. aſc. of the comet was $3^\circ\ 11'\ 8''$; and it's diſtance from the pole $69^\circ\ 0'\ 38''$.

Dec. $31^d\ 4^h\ 44'$) the R. aſc. of the comet, by the *tranſit* inſtrument, was found to be $1^\circ\ 44'\ 40''$; and it's decl. $20^\circ\ 36'\ 37''$ N.

The ſame evening, at $5^h\ 53'$ the ſky favouring us at *Oxford*, the diſtance of the comet from *aldebaran*, taken with *Hadley*'s quadrant, was

$60^\circ\ 10'$

60° 10′, corrected for refraction, 60° 11′; from γ *pegasi* 7° 2′½; corrected, 7° 2′ 40″.

Jan. 12^d 9^h 10′ the comet followed φ *pegasi*, in a 5 foot glass, 1° 43′ 32″ of R. asc.; and was more northerly than the star 1° 36′ 00″: the R. asc. of the star, by the *Greenwich* observations at that time, was 354° 52′ 12″, it's decl. 17° 41′ 55″: therefore the comet's R. asc. was 356° 35′ 44″, and it's decl. 19° 17′ 55″.

Jan. 13^d 6^h 30′) the distance of the comet from *aldebaran*, at a *medium* of several trials by the quadrant, was 65° 26′ 50″; corrected for refraction 65° 28′ 10″; it's distance from γ *pegasi* 6° 31′½; corrected, 6° 31′ 45″. - - -

At 8^h 20′ the comet followed φ *pegasi* 1° 21′ 13″ of R. asc.; and was more northerly than the star 1° 30′ 33″. Hence the comet's R. asc. was 356° 13′ 25″; and it's decl. 19° 12′ 28″ N.

Jan. 16^d at 6^h 33″ the comet's distance was observed by the quadrant from *aldebaran* 66° 36½′; corrected for refraction 66° 38′ 10″; from γ *pegasi* 7° 0′¼; corrected 7° 1′.

At 8^h the same evening) the comet followed φ *pegasi* in the 5 foot glass 10′ 24″ of R. asc.; and was more northerly than the star 1° 13′ 24″. Hence the comet's R. asc. was 355° 2′ 36″; and it's decl. 18° 55′ 19″ N.

Jan. 23^d 6^h 11′ the comet's distance was observed by the quadrant from *aldebaran* 69° 26½′; corrected for refraction 69° 28′ 5″; from γ *pegasi* 8° 42½′; corrected 8° 42′ 35″.

January 23^d 7^h 29′ the comet preceded φ *pegasi* 2° 43′ 27″ in R. asc.; and was N. of the star, in the 8 foot glass, 26′ 32″. Hence the comet's R. asc. was 352° 8′ 46″; and it's decl. 18° 8′ 27″.

The comet this evening appeared exceedingly bright and distinct, and the diameter of it's *nucleus* nearly equal to that of *Jupiter*'s; it's tail, extending above 16° from it's body, pointed towards ζ of *andromeda*; and was in length, supposing the sun's parallax 10″ above 23 millions of miles; but cloudy weather succeeding, we lost this agreeable sight till *Feb.* 5^{th}.

Feb. 5^d 7^h 31½′ a small star of *pegasus*, marked α by *Bayer*, preceded the comet in R. asc. 1° 40′ 20″; and was S. of the star 54′ 23″: the R. asc. of the star, by the *Greenwich* observations at that time, was 343° 0′ 4″; it's decl. 13° 49′ 56″: wherefore the comet's R. asc. was 344° 40′ 24″; and it's decl. 14° 44′ 19″ N.

Feb. 11^d 6^h 37½′ the comet followed ξ *pegasi*; the correction for refraction being allowed 43′ 1″ in R. asc.; and was S. of the star 50′ 3″: the R. asc. of ξ, by the *Greenwich* observations at that time, was 338° 28′ 24″; it's decl. 10° 51′ 3″: therefore the comet's R. asc. was 339° 11′ 25″; and it's decl. 10° 1′ N.

Feb. 12^d 6^h 33′ the comet followed ζ *pegasi* 56′ 45″ of R. asc.; and was more southerly than the star 44′ 42″. The R. asc. of ζ, by the *Greenwich* observations at that time, was 337° 10′ 15″; it's polar distance

distance 80° 29′ 53″. Hence the comet's R. asc. was 338° 7′ 00″; and it's decl. 8° 45′ 25″ N.

Feb. 13ᵈ 6ʰ 25′ the comet preceded ρ *pegasi* 7° 41′ 31″ in R. asc.; and was more southerly than the star 1′ 13″: the R. asc. of the star, at that time, was 344° 41′ 55″; it's polar distance 82° 40′: whence the R. asc. of the comet was 337° 0′ 24″; and it's decl. 7° 18′ 47″ N.

This was the last observation made at *Oxford*; the comet being now so near the sun, and withal so low in the evening, that the great difficulty of finding any star to compare it with, made us desist from attempting it again; however, the prodigious brightness it acquired, by it's near approach to the sun, made it visible in the day-time. And at *Sherborn*,

Feb. 16ᵈ 23ʰ 42½′ it's R. asc. by the *transit* instrument, was found to be 333° 13′ 53″; and it's decl. 0° 2′ 40″ S.

Feb. 17ᵈ 23ʰ 36′ the R. asc. was observed 332° 33′ 20″; and it's decl. 2° 29′ 00″.

By the help of these observations, which were made by the Rev. Mr Professor *Bliss* (the *transits* excepted taken at *Sherborn*), I was enabled, by the method delivered in the third book of the *Principia*, to determine the comet's parabolic trajectory; and found the place of the ascending node to be in ♉. 15° 45′ 20″; the logarithm of the *perihelion* distance 9,346472: the logarithm of the diurnal motion 0,940420: the place of the *perihelion* ♎. 17° 12′ 55″; the distance of the *perihelion* from the node 151° 27′ 35″: the logarithm, sine, and co-sine of the inclination of the orbit to the ecliptic 9,865138, 9,832616: and thence the time the comet was in the *vertex* of the *parabola*, or the time of the *perihelion*, *Feb.* 19ᵈ 8ʰ 12′: the motion of the comet, in it's orbit thus situated, was direct, or according to the order of the signs.

From these elements, by the help of Dr *Halley*'s general table (to which they are adapted), I computed the comet's places for the times of observation, exhibited in the following table: to which are added the comet's longitudes and lat. deduced from the observed R. ascensions and declinations together with the errors between the observed and computed places; the observations being all reduced to *Oxford* mean time.

Equal time at *Oxford*.	D	H	′	Longit. Comet observed. ° ′ ″	North Latit. observed. ° ′ ″	Longit. Comet computed. ° ′ ″	North Latit. computed. ° ′ ″	Diff. in Long. ″	Diff. in Latit. ″
Dec. 1743.	23	5	32	♈. 14 10 2	17 33 11	♈. 14 10 3	17 33 37	1—	26—
	27	5	7½	♈. 12 2 25	17 51 29	♈. 12 2 26	17 51 47	1—	18—
	28	5	1½	♈. 11 32 11	17 55 54	♈. 11 32 14	17 56 8	3—	14—
	31	4	44	♈. 10 4 57	18 9 3	♈. 10 5 16	18 8 53	19—	10+
		5	53	♈. 10 4 11	18 9 37	♈. 10 3 55	18 9 6	16+	31+
Jan. 1743.	12	9	10	♈. 4 52 5	18 59 37	♈. 4 52 24	18 59 13	19—	24+
	13	6	20	♈. 4 31 40	19 2 31	♈. 4 31 13	19 2 49	27+	18—
		8	20	♈. 4 29 27	19 3 32	♈. 4 26 6	19 3 12	21+	20+
	16	6	33	♈. 3 18 43	19 15 47	♈. 3 18 27	19 15 13	16+	34+
		8	00	♈. 3 17 31	19 16 7	♈. 3 17 00	19 15 30	31+	37+
	23	6	11	♈. 0 19 45	19 42 30	♈. 0 19 16	19 42 1	29+	29+
	23	7	29	♈. 0 17 58	19 42 47	♈. 0 17 45	19 42 12	13+	35+
Feb. 1744.	5	7	31½	♓. 21 52 37	19 35 00	♓. 21 52 56	19 34 42	19—	18+
	11	6	37½	♓. 14 42 45	17 23 30	♓. 14 42 58	17 24 5	13—	35—
	12	6	33	♓. 13 10 36	16 38 40	♓. 13 10 52	16 39 17	16—	37—
	13	6	25	♓. 11 32 50	15 43 45	♓. 11 33 16	15 44 16	26—	31—
	16	23	41½	♓. 5 9 14	10 17 40	♓. 5 9 1	10 18 8	13+	28—
	17	23	35	♓. 3 37 37	8 15 39	♓. 3 37 11	8 16 3	26+	24—

Perhaps

Perhaps it may not be thought foreign to my purpose to remark, that the nodes of the comet, and the planet *Mercury*, are situated within less than half a degree of each other; which, I suppose, gave rise to a report, that the comet had carried *Mercury* from it's orbit. In order therefore to find how nearly they approached each other, I had the curiosity to bring the matter to calculation; and presently found, there was above a week's difference in the times of their coming to the nodes; the comet passing it's descending node, *Feb.* 22. about 2^h in the morning; and *Mercury* not coming to his till *Feb.* 29. the comet moving all that time southwards with a prodigious velocity. Again, computing their heliocentric conjunction, which happened *Feb.* 18. about 1^h in the afternoon, I found the comet was, at that time, distant from *Mercury* nearly $\frac{1}{5}$ part of the semidiameter of the *orbis magnus*; being almost twice as near to the sun as the planet ☿; and having then 31° 30′ of N. Lat. *Mercury*'s not exceeding 3° 58′ to an eye in the sun: whence it is easily collected, that the comet could have no sensible influence upon ☿'s motion.

I shall now only beg leave to observe, that the elements above-given cannot possibly differ much from the true. For, after an interval of two months (in which time the comet had gone through almost $\frac{1}{4}$ part of it's orbit), it is surprising to find the observed and computed places agree so accurately, that the difference no-where amounts to a minute. In some parts of the orbit, the agreement is still greater; particularly, in the observations made at *Sherborn*, which come within half that quantity; and would have corresponded still nearer, but that I was ambitious to confine the whole series of observations within the narrow limit above-mentioned; which I have at last compassed, not without a long and tedious calculation.

It may, perhaps, be expected, considering the great part of it's orbit the comet described during it's appearance, that I should have settled it's period, and foretold it's return. This, I confess, would have given me great pleasure; neither would I have spared any pains in the inquiry, had I met with any prospect of success; but the period, upon my attempting it at first, came out so prodigiously long, the transverse ax of the ellipse being nearly equal to infinity, that I was stopped short in my inquiry; neither could I prevail upon myself to resume the subject again, when, upon turning over *Hevelius*, I found the account of comets, which had appeared at long intervals of time from us, as it might reasonably be expect'd, so short and uncertain: but, could I procure *Celsius*'s observations, or any made after the *Perihelion*, I might be induced to fall to work again; and would not fail communicating the result, did I meet with success; and, at the same time, the elements of the comet, which appeared in 1742, which I have had by me some time; not so perfect as I could wish, but as perfect as may be obtained from the few observations I met with.

The comet was in conjunction with the sun, *Feb.* 15. about midnight; and it's perigee, *Feb.* 16. about 1^h in the afternoon; at which time it was somewhat nearer the earth than the sun is at it's perigee; the comet's diſtance being then (,83) and the ſun's (,98) ſuch parts, as the ſemidiameter of the *magnus orbis* is (,100); from which we may have ſome idea of the comet's magnitude; and therefore may ſuppoſe it, at leaſt, equal to the earth.

The Path of the Comet, *which appeared from the beginning of* March 1742, *to the beginning of* April, *from the obſervations made at the Obſervatory and College of the Jeſuits at* Pekin *in* China, *and computed according to the equator and ecliptick, and alſo according to it's proper orbit; communicated by Mr* James Hodgſon, *F. R. S. &* *Schol. Reg. Math. Præc. in Æd.* Chriſti, Lond. No. 481, p. 264. Oct. &c. 1746.

XVIII.

True time of obſervation.	d.	h.	min.	Right Aſcenſion. ° '	Decl. from the Equator. ° '	Force in it's proper orbit. ° '	Longitude in the eclipticκ. ſ ° '	N. Lat. in the ecliptick. ° '	Digreſſion from the node in the ecliptick. ° '	Conſtellations to which the Comet paſſed.
Mart.	2	4	30. m.	281 55	6 0 A	0 0	♑ 12 24	16 58	17 14	To the foot of Antinous.
	4	4	0. m.	283 30	5 15 B	11 18	14 44	28 4	28 33	Near the tail of the Serpent.
	5	4	45. m.	283 33	10 50	16 55	16 2	33 33	34 9	Below the tail of Aquila.
	7	4	0. m.	284 48	22 40	28 48	19 32	45 9	46 3	Between Anſer and Cerberus.
	11	2	30. m.	288 1	44 57	51 15	♒ 3 6	66 22	68 29	Between the wing of Cyg. and Lyra.
	12	4	30. m.	289 6	50 3	56 24	9 56	70 53	73 38	In the Northern wing of Cygnus.
	13	3	15. m.	290 11	54 15	60 39	18 19	74 20	77 53	
	14	4	0. m.	291 40	58 50	65 18	♓ 2 20	77 33	82 32	Between Cygnus and the belly of Draco.
	15	3	15. m.	293 12	62 36	69 9	19 20	79 22	86 23	
	16	4	0. m.	295 0	66 0	72 38	♈ 8 35	79 59	89 32	In the belly of Draco.
	17	4	30. m.	297 10	69 11	75 55	26 57	79 31	93 11	
	18	4	0. m.	299 34	71 50	78 41	♉ 10 11	78 23	95 56	Between Draco and Cepheus.
	19	4	0. m.	302 39	74 23	8 123	20 27	76 49	98 38	At the knee of Cepheus.
		8	20. v.	304 38	75 40	82 47	24 46	75 53	100 1	
	22	0	0. v.	319 56	81 0	88 54	♊ 8 19	71 5	106 8	Between the feet of Cepheus, and then in the ſame place in the neighbourhood of the N. pole.
	23	9	45. v.	327 25	82 14	90 32	10 52	69 41	107 47	
	24	10	15. v.	336 22	83 12	92 1	12 53	68 23	109 16	
	27	9	0. v.	21 24	84 26	96 44	18 7	64 0	114 7	
	28	8	40. v.	26 28	84 20	97 20	18 34	63 32	114 38	
	29	1	30. m.	30 34	84 13	97 52	18 56	63 8	115 4	
	30	2	0. m.	38 13	83 54	98 42	19 38	62 21	115 55	
	31	2	50. m.	45 3	83 29	99 33	20 19	61 33	116 46	
Apr.	1	2	50. m.	50 51	83 0	100 23	20 56	60 47	117 36	
	2	3	12. m.	55 55	82 27	101 13	21 32	59 59	118 37	

But

But from the obſervations made *March* 2 and 4, it is manifeſt, that the comet came to the equator *March* 3. about 6^h *a. m.* and that it paſſed in R. aſc. 282° 30′, with inclination of it's path to the equator 84° 30′ very nearly; and therefore that it's long. was 13° 35′ in ♑, with N. lat. 22° 54′. Hence we may collect, that the path of the comet, which did not ſeem to deviate from a great circle, met the ecliptick in ♑ and ♒ 9° 19′ with incl. of 80°: and the colure of the equinoxes in the diſtance of 50° 37′½ from the poles of the world toward the equinoctial points with the angle of incl. 77° 33′½: and the colure of the ſolſtices in the diſt. of 23° 57′½ from the poles of the world, toward the ſolſticial points with ang. of incl. 13° 38′ equal to the greateſt elongation of it's orbit from the ſame colure in the averſe part, and to the diſt. of the poles of the orbit from the equinoctial points.

XIX. That the tracing of the courſes of comets belongs to the principal parts of the ſublimer Aſtronomy, has been paſt all doubt, ever ſince the great *Newton* 63 years ago publiſhed a problem of finding the path of comets by 3 accurate obſervations, from this hypotheſis, that they deſcribe a parabola about the ſun in their courſe. Dr *Halley* by this method determined the paths of 24 comets, by calculation, in a table publiſhed in the *Phil. Tranſ.* N. 297. p. 1886. and in the *Acta Erud.* 1707. p. 216. There are in reality, 21 different comets. The difficulty and neceſſity of this work has been ſufficiently ſhewn by the laſt mentioned Aſtronomer.

The Paths of Comets *according to the Hypotheſis which makes them deſcribe a Parabola about the* Sun; *by* Nicholas Struyck, *F. R. S.* No 492. p. 89. April, &c. 1749. *Preſented* April 6. 1749.

Following the ſteps of ſo great a man, I have noted by the ſame method, 18 other comets, which are not found in that table, in hopes that the periodical time of each may at length be found. But leſt thoſe obſervations of the paths of comets, ſhould, by any accident be loſt, I determined now to publiſh them, at the ſame time, thinking it my duty to mention thoſe who have accommodated them to an arithmetical calculus. The path of the comets of 1723 and 1737 was determined by Dr *Bradley*; of 1744, by Mr *Betts*; of 1699, 1702, and 1739, by the Abbot *de la Caille*. The path of the 2d comet of 1743 by Mr *Klinkenberg*; that of the 2d comet of 1746 by *M. des Chezeaux*; of the 1ſt comet of 1748, by *Maraldi*. I gave the obſervations of the comets ſeen in 1533, 1678, 1718, and 1729, to Mr *C. Downes* to be calculated. But the comets of 1706, 1707, 1742, the 1ſt of 1743, and the 2d of 1748, I calculated myſelf. I am alſo induced by various reaſons to be of opinion, that in *May* 1748, both here at *Amſterdam*, and in other places of *Europe*, on the very ſame night, 3 *Comets* were viſible; of which there is no other certain inſtance in Hiſtory. I have alſo added the comet ſeen at the end of 1680, and beginning of 1681; becauſe, in the laſt edition of Sir *I. Newton*, there are emendations, by which the ellipſis, that it deſcribed about the ſun, is determined. I ſhall only add, that of the 31 obſervations which I have of the comet ſeen in 1742, there are 22, the longi-

tudes

tudes of which ſcarce differ 1′; and 23 of which the latitudes do not differ ſo much as 1′.

Here follow the paths of the 19 comets mentioned above.

Time of the equations of the Perihelion at London.						Node of aſcenſion.				Inclination of the Orbit.			Perihelion in the Orbit.				Dif. Perih. from the Sun.	Motion.
An.		d	h	′	″		°	′	″	°	′	″		°	′	″	Part.	
1533	June	16	19	30	0	♌	5	44	0	35	49	0	♌	27	16	0	20280	Retro.
1678	Aug.	16	14	3	0	♍	11	40	0	3	4	20	♒	27	46	0	123802	Dir.
1699	Jan.	2	8	22	19	♒	21	45	35	69	20	0	♏	2	31	6	74400	Retro.
1702	March	2	14	12	19	♎	9	25	15	4	30	0	♌	18	41	3	64590	Dir.
1706	Jan.	19	4	56	0	♈	13	11	23	55	14	5	♊	12	36	25	$42686\frac{1}{2}$	Dir.
1707	Nov.	30	23	43	6	♉	22	50	29	88	37	40	♊	19	58	9	85904	Dir.
1718	Jan.	4	1	14	55	♌	7	55	20	31	12	53	♌	1	26	36	102565	Retro.
1723	Sept.	16	16	10	0	♈	14	14	16	49	59	0	♉	12	15	20	96942	Retro.
1729	June	12	6	35	41	♒	10	35	15	77	1	58	♒	22	16	53	406980	Dir.
1737	Jan.	19	8	20	0	♏	16	22	0	18	20	45	♒	25	55	0	$22282\frac{1}{3}$	Dir.
1739	June	6	9	59	49	♈	27	25	14	55	42	44	♋	12	38	40	67358	Retro.
1742	Jan.	28	4	20	50	♎	5	34	45	67	4	11	♏	7	33	44	$76555\frac{1}{2}$	Retro.
1742	Dec.	30	21	15	16	♊	8	10	48	2	15	50	♋	2	58	4	$83811\frac{1}{2}$	Dir.
1743	Sept.	9	21	16	18	♈	5	16	25	45	48	21	♐	6	33	52	52157	Retro.
1744	Feb.	19	8	17	0	♉	15	45	20	47	8	36	♎	17	12	55	$22206\frac{1}{10}$	Dir.
1747	Feb.	17	11	44	38	♌	26	58	27	77	56	55	♑	10	5	41	229388	Retro.
1748	April	17	19	25	4	♏	22	52	16	85	26	57	♏	5	0	50	$84066\frac{2}{3}$	Retro.
1748	June	7	1	24	15	♉	4	39	43	56	59	3	♑	6	9	24	$65525\frac{1}{2}$	Dir.
1680	Dec.	7	23	9	0	♑	2	2	0	61	6	48	♐	22	44	25	617	Dir.

But the *perihelian* diſtances are eſtimated in ſuch parts, as the mean diſtance of the earth from the ſun has 100,000.

Various aſtronomical obſervations made in Paragua *in* S. America; *communicated by* Jacob de Caſtro Sarmento, *M. D. F. R. S.* No. 490. p. 667. Dec. 1748. *Preſented* Jan. 28, 1747 8.

XX. 1. *Eclipſes of the ſun and moon obſerved in the miſſions of the Jeſuits to* Paragua, *by* F. Bonaventura Suarez, *Miſſionary, with a 5 foot teleſcope, and a pendulum, vibrating ſeconds, with an equal motion, and rectified to true time, by the altitude of the fixt ſtars.*

Eclipſe *of the* Sun, Nov. 5. 1706.

This eclipſe was obſerved at the town of *S. Ignatius* in *Paragua*, where the altitude of the S. pole is 26° 52′, and it's merid. diſt. from the R. Obſerv. at *Paris* 3^h 57′ 50″.

		h ′ *St. civ.*
Beginning of the eclipſe		8 52 *a. m.*
Digits obſcured - -	2	9 15
	$3\frac{1}{2}$	9 40
	4	10 0
	1	11 5
End		11 15

The greateſt quantity at 9^h 50′ dig. 4. 0′

Beginning

Beginning below the horizon: ſun riſe $5^h\ 53'$. ——March 11. 1709. *St. civ.*

	dig.		h	' a. m.
Digits obſcured	9	20	6	15
	6	30	6	50
	6	0	6	54
	5	0	7	3
	3	30	7	13
	3	0	7	17
	3	30	7	21
	1	30	7	28
End of the eclipſe			7	37 15''

The greateſt obſcuration was $9^{dig.}\ 20'$

——Moon, April 16. 1707. *p. m.*

	h	'
Beginning	7	55
Total obſcuration	8	58
Beginning of emerſion	10	45

The end was not obſerved becauſe of clouds.

——April 4. 1708. *p. m.*

Immerſion of ☽	h	'	''	*Emerſion of* ☽	h	'	''
Into a ſenſible penumbra	12	18	0	*Ariſtarchus*	14	13	15
Into the ſhadow	12	30	29	*Plato*	14	45	0
Ariſtarchus obſcured	12	37	11	Out of the ſhad.	15	3	0
Plato obſcured	12	46	0	Out of the pen.	15	12	0

——Sun, Jan. 18. 1730. *p. m.*

	h	'	''	*Digits obſcured.*	
Beginning	2	52	30	0	0
	2	58	10	1	0
	3	5	0	2	0
	3	19	45	4	15
	3	29	20	5	45
	3	21	22	6	0
	3	39	17	7	0
	3	41	55	7	20
	3	45	0	7	40
	Clouds				
	4	7	33	8	0
	4	9	36	7	45
	4	11	34	7	30
	Clouds				
	4	51	0	4	0
	4	42	0	2	0
	4	50	0	0	30

The end was not obſerved becauſe of clouds; it ſeems to have been at $4^h\ 52'$:

$4^h\ 52'$: at about $4^h\ 55'$ the diſk of ☉ was ſeen entire: ☽ did not appear on his limb.

The greateſt obſcuration ſeems to have been dig. $8\frac{1}{3}$.

——Total of the Moon, Aug. 8. 1729.

	h	′	″
Clouds			
Beginning of emerſion	10	1	0
Digits obſcured 11	10	6	28
Digits 6	10	33	2

Occultation of Jupiter *by the* Moon, Dec. 9. 1729. *p. m.*

h	′	″	
11	3	5	☽ eclipſed a ſatellite of ♃.
11	13	25	☽ touched the limb of ♃
11	15	0	☽ totally eclipſed ♃.

Eclipſe *of the* Moon, Dec. 1. 1713. *p. m.*

This eclipſe was obſerved in the town of *S. Joſeph.*
Diff. of merid. from R. Obſ. *Par.* $3^h\ 52'\ 30''$.

	h	′	″
Beginning	10	33	31
End	12	56	57

The greateſt quantity obſcured was dig. 5. at about $11^h\ 45'$.

——March 26. 1717. *p. m.*

This eclipſe was obſerved on the very meridian of *S. Coſma.* Diff. of merid. from *Paris* $3^h\ 52'\ 20''$. Sky clear and calm.

		h	′	″
Senſible penumbra		9	40	0
Beginning of the eclipſe		10	2	21
Digits obſcured	1	10	8	30
	2	10	15	2
	3	10	13	41
	4	10	31	32
	5	10	40	56
	6	10	52	8
	7	11	10	40

The greateſt quantity obſcured ſeemed to be $7^{dig.}\ 18'$

Emerſion of ☽ *from the ſhadow.*

		h	′	″
Digits obſcured	6	11	45	40
	5	12	6	25
	4	12	16	35
	3	12	24	10
	2	12	32	46
	1	12	39	25
End of the eclipſe		12	45	40
Emerſ. from penumb.		13	1	0

This

This eclipſe was obſerved in the town of *S. Michael the Archangel* with a tube of 10 foot. — Feb. 24. 1728. *p. m.*

Diff of time between *S. Mich.* and R. Obſ. *Par.* $3^h\ 48'\ 50''$.

	h	′	″
Beginning of the eclipſe - - - - - - - - - - - - - -	14	3	35
End -	17	0	37

Digits obſcured at the middle of the eclipſe $9^{dig.}\ 40'$.

This total eclipſe of ☽ was obſerved in the college *de las Corrientes.* Diff. merid. from *Paris* about $4^h\ 2'$. — Mar. 4. 1700. *p. m.*

	h	′
Beginning of the eclipſe - - - - - - - - - - - - - -	13	14
Total immerſion - - - - - - - - - - - - - - - - - -	14	34
Beginning of emerſion - - - - - - - - - - - - - - -	16	15
End of the eclipſe - - - - - - - - - - - - - - - -	17	15

	h ′ ″	Tubes	
Emerſion of the 1ſt ſatellite obſerved at *S. Ignat.*	10 52 49	13 foot	— Satellites of Jupiter, Dec. 21. 1729. *p. m.*
——— at *Peterſburg* by *M. Nic. de l'Iſle*	16 42 36	15	
Diff.	5 49 47		
Immerſion of the 4th ſatellite at *S. Ignatius*	7 23 0	18	— Mar. 27. 1730.
Peterſburg	13 12 31	13	
Diff.	5 49 31		
Emerſion of the 2d ſatellite at *S. Ignatius*	6 36 45	13	— Apr. 8. 1730. *p. m.*
Peterſburg	12 26 15	13	
Difference of meridians	5 49 30		

The following *phænomena* of the ſatellites of ♃ were obſerved at *S. Ignatius p. m.*

At $14^h\ 21'$ there was a conjunction of the 1ſt with the 2d both ſtars ſeemed to be but one. Dec. 29, 1729.

At $9^h\ 10'$ there was a conjunction of the 1ſt and 2d. Jan. 23. 1730.

At $15^h\ 21'\ 15''$ the 1ſt and 2d were conjoyned, ſo as to appear but one. At $15^h\ 27'$ one was yet viſible : but at $15^h\ 36'$ they were disjoined. Jan. 25.

At $11^h\ 36'$ there was a conjunction of the 2d and 4th. Mar. 9.

At $10^h\ 9'$ there was an occultation of the 2d being retrograde in the margin of ♃. 12.

At $6^h\ 38'$ there was a conjunction of the 2d and 3d. 18.

At $9^h\ 7'\ 40''$ there was an occultation of the 3d direct in the margin of ♃. 29.

Mar. 30. At $7^h\ 21'\ 30''$ there was an occultation of the 2d retrograde in the limb of ♃.

31. At $9^h\ 21'\ 15''$ there was an occultation of the 1st retrograde in the limb of ♃.

Apr. 1. At $6^h\ 36'\ 25''$ with a tube of 18 f. I observed an occultation of the 1st direct in the margin of ♃: but at $10^h\ 16'\ 57''$ it emerged from the shadow of ♃.

—— Jupiter. Dec. 9. 1729. *p. m.* At $11^h\ 3'\ 5''$ the margin of ☽ touched the 3d satellite of ♃. The beginning of the occultation of ♃ was $11^h\ 13'\ 15''$. The total occultation of ♃ in the margin of ☽ was $11^h\ 15'$.

—— Saturn. Apr. 27. 1730. The *ansulæ* of saturn appeared very thin; but *May* 8 17^h saturn was round and quite deprived of his *ansulæ*.

Astronomical observations of F. Bonaventura Suarez, *in the town of* S. Ignatius *in* Paragua. *There is another town of* S. Ignatius *more to the E. on the river* Zabebiri.

The town of S. Ignatius, more westward than the rest, is 50 *Spanish* leagues to the S. from the city of *Assumption* in *Paragua*. Lat. of *Assumption* observed by me is 25° 14′ S. lat. of S. *Ignatius* 26° 52′.

		h	′	″
Diff. mer. S. *Ignat.* W. from	*Petersburg*	5	59	40
	Paris	3	57	50
	London	3	48	40
	S. Cosma	0	5	30

—— *Satellites of* Jupiter.

Emersions of the 1st *p. m.*

		d	h	′	″		foot
1729	*Jan.*	16	8	16	42	tube	13
	Feb.	18	8	28	13	- -	13
	Mar.	6	6	50	18	- -	13
		13	8	47	4	- -	13
	Immersion of the first						
	Nov.	3	16	20	36	- -	13
	Dec.	21	10	52	49	- -	13
1730	*Jan.*	6	9	0	40	- -	18
		13	10	53	8	- -	18
	Emersions of the first						
1730.	*Feb.*	7	7	46	20	- -	13
	Mar.	9	9	56	21	- -	13
		16	11	53	21	- -	18
		25	8	49	46	- -	18
	Apr.	1	10	16	57	- -	18
		17	8	39	55	- -	18
	May.	3	7	1	47	- -	18
		10	8	56	51	- -	18
	Emersions of the second *p. m.*						
	Apr.	8	6	36	45	- -	18
	May.	10	6	32	30	- -	18

Emersion

	Emerſion of the third			
	d h ′ ″			foot
Apr.	20 8 44 45	tube		18
	Emerſion of the fourth			
Mar.	10 9 22 0	doubtful		
	Immerſion of the fourth			
Mar.	27 7 23 0	-	-	18

—— *communicated by* — Suarez, M. D. No. 491. p. 8. Jan. *&c.* 1749. *Preſented* Jan. 19. 1748-9.

2. The ſky was ſo cloudy, that I could make only the following obſervations, after the emerſion of the ſecond digit of the moon from the ſhadow of the earth, with a teleſcope of 10½ feet.

	h ′ ″
Ariſtarchus emerges - - - - - - - - - - - - -	14 31 47
Tycho emerges - - - - - - - - - - - - - -	14 37 30
Calippus emerges - - - - - - - - - - - - -	14 56 47
Dionyſius emerges - - - - - - - - - - - - -	15 0 4
Mare criſium begins to emerge - - - - - - - - - -	15 13 17
End of the eclipſe - - - - - - - - - - - - - -	15 16 4

Eclipſe *of the* moon *obſerved at* S. Angelus Cuſtos, Feb. 24. 1747.

The town of *S. Angelo* in the miſſions of *Paragua* is more eaſtward than the reſt. It's longitude from the Iſland of *Ferro* is 323° 30′ and lat. 28° 17′ S.

The ſenſible penumbra was 14ʰ 44′

Immerſion of the moon and ſpots into the ſhadow.

	h ′ ″
Beginning - - - - - - - - - - - - - - - -	14 55 44
Ariſtarchus - - - - - - - - - - - - - - -	15 0 13
Galilæus - - - - - - - - - - - - - - - -	15 0 41
Mare humorum begins - - - - - - - - - - - - -	15 4 14
Lower angle of *terra pruinæ* - - - - - - - - - - -	15 5 29
Copernicus &c. - - - - - - - - - - - - - - -	15 9 26
Mare humorum entire - - - - - - - - - - - - -	15 9 26
Plato and *Tycho* were equally diſtant from the centre of the ſhadow - - - - - - - - - - - - - - - -	15 13 44
The ſame - - - - - - - - - - - - - - - -	15 17 2
Plato and *Tycho* at the ſame time in the edge of the ſhadow	15 20 25
6 digits obſcured - - - - - - - - - - - - - -	15 24 6
Menelaus - - - - - - - - - - - - - - - -	15 27 28
Dionyſius - - - - - - - - - - - - - - - -	15 29 35
Lacus ſomniorum - - - - - - - - - - - - - -	15 36 10
Beginning of *mare criſium* - - - - - - - - - - -	15 43 41
Middle - - - - - - - - - - - - - - - - -	15 46 26
End - - - - - - - - - - - - - - - - - -	15 49 16
Total obſcuration of ☽ - - - - - - - - - - - - -	15 53 16

— *at* S. Maria Major, *Aug.* 19. 1747. *p. m.*

 Emerſion

Emerſion of ☽ from the ſhadow

	h	′	″
Beginning of the emerſion	17	34	48
Grimaldus	17	36	52
Ariſtarchus	17	40	0
Plato	17	53	34
Tycho	18	0	23
6 digits obſcured	18	3	30

The moon near the W. trembling with the vapours of the horizon was no longer obſerved. The ſky was very clear during the whole time of the eclipſe.

Long. of *S. Maria Maj.* from *Ferro* 322° 40′ lat. 27° 51′ S.

— *by* F. Auguſtin Hallerſtein, *Preſ. of the imperial* College *of Aſtron. at* Pekin *in* China, *to Dr* Mortimer, No. 494. p. 305. Jan. *&c.* 1750. *Read* Jan. 18. 1749.

3. The comet ſeen by us this year was very diſmal, for beſides it's ſhining with a very obſcure and malignant light, it went in ſo deſert a path, Fig. 9. and in ſuch an unfavourble ſky, that it could be obſerved but very ſeldom, and compared but with a few ſmall ſtars not well known.

The Path *and* Ephemeris *of the* Comet *ſeen at* Pekin *in* 1748.

Apr. 26. about 3 in the morning it was firſt ſeen by thoſe whoſe office it is to watch in the Obſervatory of this palace; and the place of it was rudely obſerved to be in 18° ♓ with 27° N. lat. namely in the breaſt of *pegaſus* under the ſtars λ and μ, the head was equal to a ſtar of the 3d order, and the tail ſeemed to be about 1° long.

Fig. 9.

On the following days there was no poſſibility of comparing it accurately with any fixed ſtar, and therefore ſome places of it can only be groſsly determined by configurations with the neighbouring ſtars, which as they cannot bear the ſtrictneſs of a calculus, do but barely ſhew the path which the comet held.

Apr. 27. about 2 in the morning long. ♓ 21° 21′ with N. lat. 31° 35′.

Apr. 28. about the ſame time ♓ 25° 15′ N. lat. 36° 0′

Apr. 29. about the ſame time ♓ 29° 10′ N. lat. 40° 0′

Apr. 30. and *May* 1. nothing ſeen for the clouds.

May. 2. the comet could at laſt be compared with a little ſhining ſtar in the middle of 5 ſmall ſtars in the bend of the chain of *Andromeda*, and is obſerved by means of a micrometer and pendulum 2^h 31′ 49″ true time. The comet more eaſtward than the ſtar 1′ 50″ of the pendulum, and more northward 57′ 8″.

May 6. in the morning the comet was compared with a ſmall ſtar, the 6th in order in *Caſſiopea* in *Flamſted's Cat. Brit.* by the differences of declinations and diſtances, on account of the want of clearneſs in the ſky, the ſlow motion, and malignant light of the comet, it's tranſit could not be determined by ſeveral ſeconds; at 2^h 3′ 57″ true time the comet

was

was more northward than the ſtar 44′ 8″ and the diſtance being taken was 50′ 50″.

On the following days the comet was compared with ſeveral unknown ſmall ſtars, ſo that it's place could not be determined.

May 15. about 9h *p. m.* for the moment of time was forgotten to be noted, the comet was ſeen between 2 ſmall ſtars, from the neareſt of which it was diſtant, not in an inverted but right ſituation 11′ 3″ S. and from the farther 59′ 58″ N. It was alſo more to the E. than the neareſt 1′ of the pendulum. Theſe 2 ſtars are placed in the *Cat. Brit.* in *Caſſiopea*, the more northern about the end, in ♊ 4° 49′ 7″ with N. lat. 58° 6′ 56″, and the more ſouthern in ♊ 3° 28′ 12″, with N. lat. 57° 11′ 10″, in 1690.

May 16. the comet being compared with the more northern 11h 1′ 59″ *p. m.* was more eaſtward than the ſtar 18′ 26″ of the pendulum, and more to the N. 26′ 4″.

May 19. the comet being compared with the ſtar of the 6th magnitude in *Cepheus* according to *Hevelius*'s *Cat.* in 1700, where it is called *ſub faſcia ſequens*, was at 10h 23′ 29″ in the ſame R. aſc. with the ſtar: there could not be found any difference of time between their tranſits, and the comet was more northward than the ſtar 48′ 14″. On the following days nothing certain could be determined.

May 29. the comet was ſeen amongſt feveral unknown ſmall ſtars, and on moving the tube a little there appeared one which *Hevelius*, in his *cat.* of fixed ſtars 1660, places in *Camelopardalus*, and *calls ſupra tergum, ſive in cuſpide pedis ſiniſtri Cephei 5 magnit.* But as the parallel of this ſtar was too far diſtant from the parallel of the comet to be immediately compared with it, it was compared with an intermediate ſtar, and the comet is noted 11h 21′ 25″ *p. m.* more E. than that ſtar 16′ 13″ of the pendulum, and more S. 1° 37′ 22″.

After this, as there were no ſtars near the path of the comet, nor any like to be, with which it could be compared, they were to be ſought farther off: and therefore on the following days it was compared with γ *Cephei*, from the parallel of which it was not very diſtant. Therefore, the teleſcope being well fixt,

		h	′	″	
June	1. *p. m.*	9	30	53	γ *Cephei* to the horary.
	2. *a. m.*	3	24	51	The comet to the horary more N. than the ſtar 26′ 59″.
	4. *p. m.*	8	28	58	γ *Cephei* to the horary.
	5. *a. m.*	2	41	9	The comet to the horary more S. than the ſtar γ 8′ 20″.
	p. m.	8	23	23	γ *Cephei* to the horary.
	6. *a. m.*	2	41	17	The comet to the horary more S. than the ſtar 20′ 13″.
	7. *p. m.*	8	5	3	γ *Cephei* to the horary.

8. *a. m.*

8. *a. m.*	$2^h\ 32'\ 51''$	The comet to the horary more S. than the ſtar 42′ 51″.
p. m.	8 31 59	γ *Cephei* to the horary.
9. *a. m.*	3 4 38	The comet to the horary more S. than the ſtar 55′ 34″.

12. *p. m.* When there was hardly any hope of ſeeing the comet any longer, I ſaw it obſcurely, more like the footſtep of a comet than the comet itſelf. Beſides the brightneſs of the moon, and the reflexion of it's rays from the clouds, which make all obſervations difficult and doubtful, were great obſtructions. I compared it however as well as I could with a ſmall ſtar, which I afterwards found in a little map and catalogue of M. *de la Caille*, inſerted in the *Mem. de l'Acad.* 1742. on account of a comet obſerved in that year, and noted with R. aſc. at that time 91° 21′ and N. decl. 73° 49′; and marked with the letter *A*; therefore $9^h\ 33'\ 6''$ *A* to the horary: then $9^h\ 45'\ 23''$ the comet to the horary more N. than the ſtar 46′ 34″.

June 13. *p. m.* $9^h\ 13'\ 11''$ *A* to the horary: $9^h\ 29'\ 43''$ the comet to the horary more N. than the ſtar 36′ 15″.

June 14. *p. m.* $9^h\ 15'\ 44''$ *A* to the horary $9^h\ 36'\ 4''$: the comet to the horary more N. than the ſtar 25′ 47″. Then being ſeen at $9^h\ 55'$ the diſtances were meaſured of the comet from the ſtars.

I 25′ 9″, *B* 38′ 39″, *R* 43′ 3″.

But the comet ſtood juſt by the ſtar *Q*, all which ſtars are noted in the place cited above.

June 15 and 16. nothing could be obſerved becauſe of clouds.

June 17 and 18. F. *Ant. Gaubil* obſerved in my abſence from home as follows;

June 17. *p. m.*	$9^h\ 26'\ 30''$	*A* to the horary.
	53 35	*R* to the horary.
	9 55 15	The comet to the horary in the ſame parall. with *A*.
June 18. *p. m.*	9 52 14	*A* to the horary.
	10 19 21	*K* to the horary.
	9 24 50	The comet to the horary more S. than *A* 16′ 30″. *R* 10 20.

The R. aſc. of the ſtar *R* is 98° 6′¼ and N. decl. 73° 43′. The times of the obſervations are all true and ſufficiently correct. They were made with a tube of 6 feet, in which an *Engliſh* micrometer was inſerted.

A new conſtellation. Fig. 10.

A ſmall conſtellation was obſerved *May* 29 in purſuing the comet with the teleſcope. It is repreſented in Fig. 10. The diſtances of the ſmall ſtars that compoſed it were α from β 12′ 19″: β from γ 16′ 45″: γ from δ 10′ 2″: δ from ϵ 16′ 45″; γ from ζ 19′ 53″: δ from ζ 28′ 17″: γ from ϑ, which is the *ſupra tergum* of *Camelopardalus* 58′ 16″: δ from the ſame 50′ 3″. But I leave theſe diſtances for others to meaſure more

more accurately, The ſituation of this aſteriſm is not inverted but right.

Occultation of Mars *by the* Moon, *obſerv'd at* Pekin *in* China, Dec. 6. 1747. *p. m.*

Fig. 11.

h	′	″	
4	52	17	♂ diſtant from the lucid limb of ☽ 50′ 4″. Then ♂ was obſerved and compared with the ſtar. 1. and ſo
5	3	9	♂ to the horary.
	12	47	1. to the horary more N. than ♂ 41′ 6″.
	9	38	Temporary difference.
5	18	26	♂ to the horary.
	28	2	1. to the horary more N. than ♂ 41′ 2″.
	9	36	Temporary difference.
5	34	34	♂ entering under the obſcure limb of ☽ wholly diſappeared, diſtant from the N. horn of ☽ 23′ 28″. The diameter being immediately meaſured was 32′ 53″. But the lucid part of ☽ was 7′ 39″. In the mean time whilſt ♂ lay hid behind ☽, it was obſerved and compared with ϑ ♑ and ſo:
5	58	9	ϑ to the horary.
6	3	31	Lucid limb of ☽ to the horary.
	4	23	N. horn of ☽ to the horary.
		58	S. horn of ☽ to the horary. The S. limb of ☽ was more S. than ϑ 34′ 27″. Again,
6	11	36	ϑ to the horary.
	17	22	Lucid limb of ☽ to the horary.
	18	12	N. horn of ☽ to the horary.
		46	S. horn of ☽ to the horary. The S. limb of ☽ more S. than ϑ 31′ 0″. Thirdly,
6	23	22	ϑ to the horary.
	29	26	Lucid limb of ☽ to the horary.
	30	17	N. horn of ☽ to the horary.
		52	S. horn of ☽ to the horary. S. limb of ☽ was more S. than ϑ 27′ 48″.
6	38	52	There was a very ſmall ſtar approaching to the obſcure limb of ☽, and when it was juſt entering, it was diſtant from the lucid limb of ☽ 41′ 23″. Then,
6	46	2	♂ firſt appeared coming from under ☽ and diſtant from the N. horn 29′ 24″.
7	2	23	The abovementioned little ſtar entered the dark limb of ☽ diſtant from the N. horn 29′ 24″. Laſtly,
7	12	24	♂ was diſtant from the lucid limb of ☽ 11′ 30″.

The

The moments of time are true, and corrected by correſponding altitudes.

All the phaſes with a tube of 6 feet, with an *Engliſh* micrometer.

Conjunction of Mars *and* Venus, *obſerved at* Pekin, March 1748.

A ♂, the path of ♂ is near ♀ by the *Pekin* ephemerides.
B ♂, the ſame path by the *Paris* ephem. of *de la Caille.*
C ♂, the ſame path by the obſervations.
D ♂, the ſame path by the *Bononian* eph. of *Manfredi.*

The obſervations were as follows:

Fig. 12.

	h ′ ″		h ′ ″
Mar. 12.	6 27 52	♂ more E. than ♀	1 48 48
		more N. - -	0 53 46
13.	6 25 43	more E. - -	0 56 8
		more N. - -	0 27 26
14.	6 10 33	more E. - -	0 29 34
		more N. - -	0 14 36
15.	6 28 4	more E. - -	0 3 0
		more N. - -	0 1 30
16.	6 27 13	more W. - -	0 23 48
		more S. - -	0 10 41
17.	6 25 14	more W. - -	0 50 38
		more S. - -	0 23 24
19.	6 29 52	more W. - -	1 44 47
		more S. - -	0 48 14

All theſe differences were determined by repeated operations, with a tube of 6 feet with micrometers. The times alſo are true, and corrected by correſponding altitudes.

— *of* Jupiter *with* Venus, Jan. 1. 1748. *p. m.*

h ′ ″	
5 15 41	♃ was diſtant from ♀ 1° 3′ 49″. Then,
22 14	♃ to the horary.
25 6	♀ to the horary more S. than ♃ 50′ 35′
2 52	Again,
5 26 36	♃ to the horary.
29 32	♀ to the horary more S. than ♃ 50′ 15″.
2 56	Thirdly,
5 30 35	♃ to the horary.
33 34	♀ to the horary more S. than ♃ 49′ 37″.
2 59	

4. *Apr.*

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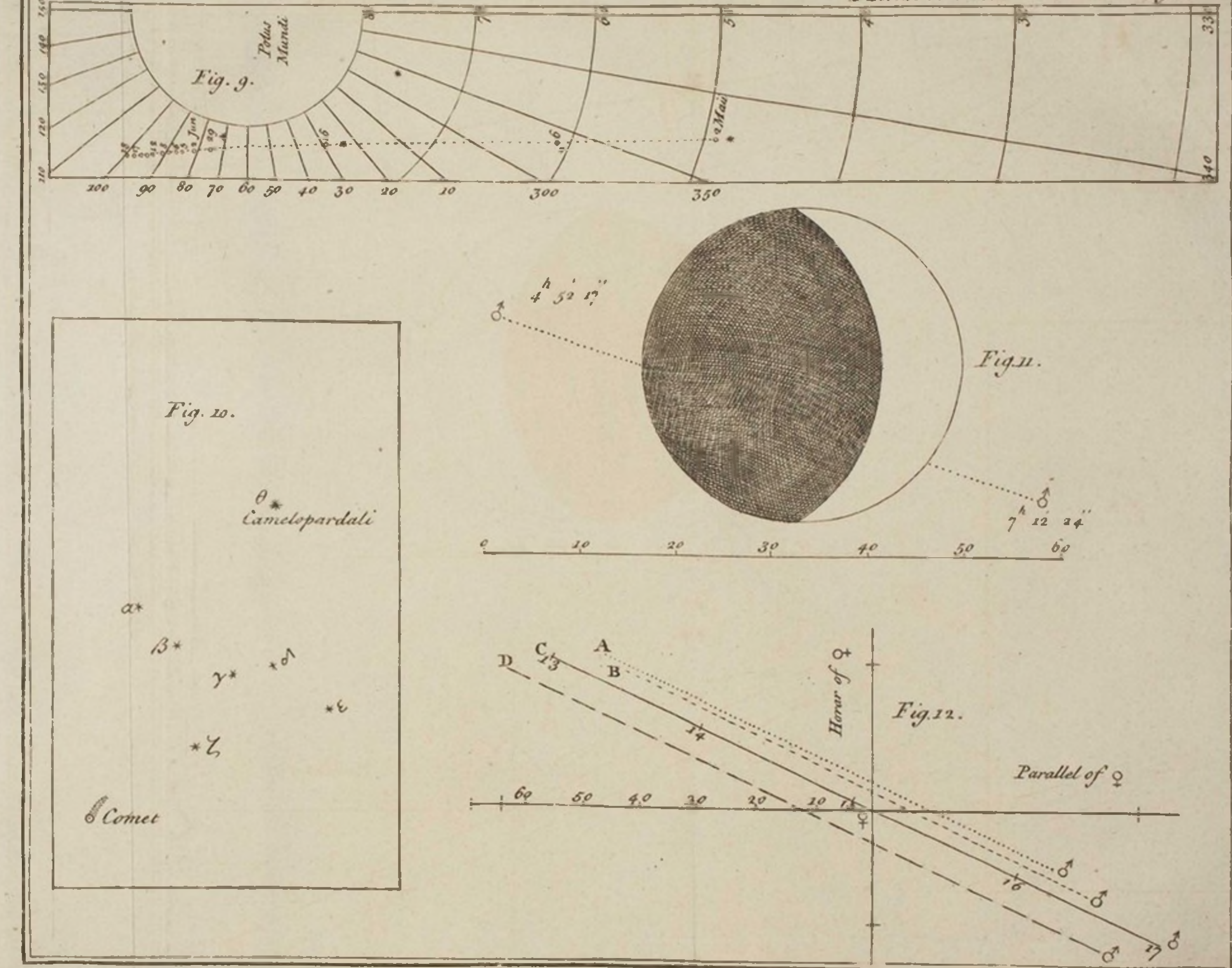

4. *Apr.* 27. 3^h 30′ *a. m.* we ſaw the comet in the middle of the ſtars of *Pegaſus* $\beta\ \lambda\ \eta$. —— *by* F. Anthony Gaubil, *of the* French College *of* Jeſuits *at* Pekin, *to the ſame.* ibid. p. 316. *dated* Pekin, Nov. 8. 1748.

May 2. we compared the comet with the ſtars mentioned by *F. Hallerſtein* in *Flamſted* 1690. The ſtar in ♈ 11° 26′ 45″. The place of the comet was concluded to be almoſt the ſame as by *F. Hallerſtein*'s obſervation.

May 3. 3^h *a. m.* α and σ of *Caſſiopea* in a right line with the comet. σ is pretty exactly in the middle.

May 4. $4^h\frac{1}{2}$ *a. m.* the comet more W. than the 3d ſtar in *Caſſiop.* in *Flamſt.* 5′ 35″, the comet more S. 1° 1′.

May 5. nothing could be obſerved exactly.

May 6. 2^h 51′ *a. m.* a line thro' α and β of *Caſſiop.* a little to the S. of the comet, diſtance of β from the ſtar $\varkappa$ = diſt. of β from the comet. Comet 1748.

May 10. 9^h 14′ *p. m.* the laſt true alt. of the comet 20° 48′ 58″. The comet more W. than the eaſtern ſtar (it is compounded of two) 27′ 12″; in *Flamſt.* the ſtar in *Taurus* 25° and ſome min.

May 15. we compared the comet with the ſtars of *F. Hallerſtein.* True merid. alt. of the comet *p. m.* 25° 51′ 30″ 10^h 12′. The comet more S. than the ſtar 8′ merid. alt. of the northern ſtar 25° 59′ 30″. We did not well obſerve the diff. of R. aſc.

May 16. *p. m.* true merid. alt. of the comet 26° 16′ 32″; *in reticulo* 10^h 22′: the comet more E. than *Hallerſtein*'s ſtar 1° 41′.

May 17. 10^h 40′ *p. m.* laſt true alt. of the comet 26° 46′ 34″.

γ *Cephei* to the horary 7^h 54′ 58″.

Comet to the horary 10 41 43.

Path of the comet ſeems more N. than the path of the ſtar 38′ 20″.

I do not find any number of obſervations made till *June* 7. But by comparing the comet with the *Hevelian* ſtar, and others not well known to me, I ſeem to be able to conclude, that from *June* 2 to 7 the R. aſc. of the comet increaſed 6° and ſome min. and that the decl. decreaſed 55′.

June 7. *a. m.* 1^h 15′ the ſtar to the horary 35′ 30″ after the comet. The ſtar more N. 1° 30′ very doubtfully obſerved *.

June 9. 0^h 45′ the comet to the horary.

0 49 10″ the ſtar to the horary *A* †.

The comet more N. 1° 30′.

To *June* 12. nothing was obſerved with ſufficient exactneſs.

June 13. *p. m.* 9^h 30′ the diſtance of the comet from the ſtar *I* ‖ 10′ 20″.

The comet more N. 4′ 25″.

The comet is more E.

* *Hevelius*'s ſtar *ann.* 1660 R. aſc. 2° 24′ 39″ diſt. from the pole 12° 42′ 17″.

† Star *A* in Fig. *D* of la Caille, *Mem. Acad.* 1740.

‖ Star *I* in Fig. *D* of la Caille, *Mem. Acad.* 1742.

	h	′	″		
June 17. *p. m.*	0	26	30	*A*	to the horary *.
		53	35	*R*	
		55	15	Comet	

Comet and *R* in the same declination.

	h	′	″		
June 18. *p. m.*	9	52	14	*A*	to the horary.
	10	19	21	*R*	
		24	57	Comet	

Comet more S. than *A* 16′ 30″.
R 10 20.

On the following days with a tube of above 7 feet, the aperture of which = 1° 0′ 24″ the comet was observed with the star *D* in Fig. *B* of *la Caille, Mem. Acad.* 1742. Many of these observations had hardly any success: I relate only two which seem to me not very exact.

	h	′	″
June 27. *p. m.* the star *D* enters the tube	9	23	10
goes out of the tube	9	36	0
Comet enters	9	40	28
goes out	9	52	16

The comet is concluded more N. 19′ 40″.

	h	′	″	
June 29. *D* enters	11	1	40	*p. m.*
goes out	11	14	51	
Comet enters	11	23	54	
goes out	11	36	0	

The comet is concluded more N. 12′ or perhaps 13′.

The comet was not easily seen *June* 29, there were clouds on *June* 30, and *July* 1 and 2; and it was not afterwards pursued.

Conjunction of Mars *and* Venus.

1748 *t. v. Mar.* 15. *p. m.* 8h 10′ the occidental limb of ♂ is distant from the occidental limb of ♀ 1′ 29″.

Eclipses *of the* Satellites *of* Jupiter.

	h	′	″	
Oct. 13. p. m. *temp. ver.*	9	40	30	Emersion of the 3d.
15.	8	37	26	1st.
20.	10	7	56	Total immersion of 3d.
21.	5	52	12	Emersion of the 2d doubtful.
28.	8	29	20	1st Emersion of the 2d.
Nov. 7.	8	52	59	1st Emersion of the 1st.

These Observations were made with a tube of 15 feet.

* *A R* notes of stars in Fig. *D* of la Caille, *Mem. Acad.* 1742.

XXI. The

Remarks upon the Solar *and the* Lunar *years, the* Cycle *of* 19 *years, commonly called the* Golden Number, *the* Epact, *and a method of finding the time of* Easter, *as it is now observed in most parts of* Europe. *Being part of a letter from the R. Hon.* Geo. *E. of* Macclesfield *to* M. Folkes, *Esq; P. R. S.* N° 495. p. 417. *Read* May 10. 1750.

Of the Solar Year.

XXI. The mean *tropical solar year*, or that mean space of time wherein the sun, or earth, after departing from any point of the ecliptic, returns to the same again, consists, according to Dr *Halley*'s tables, of 365^d, 5^h, $48'$, $55''$: which is less by $11'$, $5''$, than the mean *Julian* year, consisting of 365^d, 6^h, $0'$, $0''$. Hence the equinoxes and solstices anticipate, or come earlier than the *Julian* account supposes them to do, by $11'$, $5''$, in each mean *Julian* year; or $44'$, $20''$ in every 4; or 3^d, 1^h, $53'$, $20''$, in every 400 *Julian* years.

In order to correct this error in the *Julian* year, the authors of the *Gregorian* method of regulating the year, when they reformed the calendar in the beginning of *Oct.* 1582, directed that 3 intercalary days should be omitted or dropped in every 400 years; by reckoning all those years, whose date consists of a number of entire hundreds not divisible by 4, such as 1700, 1800, 1900, 2100, *&c.* to be only common, and not bissextile or leap years, as they would otherwise have been; and consequently omitting the intercalary days, which, according to the *Julian* account, should have been inserted in *Feb.* in those years. But at the same time they ordered that every fourth hundredth year, consisting of a number of entire hundreds, divisible by 4, such as 1600, 2000, 2400, 2800, *&c.* should still be considered as bissextile or leap years, and, of consequence, that 1 day should be intercalated as usual in those years.

This correction, however, did not entirely remove the error: for the equinoxes and solstices still anticipate 1^h, $53'$, $20''$ in every 400 *Gregorian* years. But that difference is so inconsiderable as not to amount to 24 hours, or to one whole day, in less than 5082 *Gregorian* years.

Of the Lunar Year, Cycle of 19 Years, *and the* Epact.

The space of time betwixt one mean conjunction of the moon with the sun and the next following, or a mean *synodical month*, is equal to 29^d, 12^h, $44'$, $3''$, $2'''$, 56^{vi}, according to Mr *Pound*'s tables of mean conjunctions. The common lunar year consists of 12 such months. The intercalary or *embolimæan* year consists of 13 such months. In each cycle of 19 lunar years, there are 12 common, and 7 intercalary or *embolimæan* years, making together 235 synodical months.

It was thought, at the time of the General Council of *Nice*, which was holden in 325, that 19 *Julian* solar years were exactly equal to such a cycle of 19 lunar years, or to 235 synodical months; and therefore, that, at the end of 19 years, the new moons or conjunctions would happen exactly at the same times, as they did 19 years before: and upon this supposition it was, that, some time afterwards, the several numbers of that cycle, commonly called the golden numbers, were prefixed to all those days in the calendar, on which the new moons then happened in the respective years corresponding to those numbers; it being imagined, that whensoever any of those numbers should for the future, be the golden number of the year, the new moons would invariably happen on those days in the several months, to which that number was prefixed.

But this was a mistake:

	d	h	′	″	‴
For 19 *Julian* ſolar years contain - - - - - -	6939	18	0	0	0
Whereas 235 ſynodical months contain only - -	6939	16	31	56	30
And are therefore leſs than 19 *Julian* ſolar years by	0	1	28	3	30

This difference amounts to a whole day very nearly in 310.7 years, the new moons anticipating, or falling earlier, by 24 hours in that ſpace of time, than they did before: and therefore now in 1750, the new moons happen above $4\frac{1}{2}$ days ſooner, than the times pointed out by the golden numbers in the calendar.

In order therefore to preſerve a ſort of regular correſpondence betwixt the ſolar and the lunar years, and to make the golden numbers, prefixed to the days of the month, uſeful for determining the times of the new moons, it would be neceſſary when once thoſe golden numbers ſhould have been prefixed to the proper days, to make them anticipate a day at the end of every 310.7 years, as the moons will actually have done; that is, to ſet them back one day, by prefixing each of them to the day preceding that, againſt which they before ſtood.

But as ſuch a rule would neither be ſo eaſily comprehended or retained in memory, as if the alteration was to be made at the end or at the begining of complete centuries of years; the rule would be much more fit for practice, and keep ſufficiently near to the truth, if thoſe numbers ſhould be ſet back 9 days in the ſpace of 2800 years; by ſetting them back 1 day, firſt at the end of 400 years, and then at the end of every 200 years for eight times ſucceſſively: whereby they would be ſet back, in the whole, 9 days in 2800 years. After which they muſt again be ſet one day back at the end of 400 years, and ſo on, as in the preceding 2800 years. By which means the golden numbers would always point out the mean times of the new moons, within a day of the truth.

It is plain however, that the lunar year will have loſt one day more than ordinary, with reſpect to the ſolar year, whenever the new moons ſhall have anticipated a whole day, as they will have done at thoſe times when it is neceſſary that the golden numbers ſhould, by the rule juſt now given, be ſet back one day: and conſequently the epact, for that and the ſucceeding years, muſt exceed by an unit the ſeveral correſponding epacts of the preceding 19 years.

For the epact is the difference, in whole days, betwixt the common *Julian* ſolar and the lunar year; the former being reckoned to conſiſt of 365, and the latter of only 354 days. If therefore the ſolar and the lunar year at any time ſhould commence on the ſame day, the ſolar would, at the end of the year, have exceeded the lunar by 11 days: which number 11 would be the epact of the next year: 22 would be the epact of the year following, and 33 the epact of the year after that, the epacts increaſing yearly by 11. But as often as this yearly addition makes the epact exceed 30, thoſe 30 are rejected as making an intercalary month, and only

only the excefs of the epact above 30 is accounted the true epact for that year. Thus when the epact would amount to 31, 32, 33, 34, *&c.* the 30 is rejected, and the epact becomes 1, 2, 3, 4, *&c.*

Since therefore the lunar year will have loft a day more than ordinary in refpect of the folar year, whenever it is neceffary to fet the golden numbers one day back, as was before obferved; it follows, that the epact muft at the fame time be increafed by an unit more than ufual; the difference betwixt the folar and the lunar year having been juft fo much greater than ufual. That is, 12 muft be added, inftead of 11, to the epact of the preceding, in order to form what will be the epact of the then prefent year. Which addition of an unit extraordinary to one epact, will occafion all the fubfequent epacts (which will follow each other in the ufual manner, each exceeding the foregoing by 11) to be greater by an unit than their refpectively correfponding epacts of the preceeding 19 years.

If therefore, inftead of the golden numbers, the epacts of the feveral years were prefixed, in the manner the *Gregorians* have done, to the days of the calendar, in order to denote the days on which the new moons fall in thofe years whereof thofe numbers are the epacts; there would never be occafion to fhift the places of thofe epacts in the calendar; fince the augmentation by an unit extraordinary of the epacts themfelves would anfwer the purpofe, and keep all tolerably right. Thus in a very eafy method may the courfe of the new moons be pointed out, either by the golden numbers, or by the epacts, according to the *Julian* account or manner of adjufting the year, which goes on regular and uniform without any variation.

But the regulating thefe things for thofe who ufe the *Gregorian* account, is an affair of more intricacy; and for them it will require more confideration to determine, when the epacts are to be more than ordinarily augmented, and at what times they are to continue in their ufual courfe; nay, to know when they are not only not to be extraordinarily augmented, but alfo when they are to be diminifhed by an unit, by increafing one of them by 10 only inftead of 11 as ufual: and this happens much oftener with the *Gregorians*, than the increafing one of them by 12 inftead of 11. For, in every *Gregorian* folar year, whofe date confifts of any number of entire hundreds not divifible by 4, it is fuppofed that the equinox has anticipated one whole day; and therefore one day, that which ought to be the intercalary one, is omitted; and confequently the preceding folar year, where one day was loft, exceeded the lunar year by 10 days only inftead of 11.

In order therefore to adapt the beforemention'd rule to the *Gregorian* account, and to know in what years the epacts fhould either be extraordinarily augmented or diminifhed, and the golden numbers fhould either be fet backwards or forwards in the calendar; the following rules and directions muft be obferved.

Firft. That in the years 1800, 2100, 2700, 3000, *&c.* where the number of entire hundreds is divifible by 3, but not by 4, the *Gregorian* folar, as well as the lunar year, will have loft a day; and confequently the

the difference betwixt them will be the same as usual: therefore in those years there must be no alteration, either in the epacts or the golden numbers; but the former must go on in the same manner, and the latter stand prefixed to the same days in the calendar, for another, as they did for the last hundred years.

2dly. The like will happen in the years 2000, 2800, 3200, *&c.* where the number of entire hundreds is divisible by 4, but not by 3: For neither the *Gregorian* solar nor the lunar year is to be altered; and therefore the epacts must go on, and the golden numbers stand, as they did before.

But 3dly, In the years 2400, and 3600, whose number of entire hundreds is divisible both by 3 and 4, the *Gregorian* solar year goes on as usual, and the lunar year has lost a day. The difference therefore betwixt them being 12, the epact of the preceding year must be augmented by that number instead of 11, in order to form the epact of the then present year; whereby a new set of epacts will be introduced, exceeding their precedent corresponding epacts by an unit: and the golden numbers must be set one day back in the calendar.

4thly and lastly, In the years 1900, 2200, 2300, 2500, *&c.* where the number of hundreds is divisible neither by 3 nor 4; the *Gregorian* solar year having lost one day, and the lunar none, the difference betwixt them being only 10; that number only, and not 11 is to be added to the epact of the preceding, in order to form the epact of that, the then present year; whereby a new set of epacts will be introduced, all of them less by an unit than their precedent corresponding epacts: and the golden numbers must be set a day forwarder in the calendar; that is, be prefixed to the day following that, against which they stood in the precedent hundred years.

This method would preserve a sort of regularity betwixt the solar and the lunar years; and, by means of the rules and directions beforementioned, the days of the new moons might be pointed out, either by the golden numbers or by the epacts, placed in the calendar for that purpose; according to the *Julian* account for ever, and according to the *Gregorian* account till the year 4199 inclusive, after which there must be some little variation made in the four last precepts or rules: but it would be to little purpose now, to attempt the framing of new set of rules for so distant a time.

The *Gregorians* have chosen to make use of the epacts to determine the days of the new moons, and follow pretty nearly the rules prescribed above; except that they order the epacts to have an additional augmentation of an unit 8 times in 2500 years, beginning with the year 1800, as at the end of 400 years; to which 400 years, if there be added 3 × 700, or 2100 years, the period of 2500 years will be completed in 3900. After which they do not make their extraordinary augmentation of an unit in the epacts, till at the end of another term of 400 years; which defers that augmentation from the year 4200 to the year 4300. And this

this is the reason that the rules above deliver'd will require a variation in the year 4200; whereas it is directed in this paper that the epacts should be augmented, or (which is the same thing) the golden numbers be set back in the calendar 9 times in 2800 years. This arises from the *Gregorians* supposing, that the difference betwixt 19 solar and as many lunar years would not amount to a whole day in less than 312 ½ years; whereas it has appeared above, that it would amount to a whole day in 310.7 years. But although the rule prescribed in this paper comes much nearer to the truth, yet the error in either case is very inconsiderable, being so small as not to amount to a whole day in many thousand years; and therefore is not worth regarding.

A method of finding the time of Easter, *as it is observed in most parts of* Europe.

From what has been already said, a method may be obtained, for fixing with sufficient exactness, the time of the celebration of the feast of *Easter*, which is governed by the *vernal equinox*, and by the age of the moon nearest to it. The former whereof, when once rightly adjusted, may (by the corrections mentioned in that part of this paper which relates to the solar year) be made to continue to fall at very near the same time with, or at most not to differ a whole day from the true *equinox:* and the same rules and directions, which, as was before shewn, would, without any great error, point out the times of the first day of the moon, would with equal certainty point out the 14th, 15th, or any other: and thus the times of the oppositions or the full moons might be as well marked out thereby, as those of the conjunctions or the new moons.

I shall not at present take notice of the canon of the Council of *Nice*, in 325, which directs the time of celebrating *Easter:* or of the reasons upon which that canon was founded. Nor shall I endeavour to explain the rule now in use in the Church of *England* for finding *Easter:* for, besides that such an explanation would extend this paper to an impropor length, those points have already been treated of by several much abler hands, and particularly by our countryman the learned Dr *Prideaux*. Nor is it my intention to enter far into the methods used by the *Gregorians*, or those of the Church of *Rome*, or by any other nations or countries, for finding the time of that feast. As to our own, I shall only observe, that the method now used in *England*, for finding the 14th day of the moon, or the ecclesiastical full moon, on which *Easter* dependeth, is, by process of time, become considerably erroneous: as the golden numbers, which were placed in the calendar, to point out the days on which the new moons fall in those years of which they are respectively the golden numbers, do now stand several days later in the same than those new moons do really happen. Which error, as was before observed, arises from the anticipation of the moons since the time of the Council of *Nice:* and as the *vernal equinox* has also anticipated 11 days since that time; neither that equinox, nor the new moons, do now happen on those days upon which the Church of *England* supposes them so to happen.

When Pope *Gregory* XIII. reformed the *Julian* solar year, he likewise made a correction as to the time of celebrating the feast of *Easter*, by placing

placing the epacts (which he directed to be made uſe of for the future inſtead of the golden numbers) much nearer to the true times of the new moons than the golden numbers then ſtood in the old calendar: I ſay, *much nearer to the true times*; becauſe in fact the epacts, as placed by him, were not prefixed to the exact days upon which the new moons then truly fell. And this was done with deſign, and for a reaſon which it is not material to the purpoſe of this paper to mention.

But the Church of *England*, and that of *Rome* or the *Gregorians*, do ſtill agree in this; that both of them mark (the former by the golden numbers, and the latter by the epacts correſponding to them) the days on which their eccleſiaſtical new moons are ſuppoſed to happen: and that 14th day of the moon incluſive, or that full moon, which falls upon, or next after, the 21ſt of *March*, is the paſchal limit or full moon to both: and the *Sunday* next following that limit or full moon, is by both Churches celebrated as *Eaſter* day. But the 21ſt of *March* being reckoned, according to the *Gregorian* account or the new ſtyle, 11 days ſooner than by the *Julian* account or the old ſtyle, which is ſtill in uſe among us; and their eccleſiaſtical new moons being 3 days earlier than thoſe of the Church of *England*; it happens that although the Church of *England* and that of *Rome* often do, yet more frequently they do not, celebrate the feaſt of *Eaſter* upon the ſame natural day.

It might however be eaſier for both, and could occaſion no inconvenience, now that Almanacks, which tell the exact times of the new moons, are in moſt peoples hands; if all the golden numbers and epacts now prefixed to thoſe days of the calendar, in our book of Common Prayer, and in the *Roman Breviary*, on which the reſpective eccleſiaſtical new moons happen, were omitted in the places where they now ſtand; and were ſet only againſt thoſe 14th days of the moon, or thoſe full moons, which happen betwixt the 21ſt of *March* and the 18th of *April*, both incluſive. Since no 14th day or full moon, which happens before the 21ſt of *March*, or after the 18th day of *April*, can have any ſhare in fixing the time of *Eaſter*. By which means the trouble of counting to the 14th day, and the miſtakes which ſometimes ariſe therefrom, would be avoided.

We do as yet in *England* follow the *Julian* account or the Old Style in the civil year; as alſo the old method of finding thoſe moons upon which *Eaſter* depends: both of which have been ſhewn to be very erroneous.

If therefore this nation ſhould ever judge it proper to correct the civil year, and to make it conſormable to that of the *Gregorians*, it would ſurely be adviſable to correct the time of the celebration of the feaſt of *Eaſter* likewiſe, and to bring it to the ſame day upon which it is kept and ſolemnized by the inhabitants of the greateſt part of *Europe*, that is, by thoſe who follow the *Gregorian* account. For tho' I am aware that their method of finding the time of *Eaſter* is not quite exact, but is liable to ſome errors; yet I apprehend, that all other practicable methods of doing it would be ſo: and if they were more free from error, they would probably be more intricate,

and

and harder to be underftood by numbers of people, than the method of determining that feaft either by a cycle of epacts, as is practifed by the *Gregorians*, or by that of 19 years or the golden numbers, in the manner propofed in the following part of this paper: and it is of no fmall importance, that a matter of fo general a concern, as the method of finding *Eafter* is, fhould be within the reach of the generality of mankind, at leaft as far as the nature of the thing will admit.

For which reafon, in cafe the legiflature of this country fhould before the year 1900, think fit to make our civil year correfpond with that of the *Gregorians*, and alfo to celebrate all the future feafts of *Eafter* upon the fame days upon which they celebrate them; this laft particular might be eafily effected, without altering the rule of the Church of *England* for the finding of that feaft: and this only by advancing the golden numbers, prefixed to certain days in the calendar, 8 days forwarder for the new moons, or 21 days forwarder for the 14th days or full moons, than they now ftand in our calendar.

In order to explain this, it muft be obferved, that the *Gregorian* account, or the new ftyle, is 11 days forwarder than the *Julian* account, or the old ftyle, which we ftill make ufe of; that is, the laft day of any of our months is the 11th day of their next fucceeding month. If therefore their ecclefiaftical new moons fell on the fame days with thofe of the Church of *England*, the golden number 14, which now ftands againft the laft day of *February* in our, that is the *Julian*, calendar, fhould, when we fhould have adopted the *Gregorian* calendar, be prefixed to the 11th of *March*. But fince their ecclefiaftical new moons happen 3 days, earlier than our ecclefiaftical new moons at prefent do; fo much fhould be deducted from thofe 11 days, by which the golden numbers ought otherwife to be advanced; and the golden number 14 fhould not be placed againft the 11th, but the 8th of *March*: which being reckoned the firft day of the moon, if we count on to the 14th day of the fame inclufive, that would be found to fall on the 21ft of *March*; on which day the *Gregorian* pafchal limit or full moon will happen when the golden number is 14. And the like courfe fhould be taken with the reft of the 19 golden numbers; which ought to be placed 8 days forwarder than they now ftand, if they are to point out the new moon; or 21 days forwarder than they are at prefent, if they are to mark the 14th day of the moon or the full moon: the latter of which, as has been fhewn, would be more eligible, than to prefix thofe numbers to the days on which the new moons happen.

Thus may the rule and method now ufed in the Church of *England*, be moft eafily adapted to fhew the time of *Eafter*, as it is obferved by the *Gregorians*, till the year 1900, at which time, and at the other proper fucceeding times, if the golden numbers in the calendar fhall either be advanced or fet backward a day, according to the foregoing rules and directions for that purpofe, they will continue to fhew us the new or the full moons of the Church of *Rome* or the *Gregorian* calendar with great exactnefs, till the year 4199: when, as has been already mentioned, there muft be a little variation made in thofe rules and directions.

There is however one exception to thofe general rules and directions, which will be taken notice of in the next paragraph.

Upon thefe principles I framed the table accompanying this paper, and fhewing, by means of the golden numbers, all the *Gregorian* pafchal limits or full moons, from the reformation of the calendar, *&c.* by Pope *Gregory* to the year 4199 inclufive. Which fpace of time is therein divided into 16 unequal portions or periods; at the beginning of each of which, all the golden numbers, when once they fhall have been properly placed in the calendar, muft either be advanced or fet back one day, with refpect to the place where they ftood in the preceding period, agreeably to the foregoing rules: except thofe numbers which fhall happen to ftand againft the 4th and 5th of *April* to fhew the pafchal new moons, or againft the 17th and 18th of the fame month to mark out the pafchal full moons; both which numbers at fome times, and only one of them at others, muft keep the fame place for that, which was allotted to them in the immediately preceding period.

In order to determine at what times, and on what occafions, this exception is to take place; let it be obferved, that, in the months of *Jan. Mar. May*, and fome others in our prefent calendar, as well as in the table above-mentioned, fome of the golden numbers ftand double or in pairs, and follow one the other immediately; whilft others, on the contrary, generally ftand fingle and by themfelves.

Now, when any of thofe pairs, or 2 numbers which ufually accompany each other, happen, in purfuance of the foregoing rules, to be prefixed the one to the 4th and the other to the 5th of *April* for the new moons, or the one to the 17th and the other to the 18th of *April* for the pafchal limits or full moons: and when any of thofe numbers, which generally ftand fingle, are prefixed, according to the faid rules, to the 5th of *April* for the new moons, or to the 18th for the full moons: in thefe cafes thofe pairs or fingle numbers that are fo fituated, muft not be fet forward or advanced at the beginning of the next period, but muft keep their places during another period, if the foregoing rules direct all the golden numbers to be advanced a day; which muft be complied with in refpect to all the other golden numbers, except thofe fo fituated as above. Inftances whereof may be feen in the table, under the refpective periods beginning with the years 1900, 2600, 3100, and 3300.

But if, in conformity to the foregoing rules, all the golden numbers are to be fet one day backward; thofe pairs or fingle numbers, tho' fituated as is above-mentioned, muft not keep their places, but muft move one day backward like all the other golden numbers; as they may be feen to do in the periods beginning with the years 2400 and 3600.

To give a plain and intelligible account of the reafon, on which the directions now given with refpect to this exception are founded, would extend this paper, already too long, far beyond its due and proper bounds.

bounds. I ſhall therefore content myſelf with obſerving, that it depends chiefly upon the nature of the *Menſes Pleni* and *Menſes Cavi*, into which the lunar year is uſually divided: and that, in order to make uſe of the golden numbers for finding the time of the *Gregorian Eaſter*, it will be neceſſary not only to conform to the general rules laid down in the former part of this paper; but alſo to follow the directions juſt now given, with reſpect to the abovementioned exception to thoſe general rules.

But I ſhould not do juſtice to *Peter Davall*, of the *Middle Temple* Eſq; Secretary of the *Royal Society*, did I not here acknowledge, that, before I had ſo fully conſidered theſe matters as I have ſince done, I had the firſt hint of applying the golden numbers to find the *Gregorian* paſchal limit or full moon, from him; who has ſince that time compoſed and drawn up tables, *&c.* which may poſſibly be of conſiderable and general uſe in this nation hereafter.

A TABLE, ſhewing, by means of the Golden Numbers, the ſeveral days on which the Paſchal Limits or Full Moons, according to the *Gregorian* Account, have already happened, or will hereafter happen; from the Reformation of the Calendar in the Year of our Lord 1582, to the Year 4199 incluſive.

To find the Day on which the Paſchal Limit or Full Moon falls in any given Year; Look, in the Column of Golden Numbers belonging to that Period of Time wherein the given Year is contained, for the Golden Number of that Year; over-againſt which, in the ſame Line continued to the Column intitled *Paſchal Full Moons*, you will find the Day of the Month, on which the Paſchal Limit or Full Moon happens in that Year. And the *Sunday* next after that Day is *Eaſter* Day in that Year, according to the *Gregorian* Account.

Golden Numbers from the Year 1583 to 1699, and ſo on to 4199, all incluſive.																Paſchal Full Moons.	
1583 to 1699	1700 to 1899	1900 to 2199	2200 to 2299	2300 to 2399	2400 to 2499	2500 to 2599	2600 to 2899	2900 to 3099	3100 to 3399	3400 to 3499	3500 to 3599	3600 to 3699	3700 to 3799	3800 to 4099	4100 to 4199	Days of the Month, and Sunday Letters	
3	14	—	6	17	6	17	—	9	—	1	12	1	12	—	4	March	21. C
—	3	14	—	6	—	6	17	—	9	—	1	—	1	12	—	—	22. D
11	—	3	14	—	14	—	6	17	—	9	—	9	—	1	12	—	23. E
—	11	—	3	14	3	14	—	6	17	—	9	—	9	—	1	—	24. F
19	—	11	—	3	—	3	14	—	6	17	—	17	—	9	—	—	25. G
8	19	—	11	—	11	—	3	14	—	6	17	6	17	—	9	—	26. A
—	8	19	—	11	—	11	—	3	14	—	6	—	6	17	—	—	27. B
16	—	8	19	—	19	—	11	—	3	14	—	14	—	6	17	—	28. C
5	16	—	8	19	8	19	—	11	—	3	14	3	14	—	6	—	29. D
—	5	16	—	8	—	8	19	—	11	—	3	—	3	14	—	—	30. E
13	—	5	16	—	16	—	8	19	—	11	—	11	—	3	14	—	31. F
2	13	—	5	16	5	16	—	8	19	—	11	—	11	—	3	April	1. G
—	2	13	—	5	—	5	16	—	8	19	—	19	—	11	—	—	2. A
10	—	2	13	—	13	—	5	16	—	8	19	8	19	—	11	—	3. B
—	10	—	2	13	2	13	—	5	16	—	8	—	8	19	—	—	4. C
18	—	10	—	2	—	2	13	—	5	16	—	16	—	8	19	—	5. D
7	18	—	10	—	10	—	2	13	—	5	16	5	16	—	8	—	6. E
—	7	18	—	10	—	10	—	2	13	—	5	—	5	16	—	—	7. F
15	—	7	18	—	18	—	10	—	2	13	—	13.	—	5	16	—	8. G
4	15	—	7	18	7	18	—	10	—	2	13	2	13	—	5	—	9. A
—	4	15	—	7	—	7	18	—	10	—	2	—	2	13	—	—	10. B
12	—	4	15	—	15	—	7	18	—	10	—	10	—	2	13	—	11. C
1	12	—	4	15	4	15	—	7	18	—	10	—	10	—	2	—	12. D
—	1	12	—	4	—	4	15	—	7	18	—	18	—	10	—	—	13. E
9	—	1	12	—	12	—	4	15	—	7	18	7	18	—	10	—	14. F
—	9	—	1	12	1	12	—	4	15	—	7	—	7	18	—	—	15. G
17	—	9	—	1	—	1	12	—	4	15	—	15	—	7	18	—	16. A
6	17	17	9	—	9	—	1	12	12	4	15	4	15	15	7	—	17. B
14	6	6	17	9	17	9	9	1	1	12	4	12	4	4	15	—	18. C
																—	19. D
																—	20. E
																—	21. F
																—	22. G
																—	23. A
																—	24. B
																—	25. C

XXII.

Part of a letter from Leonard Euler, *Prof. Math. at* Berlin, *and F. R. S. To the Rev. Mr* Caspar Wetstein, *Chaplain to the Prince of* Wales, *concerning the gradual approach of the* Earth *to the* Sun. *Translated from the* French, *by* T. S. *M. D. F. R. S.* No 493. p. 203. *dated* Berlin, June 28. 1749. *Read* Nov. 2. 1749.

XXII. Monsieur *le Monnier* writes to me, that there is, at *Leyden*, an *Arabick* manuscript of *Ibn jounis* (if I am not mistaken in the name, for it is not distinctly written in the letter), which contains a history of astronomical observations. M. *le Monnier* says, that he insisted strongly on publishing a good translation of that book. And as such a work would contribute much to the improvement of Astronomy, I should be glad to see it published. I am very impatient to see such a work which contains observations, that are not so old as those recorded by *Ptolemy*. For having carefully examined the modern observations of the sun with those of some centuries past, although I have not gone farther back than the 15th century, in which I have found *Walther*'s observations made at *Nuremberg*; yet I have observed that the motion of the sun (or of the earth) is sensibly accelerated since that time; so that the years are shorter at present than formerly: the reason of which is very natural; for if the earth, in it's motion, suffers some little resistance (which cannot be doubted, since the space through which the planets move, is necessarily full of some subtile matter, were it no other than that of light) the effect of this resistance will gradually bring the planets nearer and nearer the sun; and as their orbits thereby become less, their periodical times will also be diminished. Thus in time the earth ought to come within the region of *Venus*, and in fine into that of *Mercury*, where it would necessarily be burnt. Hence it is manifest, that the system of the planets cannot last for ever in it's (present) state. It also incontestably follows, that this system must have had a beginning: for whoever denies it, must grant me, that there was a time, when the earth was at the distance of *Saturn*, and even farther; and consequently that no living creature could subsist there. Nay there must have been a time, when the planets were nearer to some fixt stars than to the sun; and in this case they could never come into the solar system. This then is a proof, purely physical, that the world, in it's present state, must have had a beginning, and must have an end. In order to improve this notion, and to find with exactitude, how much the years become shorter in each century; I am in hopes that a great number of older observations will afford me the necessary succours.

Part of a letter from Mr Professor Euler *to the Rev. Mr* Wetstein, *Chaplain to the Prince, concerning the contraction of the* Orbits *of the* Planets. *Translated from the* French *by*

XXIII. I am still thoroughly convinced of the truth of what I advanced *, that the orbs of the planets continue to be contracted, and consequently their periodical times grow shorter. But in order to put this fact out of doubt, we ought to be furnished with good ancient observations, and also to be very sure of the time elapsed, since those observations, to this day: which we are not, with regard to the observations that *Ptolemy* has left us. For Chronologists, in fixing the moments of those observations, run into a mistake, by supposing the sun's mean motion to be known; which ought rather itself to be determined by these same observations. Now, if we reduce the days marked by *Ptolemy* to

* In the preceding paper.

the

T. S. *M. D. and F. R. S.* N° 494. p. 356. *dated* Berlin, Dec. 20. 1749. *Read* March 1. 1749.

the *Julian* calendar, we run the risque of committing an error of a day or two, in the whole number of days elapsed, from that to our time; because the course of the *Julian* years, according to which every 4th ought to have been *bissextile*, has been frequently interrupted by the *Pontifices*; of which we find some sure marks in *Censorinus* and *Dion Cassius*. Wherefore it might well happen, since the times marked by *Ptolemy*, that there has really been a day or 2 more than we reckon, and consequently, that *Ptolemy*'s equinoxes, ought to be put a day or 2 back; which would lengthen the years of those times. I was in hopes, that the *Arabian* observations would not be liable to this inconvenience; because the *Julian* calendar has not been interrupted for these last 1200 years. The late Dr *Halley* had also remarked, that the revolutions of the moon are quicker at present than they were in the time of the ancient *Chaldeans*, who have left us some observations of eclipses. But as we measure the length of years by the number of days and parts of a day, which are contained in each of them; it is a new question, whether the days, or the revolutions of the earth round it's axis, have always been of the same length. This is unanimously supposed, without our being able to produce the least proof of it: nor indeed do I see, how it could be possible to perceive such an inequality, in case it had really existed. At present we measure the duration of a day by the number of oscillations, which a pendulum of a given length makes in this space of time: but the Ancients were not acquainted with these experiments, whereby we might have been informed, whether a pendulum of the same length made as many vibrations in a day formerly as now. But even though the Ancients had actually made such experiments, we could draw no inferences from them, without supposing, that gravity, on which the time of an oscillation depends, has always been of the same force: but who will ever be in a condition to prove this invariability in gravity? Thus, even supposing that the days had suffered considerable changes; and that gravity had been altered suitably thereto, so that the same pendulum had always completed the same number of vibrations in a day; it would nevertheless be still impossible for us to perceive this inequality, were it ever so great. And yet I have some reasons, deduced from *Jupiter*'s action on the earth, to think, that the earth's revolution round it's axis continually becomes more and more rapid. For the force of *Jupiter* so accelerates the earth's motion in it's orbit round the sun, that the diminution of the years would be too sensible, if the diurnal motion had not been accelerated nearly in the same proportion. Wherefore, since we hardly at all remark this considerable diminution in the years, from thence I conclude, that the days suffer much the same diminution; so that the same number will answer nearly to a year.

A new method of making a Mural Quadrant, *which*

XXIV. The great usefulness of arches, firmly fixed to walls in the plane of the meridian, is well known to all who are the least acquainted with astronomical studies. Hence it comes to pass, that few observatories

ries are thought to be well furnished without one: but however it is found, that there is no wall so solid and immovable, and no bond of iron or other metal so strong, as to keep this instrument perfectly true with regard to the axis of the earth. I have thought therefore of a new contrivance, and propose a *mural arch*, furnished with a telescope and micrometer, to be constructed under the following conditions;

shall be free from many of the inconveniencies to which those now in use are subject, by Christian Lewis Gersten, *F. R. S.* N° 483. p. 507. March &c. 1747. *Read* May 7. 1747.

1. That it may be seen at any point of time, whether the plane of the instrument be placed vertically: and

2. Whether a perpendicular passes exactly through the centre of the quadrant, and beginning of the division of the limb.

3. That the aberration of the plane of the quadrant from the vertical line may be corrected, without altering the position of the beginning of the division on the limb, with regard to the perpendicular: and again that

4. The aberration of the beginning of the division on the limb, from the perpendicular may be corrected, without changing the notable position of the plane of the quadrant, with regard to the vertical line: in like manner

5. That the deviation of the plane of the quadrant from the plane of the meridian, may be amended without altering the perpendicular situation of the plane of the quadrant, and of the beginning of the division on the limb.

6. That it may be quite free from the variation that may be produced from the extension of the metals by heat and cold.

7. That it may easily be rectified; that is, that it may easily be seen, whether the line, passing from the object, through the intersection of threads in the tube to the eye, is exactly parallel to the line passing thro' the centre of the quadrant, and the division shewn by the rule, and to set it easily right when there is occasion; a business otherwise very laborious and difficult.

To obtain all these requisites, let there be

1. An iron *fulcrum a a a, c c b*, of which the part to be applied to the wall is described in *Fig.* 13. and the other in *Fig.* 14. It consists of an iron square *a a a*, and a transverse rule *c c*, strongly fastened with nails to the square. In *b*, the horizontal arm of the square is bent, that it may project behind, having besides, a round horizontal hole, the use of which will be shewn below, and another smaller vertical one, formed to receive the skrew *m*. Fig. 13, 14.

2. On the back of the *fulcrum*, at the vertical arm of the square, two ears *e h k*, and *d k*, are fastened with nails. The upper one *e h k* ends below in a cylinder *h*, and skrew *k*. The lower one *d g* at *g* is hollowed, and conically excavated in it's lower surface: and the *axis* of the cylinder *h*, and the *apex* of the cone *g*, must be in the same line; and that parallel to the anterior plane of the square *a a*, *Fig.* 14. but the utmost exactness in these is not necessary.

3. Into

3. Into the wall itself, parallel to the plane of the meridian, 3 iron corbels, *a b*, *g b*, *c e*, must be let in; and strongly fastened; two of which, *a b* and *g b*, are nearly equal, and in the same perpendicular line. The upper one *b a* has a cylindrical hole *b*, of a sufficient capacity to admit the cylinder *b* of the *fulcrum*, *Fig.* 14. The lower corbel *b g*, has instead of the hole, a conical *apex g*, to enter into the cavity of the ear *g*, *Fig.* 13. The distance between these corbels must be such, that the cylinder *b*, *Fig.* 13, 14, being let into the hole *b*, *Fig.* 16. and the *apex g*, *Fig.* 16. into the cavity *g*, *Fig.* 13. the whole weight of the *fulcrum* may be sustained by the *apex g*, and the *fulcrum* may be turned about horizontally with ease. Therefore the part *c* of the upper ear, *Fig.* 14. must not press upon the corbel, but be at some little distance from it. But to keep the *apex g*, *Fig.* 16. from slipping out of the cavity *g*, *Fig.* 13. a female skrew may be added to the male one *k*, by means of which, pressing the lower part of the corbel *a b* the *fulcrum* is sufficiently depressed vertically to the *apex g*.

Fig. 16.

4. That the cylinder *b*, *Fig.* 14. may be kept steady in the hole *b*, *Fig.* 16. let there be added another smaller horizontal skrew *f*, or 2 on the opposite sides, touching the cylinder in the hole. That the *axis* of the hole *b*, and the *apex g*, *Fig.* 16. may be in the same perpendicular, is no difficulty to effect in practice, because these corbels may be so disposed in the very building of the wall. First, the lower one *g b*, and then, the upper one *b a*, being set by a perpendicular, the line of which must pass through the *axis* of a brazen disk exactly filling the cavity of the hole *b*.

5. The third corbel *c e*, *Fig.* 16. consists of a thick male skrew standing out a good way from the wall. The hole *b*, *Fig.* 14. being of sufficient capacity to let this skrew pass, the upper part of the skrew must be taken off, that it may have a horizontal plane, on which the skrew in *Fig.* 14. may rest. Therefore the cylinder *b*, *Fig.* 14. being let into the hole *b*, *Fig.* 16. and the cavity *g* of the ear *d*, *Fig.* 14. being applied to the cone *g*, *Fig.* 16. and a female skrew being added at *k*, *Fig.* 14. and applied to the lower part of the corbel *a b*, *Fig.* 16. to the skrew of the corbel *c e*, let there be applied a plain female skrew, orbicular, and indented in the edge, that it may be the more easily turned by a key made on purpose, and brought near to the part of the corbel *c*. Then, by turning the *fulcrum* about horizontally, let the skrew *e*, *Fig.* 16. into the hole of the horizontal arm of the *fulcrum*, and turn the skrew *m* about, till it touches the plane of the thick skrew of the corbel *e*, and the corbel itself sustains some part of the weight of the *fulcrum*. Then let there be added another plain and indented female skrew to the male one *e*, *Fig.* 16. and let it be turned to the plane of the horizontal arm of the square, which is bent on purpose thus to receive this female skrew, that it may not hinder the suspension of the quadrant on the *fulcrum*, and that the greater length may be allowed to the thick skrew of the corbel *e*. Thus the part *b* of the *fulcrum*, *Fig.* 14. rests vertically on the skrew of the corbel *e c*, *Fig.* 16. and

and is kept in the azimuthal poſition by the 2 indented female ſkrews of the corbel *c e*. Now if any aberration happens in the poſition of the mural plane, it may at any time be corrected by means of theſe indented ſkrews The reader will eaſily imagine that the hole of the horizontal arm *b* muſt be large enough, and of an oval figure, that it's narrowneſs may not obſtruct the azimuthal motion.

6. The anterior part of the *fulcrum*, *Fig.* 14. has 3 corbels, *n*, *o*, and *p*. The firſt, *n*, is in form of a cube or parallelipiped, only it's upper ſurface is excavated ſemicylindrically. The ſecond, *o*, is in form of a hook, and is deſcribed ſeparately, in *Fig.* 15. The third, *p*, is only a prominent male ſkrew. They all muſt be fitted as firmly and exactly as poſſible. Fig. 15.

7. The quadrant itſelf muſt be of ſolid metal. It's anterior face is repreſented in *Fig.* 19. It muſt be of a ſufficient thickneſs, and properly exceeded by it's limb. To rectify the plane of the limb, there muſt be a rule compoſed *kk* of two, one of which *rr* is perpendicular to the plane of the other *kk*, ſo that it may not eaſily be bent to either ſide. The edge of the rule *rr* muſt be perfectly ſtrait, and ſhew the right line which is in the plane of the anterior ſurface of the limb. This rule is fixed to the back of the quadrant by ſkrews. Now if this rule falls in well with the plane of the limb in *m* and *n*, and another rectilinear rule to examine it is fixed to the centre, it will eaſily appear whether the limb and edge *rr* of the rule are in the ſame plane, and conſequently whether the plane of the limb is right. For only one right line can be drawn from the points *m* and *n*, which by the hypotheſis really exiſts in the the edge of the rule *rr*: and but one plane can be drawn through the right line *mn* and the point *a*. Now if the examining rule fixt at *a* exactly touches every where the edge of the rule *rr*, and the limb of the quadrant, the plane of the limb muſt neceſſarily be in the plane of the triangle *anm*. After examination and correction, this rule *rr* is ſuperfluous, and may therefore be taken away. Fig. 19. Fig. 20.

8. In the back of the quadrant, *Fig.* 20. let there be two brazen ſupports *ab* and *ef*, well faſtened with ſkrews to the ſurface of the quadrant. Let the ſupport *ab* have an oval hole in *b*; let there be 2 pointed ſkrews in *c* and *d*. The ſides of the hole are convex above, that the corbel *o*, *Fig.* 14. being let into this hole, the hollow part *a* of the corbel, *Fig.* 15. may be filled, and the points of the horizontal ſkrews, *Fig.* 20. *c* and *d*, may fit the lower convex part of the corbel or hook, *Fig.* 15. on the two oppoſite parts, that ſo there may be no danger of ſhaking.

9. Another ſupport *ef* fixt in *e* to the ſkrews in the plane of the quadrant, has a rectangular hole at *f*, which is entered by the vertical male ſkrews *h* and *i*, the uſe of which is as follows. The corbel or hook *o*, *Fig.* 14. being let into the hole of the ſupport *ab*, *Fig.* 20. the corbel *n* *Fig.* 14. is alſo let into the rectangular hole of the ſupport *ef*, and the *apex* of the ſkrew *h*, which ought to be hemiſpherically convex, ſtands in the cavity of the corbel *n*, *Fig.* 14. and ſo by the motion of the ſkrew *h*, the poſition

of the quadrant becomes ſomething variable by riſing and falling. The hole *f* ought to be pretty large for this purpoſe; but when once a convenient poſition is determined by the upper ſkrew, then the ſhaking of the quadrant becomes uſeleſs, and it is made faſt to the corbel by the lower ſkrew *i*.

10. By theſe two ſupports the quadrant may be kept in it's due poſition, and corrected when there is occaſion, with regard to the beginning of the diviſions on the limb, to make it agree with the perpendicular drawn thro' the centre of the quadrant. But the plane of the quadrant muſt alſo be perpendicular. To effect this there muſt be 2 plano-orbicular female ſkrews, embracing the male ſkrew *p*, *Fig.* 14; the firſt of theſe, which muſt be indented, muſt be applied to the male one *p* before the quadrant is applied to the corbels of the *fulcrum*. But when the quadrant hangs on the ſupports *c d* and *f e*, *Fig.* 20. and the ſkrew *p* is lodged in the hole *c*, *Fig.* 19. and 20, which muſt be ſufficiently large and of an oval figure, that it's poſition may be varied by the upper ſkrew *b*, *Fig.* 20. of the ear; then the back of the quadrant is ſuſtained by the indented orbicular female ſkrew, applied to the male one *p*, *Fig.* 14. above deſcribed, and the face by the other orbicular female ſkrew, which is to be turned about by a ſort of key thruſt into ſome little holes made on purpoſe. The plane therefore of theſe female ſkrews muſt be ſo large as not to enter the hole of the quadrant. And thus the quadrant is not only held tight on both ſides by theſe 2 ſkrews, but alſo can be moved backwards or forwards on occaſion, becauſe it's ſuſpenſion on the corbels *o* and *n*, *Fig.* 14. does not hinder this motion. But becauſe the too great length of the ſkrew *p* is on obſtacle, it will be proper to make the anterior female ſkrew of ſuch a form, as is deſcri-
Fig. 23. bed in the ſection *Fig.* 23. where the margin *a b* muſt touch the anterior face of the quadrant, and the neck *c d* muſt enter the hole *c*, *Fig.* 19.

11. The centre of the quadrant *a*, *Fig.* 19. is hollowed cylindrically to admit the joint of the rule. In the back of the quadrant, by means of the ſkrews, is fixt a plate *m n*, *Fig.* 20. having a ſquare hole *m*, anſwering to the centre; and let this ſquare be inſcribed in a circle of the hole *a*, *Fig.* 19. or a little leſs. The plate *m n*, *Fig.* 20. muſt have a proper thickneſs, and be doubly bent according to the ſquare, and end in the face in the part *b*, *Fig.* 19. diſtant enough from the plane of the quadrant, to hold a thin ſtyle which enters the centre of a pin, and the ſuſtaining
Fig. 21. thread of the perpendicular. The pin is delineated in *Fig.* 21. where *a* is the head, and *b* a cylinder exactly filling the cavity of the centre of the quadrant and rule, *c* a ſquare piece to be admitted into the hole *m*, *Fig.* 20. *d* a male ſkrew, to which a female one is to be fitted.

Fig. 22. 12. *Fig.* 22. ſhews the quadrant with the rule and *apparatus* of the perpendicular: *p r* is a line inſcribed on the ſurface of the quadrant, which would paſs thro' the centre if it was continued, where the beginning is of the diviſions on the limb. *h i k g* is a parallelipiped, hollowed as in the figure, faſtened by ſkrews to the plane of the quadrant, in which

which the thread *m o* hangs perpendicularly on the line *p r*, which may be easily performed. Another thread *i k* is parallel to the plane of the quadrant, but at a distance exactly equal to the height of the centre in the pin *c*. Let a third be added like the second in the opposite side of the parallelipiped *b g v*.

13. The thread, which is to be hung on the thin style that enters the centre of the pin *c*, is to be made of human hair, easily sustaining a weight of half an ounce *f*, and must swing freely in the cavity of the parallelipiped *b i k g*. The smaller threads of the parallelipiped *m o*, *i k*, *&c.* must be also made of the same hair, and have an equal thickness. The use of them is to shew easily the position of the quadrant with regard to the perpendicular *c f*. For by levelling by the eye thro' the thread *m o* to the line *p r*, one may judge exactly whether *p r*, the beginning of the divisions on the limb, agrees with the perpendicular *c f*. Again, by levelling thro' the thread *i k* to the other opposite, one may see whether the plane of the quadrant is parallel to the perpendicular. But if instead of the parallelipiped, 2 little bridges are substituted to sustain these 3 threads, the same end will be more shortly obtained, and as a small space is sufficient for the oscillation of the thread *c f*, the disposition of these 3 threads may be such, that the level may be taken by convex glasses; which will be convenient for those who have not good eyes. The orbicular margin of the female skrew *b*, and the male skrew of the *fulcrum p*, *Fig.* 14. which it embraces, should project so far beyond the surface of the quadrant, as not to hinder the oscillation of the line *c f*, or the place of those skrews and of the hole in the quadrant should be without the space of the oscillation of the line. But if the structure of the observatory will permit a view of the stars from the horizon to the zenith, then the rule ought to have a free access to the line *p r*, and so the parallelipiped *b i k g* must be placed a little lower into the appendix, or a little higher into the vertical arm, and the appendix itself ought to have a convenient incision.

14. I proceed now to the rule itself, which is drawn as to the greatest part scenographically in *Fig.* 22. and distinguished by the letters *n n n n*, observing a just magnitude and proportion of it's parts. I shall now give a particular explanation of the structure of this rule, because it is very peculiar. *n n n n* is the plane of the rule which turns about upon the pin *c*. The danger of it's bending is prevented by another rule *d d d*, to be strongly fastened perpendicularly to the plane *n n n*. The divisions of the limb are shewn in the aperture or window of the rule *x x*. *q q* is the telescope. Now if you would have the rule to shew exactly the degree *o* or 1 of altitude on the limb, there will be nothing wanting in the machine, but to have the line from the object thro' the decussation of the threads of the telescope to the eye, parallel exactly to the line passing thro' the centre of the quadrant, and the degree *o* or 1 of altitude on the limb. But as the tube cannot be fix'd at first after this manner to the rule, I would have it so connected therewith, that in the

 position

poſition deſcribed in *Fig.* 22. it may have ſome ſort of motion, not only to the altitude, but alſo to parts of the azimuth. The motion to the altitude is performed by the ſkrews *u u*, and the motion to the azimuth by the ſkrews *w w*, all which are more particularly and
Fig. 25. diſtinctly delineated in *Fig.* 25. *& ſeq.*

15. The motion to the altitude is effected after the following manner.
Fig. 30. A foot or little bridge, *Fig.* 30. is faſtened to the tube in a convenient place; the baſe *b c k* is plain and rectangular, at *b* and *c k* the thickneſs of the metal is leſs. The part *i* is perpendicularly faſtened to the baſe, to this is connected another voluble part *n a-m h*, by means of the ſtyle *h*, which paſſes thro' the joints of both parts. In *a* the voluble part is excavated, bending in ſuch a manner as to receive the tube faſtened into this canal with tin. The baſe of this foot *b c k* is faſtened to the plane
Fig. 31. of the rule by two brazen pieces, *Fig.* 31. which I call *depreſſors of the foot*. Theſe depreſſors have 2 holes, *k* and *m*, to receive the ſkrews and the 2 *apices h* and *i*, which are to be thruſt into little holes perforated for this purpoſe in the plane of the rule. All this will be better under-
Fig. 26. ſtood by the ichnographical horizontal delineation *Fig.* 26, where *a a* is part of the tube, *b c*, *c e* the foot, *e e* the baſe of the foot, *b c* the voluble part of the foot faſtened to the tube, *f f* the 2 depreſſors of the foot, *g g* male ſkrews, which enter *matrices* excavated in the rule, and in this manner depreſſing the foot at will to the plane of the rule. *Fig.*
Fig. 29. 29. ſhews the vertical delineation; *a a* part of the tube, *e e*, *f f* ſkrews of the depreſſors: and the foot itſelf is hid under the tube: *g*, *h* are 2 male ſkrews, the ends of which are plain, and keep the foot cloſe and unmoveable, and when the horizontal poſition of the tube is to be altered by elevating or depreſſing, it is eaſily performed by the revolutions of theſe ſkrews: but then the ſkrews of the depreſſors of the foot *f f*, *e e*, muſt not depreſs the foot too ſtrongly to the plane of the rule.

16. Now follows the motion to the parts of the azimuth, which be-
Fig. 33. ing delineated *Fig.* 33. we ſhall call the *tabula plicata*. It conſiſts of a rectangular plane of braſs *k f g h*, on which reſts at right angles another plane *a b c f*, the ſides of which, *a f* and *b c*, are in the curvature of a circle drawn from the centre of the hole *q*, the margins, *a f* and *d b c g*, are hollowed at right angles, in the ſame manner as the baſe of the foot *Fig.* 30. To this muſt anſwer 2 depreſſors like that in *Fig.* 31. only theſe, *b c*, muſt have a proper curvature. The plane *k f g h* has rectangular apertures *m r n* and *o ſ p*, to receive the appendages of the
Fig. 32. tube, which I ſhall deſcribe preſently. In *Fig.* 32. *a a* is part of the tube, *e e* the vertical part of the *tabula plicata*, faſtened to the plane of the rule by means of the pin *b*, and turning about the pin, *c c* and *d d*, the depreſſors of the table and their ſkrews. *b b* is a ſection of
Fig. 33. part of the horizontal table, which is mark'd in *Fig.* 33. by the letters *k f g h*. *g g* are peculiar appendages, faſtened to the tube, which end in male ſkrews, and part of this anſwers to the quadrangular perforations *m n* and *o p*, *Fig.* 33. ſo that according to the length *m r* or *o s*, they

they may be removed this way and that at any convenient diſtance, and yet by means of the female ſkrews *k k*, the little orbs *i i* being interpoſed beforehand, be ſtrongly faſtened whenſoever you pleaſe to the plane of the table. But if, *Fig*. 25. the teleſcope is raiſed or depreſſed by the ſkrews *u u* againſt the ſides of the foot, this is permitted by the pin of the complicated table, the ſkrews of the depreſſors *x x* being a little looſened. Again when it is to be performed to the azimuthal motion, the appendages *w w*, the female ſkrews being on each ſide relaxed, conſequently the tube itſelf is moved forward, which is alſo permitted by the juncture of the foot. But if the appendages *g g*, *Fig*. 32. are connected tranſverſly by a ſtrong piece of metal, this azimuthal motion may be rendered eaſy and ſecure by the uſe of one ſkrew.

17. Beſides the rule has a peculiar appendage, which ſweeps the back of the limb. It conſiſts, *Fig*. 25, and 26. of a part bent at right angles *A m*, very ſtrongly faſtened to the plane of the rule, and another voluble one *k o*, with a ſtyle *m*, connected by it's joints with the immoveable *A m*, in the voluble part the little orb *i*, *Fig*. 26. turning about the cylinder *q*, which ends in a ſkrew, ſweeps the back of the limb, and is preſſed againſt it by the *clauſtrum n s* with the ſkrews *s s s s*, *Fig*. 25. The rule being then moved to any diviſion is faſtened by the ſkrew *v*, to which is objected a thin plate *u t*, *Fig*. 26. which hinders an immediate contact, that the point may not excavate the metal of the limb.

18. In the glaſſes of the teleſcope I do not require what they call a centration, that is, that the greateſt thickneſs may be in the middle of the glaſs, a tedious and laborious buſineſs. I would only have the glaſſes, eſpecially the object glaſs, have a conſtant ſituation in the tube, to which, if they are taken out to be cleaned, they may eaſily be reſtored. The *Engliſh* artificers commonly fix the eye glaſſes, eſpecially of reflecting teleſcopes, ſtrongly in cylinders cut ſpirally, and ſo place them in the tube with fit *matrices*. But if the ſpires are good, and a mark is made in the margin of the cylinder, anſwering to another made in the cavity of the tube, the glaſs will neceſſarily keep the ſame ſituation, tho' it be moved 100 times, provided it be inſerted again into the tube, ſo that one mark may anſwer the other.

19. The micrometer, *Fig*. 28. has a neck, the margin of which is Fig. 28.
ſcrupulouſly divided into 8 equal parts by lines converging to the centre, or if more vertical threads are required, into as many other parts as you pleaſe. To theſe diviſions are eaſily applied threads either of ſilk or metal, and are faſtened in the neck either by wax, or very thin pins made either of box or metal. *a* is a quadrangular prominence, having another like it in the oppoſite part, but either greater or leſs: let the thickneſs of both be equal to the thickneſs of the tube into which they are inſerted, or a little leſs. The tube itſelf is delineated in *Fig*. 27. Fig. 27.
In *d* it has a rectangular notch, receiving the prominence of the ring *a*, *Fig*. 28. without ſhaking. The ring of the micrometer muſt be well fitted

fitted to the cavity of the tube, and when inferted muft exactly touch it's inner fides. *b* is an eye glafs, at a due diftance from the threads of the micrometer, *f* the *foramen oculare* cut into a cochleated *operculum b*. When this *operculum* is removed, another muft be fubftituted furnifhed with fmoaked glaffes. That the horizontal thread of the micrometer may be always in the fame fituation, or if difturbed be eafily reftored to
Fig. 29. it's place, let the greater tube of the telefcope, *Fig.* 29. end in a cufp *i*. Laftly, when the ocular tube *b* is inferted, and reduced to it's due fituation, let a line be drawn on the external furface of the tube *b*, to the direction of the margin of the horizontal cufp *i*, and afterwards let thefe 2 tubes, *a* and *b*, be faftened to each other by very fmall fkrews. But if you defire a micrometer furnifhed with a moveable thread, then the ftructure muft be conveniently altered.

20. To proceed now to the rectification of the rule; a plank of thick and folid wood muft be provided, with a horizontal furface of nearly the length and breadth of the rule. In the extreme part of the horizontal furface let a brazen pin be vertically erected, ending at the top in a fkrew, and exactly filling the cavity of the central hole. Let it have one of the extremes plain, or a parallelipipedal and prominent
Fig. 24. brazen table. *h h*, *Fig.* 24. denotes part of a plank, *f e g k* a prominent brazen table, well faftened to the plank by fkrews *e e e e*, but fo that the heads of the fkrews may not appear above the furface of the table. Let the upper furface of the metal be well polifhed, and agree with the upper plane of the plank, and let there be a thin line *a b* drawn upon it, which if continued would pafs thro' the *axis* of the pin. Let there be alfo two brazen parallelipipeds, *c d*, faftened to the table with 2 fkrews, but at a proper diftance from the extremity *g k*, as may be collected from what follows. Let each of them have beneath 4 cylindrical *apices* thruft into holes bored in the table, that their fituation may be as firm us poffible; let thefe 2 parallelipipeds touch each other exactly, and let the plane of contact be perpendicular to the line *a b*. Now therefore if the divifions of the limb of the quadrant are fhewn by the inner margin of the aperture *x x*, *Fig.* 22. or by fome line extended thro' that aperture, then the rule of the quadrant is fo laid upon the plank, it's upper furface being firft fo placed horizontally, that the vertical pin of the plank may pafs thro' the central hole, the telefcope looking upwards, and fo the parallelipiped *d*, *Fig.* 24. being removed, the margin of the aperture *x x* fhewing the divifions *Fig.* 22. or the extended line may be exactly applied to the perpendicular furface of the parallelipiped *c*, *Fig.* 24. And when this is done, the margin of the aperture, or the extended thread, will be in a plane perpendicular to the line *a b*. Moreover the rule
Fig. 25, 26. being faftened to the table *f g* by means of the fkrew *v*, *Fig.* 25, 26. let the telefcope with the plank be directed to any remote object, immoveable; and let the point therein be noted, which is covered by the decuffation of the threads, and let the plank remain unmoved in that pofition. Afterwards let the rule be inverted, the parallelipipedon *c* be

removed

removed and reſtored to it's former place *d*, *Fig.* 24; and let the margin of the aperture *x x*, *Fig.* 22. or the thread be applied to the perpendicular plane of the parallelipiped *d*. And as in this ſtate the left part of the rule reſts upon the plank, and ſo it is neceſſary that the reſt ſhould preponderate with the teleſcope, a prop furniſhed with ſkrews muſt be combined with the plank. Therefore when the rule is applied in it's inverted ſituation to the ſame perpendicular plane of the line *a b*, *Fig.* 24. you muſt again view the object thro' the teleſcope, and ſee whether the point of decuſſation of the threads is in the ſame point of the object as before. If this is the caſe, the rule has no need of correction. For when the right line *a b* is unmoved, paſſing thro' the centre of the pin of the rule, and the ſame point of the object appears thro' the teleſcope both in a right and an inverted poſition, the line paſſing from the object to the eye thro' the decuſſation of the threads muſt neceſſarily be parallel to the line *a b*. But as this caſe will hardly ever happen at the firſt trial, but the croſſing threads will generally touch another point of the object, the error may be either in the altitude, or azimuth, or both together. In each caſe the poſition of the teleſcope is to be corrected to half the angle of aberration by the ſkrews *u u* and *w w*, *Fig.* 22. as far as this can be determined by the judgment of the eyes. Then the rule is to be laid on again in a right ſituation as before, and the object to be viewed anew, and then you muſt invert it again, and ſee whether the ſame point appears. If not, the poſition of the teleſcope muſt be again corrected by the quantity of half the error. This examination muſt be repeated, as often as any difference appears. When this is done, all the ſkrews *d d*, *c c*, *k k*, *Fig.* 32. and *e e*, *f f*, *g b*, *Fig.* 29. muſt be Fig. 32.
made faſt, that the teleſcope may remain in that ſtate. If this manifold Fig. 29.
inverſion of the rule on the plank, which may however be performed in a reaſonably ſhort time, ſeems too tedious, he may add a micrometer with a moveable thread, or rather a white table, *b c d e*, *Fig.* 18. which has 2 black *faſciæ*, *h l*, *g f*, croſſing each other at right angles, in a horizontal and vertical ſituation, which he may diſpoſe at a convenient diſtance, and then ſo direct the plank with the teleſcope laid on it in a right ſituation, that the point of decuſſation of the *faſciæ* may be in the point of decuſſation of the threads in the tube. The rule being inverted, without moving the plank, an aſſiſtant muſt be near the table, to perform the directions of the obſerver by ſigns, and he muſt have 2 other black *faſciæ*, *k m* and *n o*, which he may move at the beck of the obſerver, in a ſituation parallel to *g f* and *h l*, which may eaſily be done by ſome peculiar ſtructure in the plank, *n o* horizontally and *k m* vertically. But when the rule is inverted on the plank, if the point of decuſſation falls on a point of the table, for inſtance *g*, then the aſſiſtant muſt ſo diſpoſe both the *faſciæ k m* and *n o* ſucceſſively, that the vertical thread of the tube may fall on the *faſcia b e*, and the horizontal one of the tube on the *faſcia* of the table *k m*. When this is finiſhed the in-

tervals

tervals *f o* and *k h* are to be bisected, and the *fasciæ* on the table to be placed by a motion parallel to those points of bisection. Then, without moving the plank, the rule with the telescope is restored to it's right situation, and the position of it rectified by the skrews, till the point of decussation of the *fasciæ* on the table, coincides with the point of decussation of the threads in the tube. But if you look at the table, the first time with the rule and tube inverted, and the second time in the right situation, mark the point of decussation on the table, and by putting the moveable *fasciæ* in the right place, the error of the rule may be corrected in the same situation. And so the position of the tube with regard to the rule will be such, that the same point of the object may be seen in the tube, either in the right or inverted position of the telescope, and consequently the rule of the quadrant will be rectified.

21. But if the rule cannot be hindered from being too heavy, for a convenient direction of it to the stars, there are two ways of remedying this inconvenience. The first is that which *Flamstead* long ago applied to his sextant, and described in his *Hist. Cælest.* The exterior limb of the quadrant is cut with correspondent notches, and swept by the perpetual skrew in the versatile appendage of the rule. But then the apparatus of the appendage, *s q*, *Fig.* 26. should not be omitted, that the plane of the rule may be exactly contiguous to the plane of the limb. For in the mural sextant at *Petersburg*, made by the famous *Rowley*, I observed this defect, that the margin of the aperture *x x*, *Fig.* 22. which shews the divisions of the limb, presses the limb indeed very well, but the other part of the rule is too far distant from the plane of the limb, so that the telescope shakes, though the skrew be turned ever so close. But this whole artifice seems to me to be too laborious, and not convenient enough in observations. For it does not appear to me safe enough, either to examine, or correct, or compare, the divisions of the limb by the revolutions of the skrew; but to raise the weight of the rule, and by this artifice to cause a more easy direction of the tube to the stars, is abundantly too laborious. I would choose to make use of one 100 times more
Fig. 17. simple. Let *a b c*, *Fig.* 17. be a mural quadrant, *a d* the rule; in *m*, vertical to the centre *a* of the quadrant, let there be an axis of an iron bar, *g m f*, so that the arm *m f*, and it's revolution, may be very nearly in the plane of the quadrant, and the other *m g* in another distant parallel. Let the length *m f* be about $\frac{1}{4}$ the length of the rule *a d*, and the length *m g*, 3 or 4 inches. Let the angle *g m f* of the rotation *m*, and of the suspensions *g f*, be a right one. At *f* let the rule be connected by a little chain or small cord, so that *m f* may be equal and parallel to *a e*. At *g* let a small cord *g h* be fastened, and extended horizontally to the extremity of the room; there let it be supported by a pulley *h*, and let down almost to the ground, with the weight *k* hung to it. Now if this weight *k* is made sufficient for the rule, it will remain in any situation, whether it be elevated or depressed. It will be but a small obstacle, that the centre of gravity of our rule will be distant from the point of suspension, because

the preſſure of the elaſticity in the back of the limb, will ſufficiently moderate the unequal action of gravity.

22. It remains now to ſhew that the inſtrument deſcribed is ſufficient for all it's requiſites.

1. As the thread *ik*, *Fig*. 22. and the other ſimilar to it, in the oppoſite part of the parallelipiped have a ſituation parallel to the plane of the quadrant, and the ſame diſtance and point of ſuſpenſion *c*, it will eaſily appear by looking, whether the thread of the perpendicular is in the plane of theſe threads, and ſo the plane of the quadrant in a vertical poſition.

2. Becauſe the thread *mo*, is in the plane of the line *pr*, and that perpendicular to the plane of the quadrant, it will be eaſily known by looking through *mo* and *pr*, whether the perpendicular is in this plane, and conſequently, whether the beginning of diviſion *pr* is in a vertical plane.

3. Becauſe the quadrant is ſuſpended by 2 points, firſt, by the hook *o*, *Fig*. 14. and then in the cavity of the corbel *n*, nothing hinders it's vertical motion but the 2 female ſkrews, embracing the male one *p*. By theſe ſkrews, therefore, the ſituation of the plane of the quadrant to the perpendicular, may be corrected; and as by this correction, the horizontal arm of the quadrant will not be inclined, it follows, that this correction is independent on the horizontal ſituation of the quadrant.

4. The hook, *Fig*. 15. is not only concave in *a*, but alſo convex, ſo that it's ſection *ab* is circular: therefore this form of the hook does not hinder the quadrant from being a little raiſed or depreſſed in the corbel *n* by the ſkrew *b*, *Fig*. 20. Therefore, as the poſition of the beginning of diviſion, depends, at the ſame time, on the poſition of the horizontal arm, it is evident, by the motion of the ſkrew *b*, that the poſition of the beginning of diviſion, with regard to the perpendicular, may be corrected.

5. As the iron *fulcrum* itſelf has ſome horizontal motion in the corbels *ab* and *gb*, *Fig*. 16. and the *axis* of rotation is perpendicular, it follows, that all the reſt remaining, the deviation of the plane of the quadrant, from the plane of the meridian, may be corrected. And if even the *axis* of rotation of the *fulcrum* ſhould not be exactly perpendicular, yet it will not hinder the obſerver from diſcovering to what part the inclination of this *axis* tends; and ſo he may make his corrections as occaſion requires.

6. The quadrant itſelf is of one ſolid metal. Now if this is extended or contracted by heat or cold, it will always remain ſimilar to itſelf. Nor does the ſuſpenſion of it hinder it's extenſion. For in the *fulcrum*, the corbel *u*, *Fig*. 14. has a horizontal canal, in which the apex of the ſkrew *b*, *Fig*. 20. reſts, and the hole *c*, *Fig*. 19. is large enough for the ſmall extenſion or contraction which heat or cold produces, nor is the plain ſurface of the female ſkrews, which cover the hole on both ſides, and faſten the arm, any obſtacle.

7. Laſtly, as to the rectification, that ſufficiently appears from the precepts. Every one will underſtand, that it is more eaſy and expe-

ditious than thofe in ufe; and as the plank conftructed for the rectification of the rule may be preferved, the obferver may at any time without much labour examine his rule anew. Thus the inftrument anfwers all it's requifites.

But as it very rarely happens, that houfes are fo built, as to have walls in the plane of the meridian, or at leaft, places fit for conftructing thefe walls, the mural arches have hitherto required a building exprefsly difpofed for this purpofe. But any one will eafily underftand that our contrivance is applicable to almoft any place. If, for inftance, in fuch an opening as is ufually made for doors, 2 corbels *a b*, *g b*, *Fig.* 16. are let into the wall, the third *e c* does not require a wall exactly contiguous; but may be fixed ftrongly enough in a piece of iron projecting a good way from the plane of the wall.

Defcription *and* Ufes *of an* Equatorial Telefcope, *by Mr* James Short, *F. R. S. to the Pref.* No. 493. p. 241. Oct. *&c.* 1749. *Read* Dec. 7. 1749.

XXV. I have made 3 of thefe inftruments, one of which was bought by Count *Bentink* for the Prince of *Orange*; the other two I have ftill by me, one of which I fhall fhew to the Society. I do not pretend to any thing new in the combination of thefe circles, of which this inftrument confifts, the fame combination having feveral times been made before me by way of a dial: but I believe the putting fo large a telefcope upon this machinery, and applying it to the ufes which I have done, is fomewhat new.

Defcription and Ufes of the Equatorial Telefcope, or Portable Obfervatory. Fig. 34.

This inftrument confifts of 2 circular planes or plates, *AA*, which are fupported upon 4 pillars; and thefe are again fupported upon a crofs-foot, or pedeftal moveable at each end by the 4 fcrews *BBBB*: the two circular plates *AA* are moveable, the one above the other, and are called the horizontal plates, as reprefenting the horizon of the place; and upon the upper one are placed 2 fpirit-levels to render them at all times horizontal: thefe levels are fixed at right-angles to one another: this upper plate is moved by a handle *C*, which is called the horizontal handle, and is divided into 360°, and has a *Nonius* index divided into every 3′.

Above this horizontal plate there is a femicircle *DD*, divided into twice 90°; which is called the meridian femicircle, as reprefenting the meridian of the place, and is moved by a handle *E*, which is called the meridian handle, and has a *Nonius* index divided into every 3′.

Above this meridian femicircle is faften'd a circular plate, upon which are affixed 2 other circular plates *FF*, moveable the one upon the other, and are called the equatorial plates; one of them, reprefenting the plane of the equator, is divided into twice 12 hours, and thefe are fubdivided into every 10′ of time. This plate is moved by a handle *G*, called the equatorial handle, and has a *Nonius* index for fhewing every minute.

Above this equatorial plate there is a femicircle *HH*, which is called the declination-femicircle, as reprefenting the half of a circle of declination, or horary circle, and is divided into twice 90°, being moved by the handle

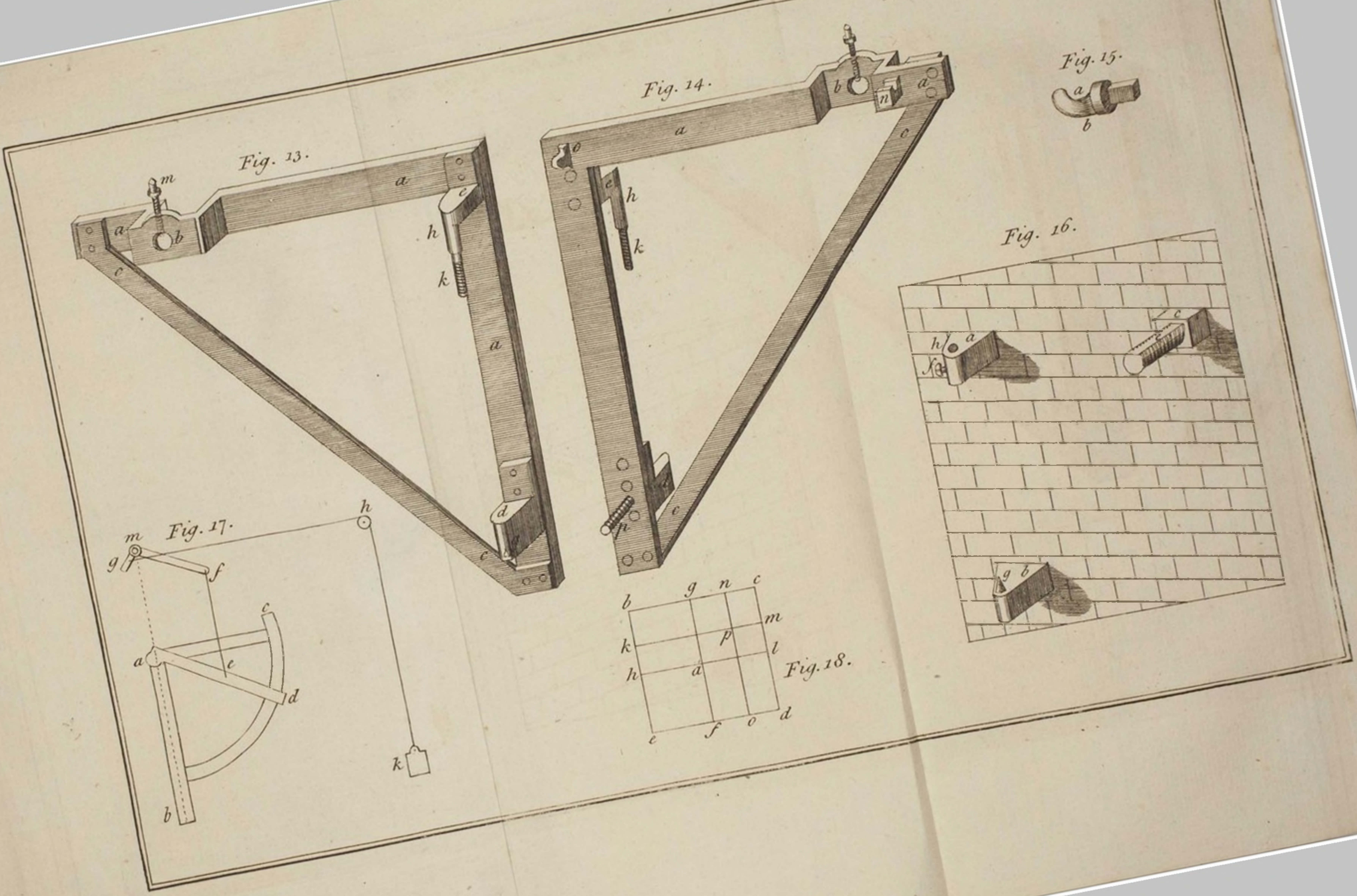
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.

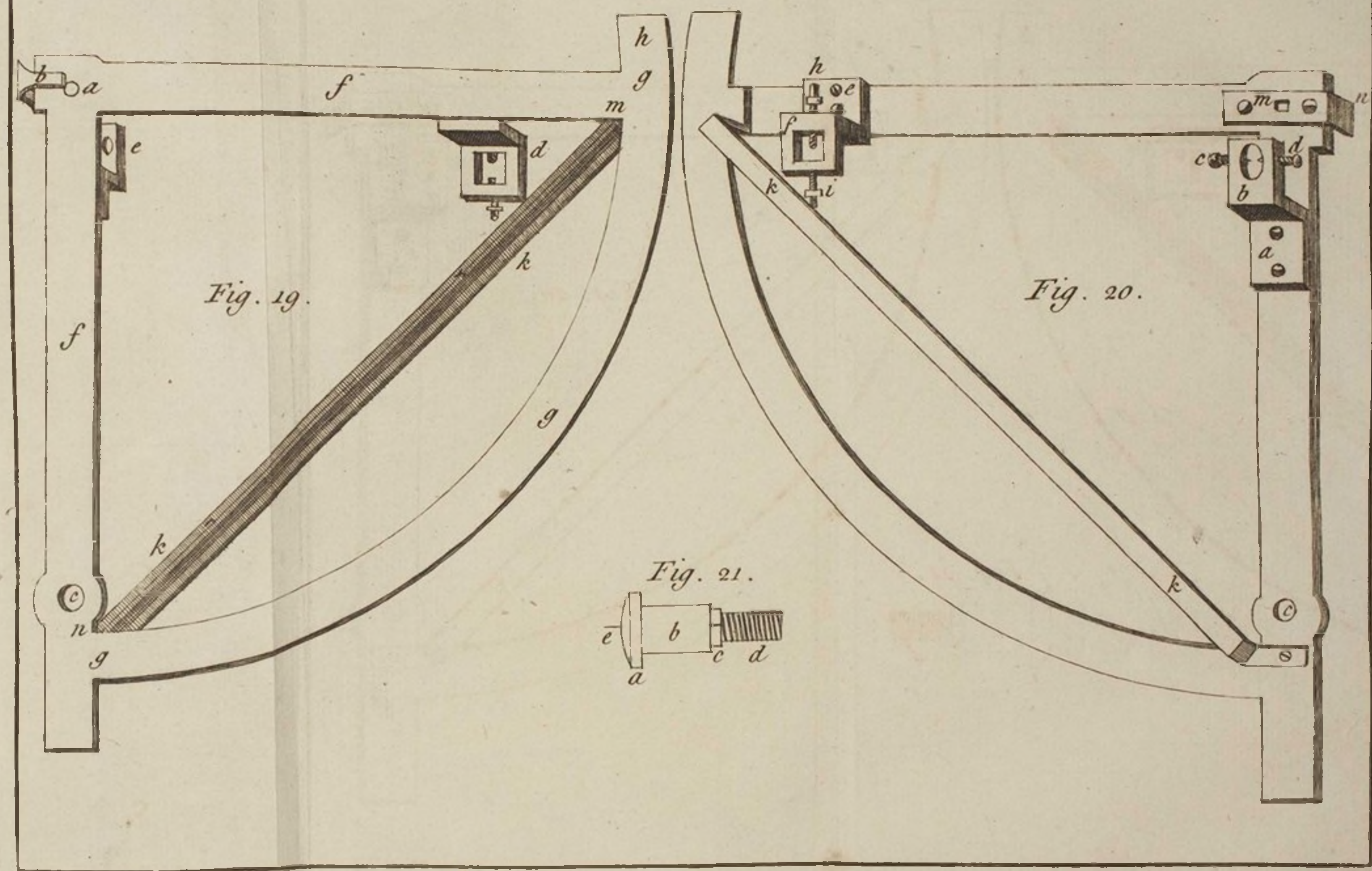
Fig. 19.
Fig. 20.
Fig. 21.

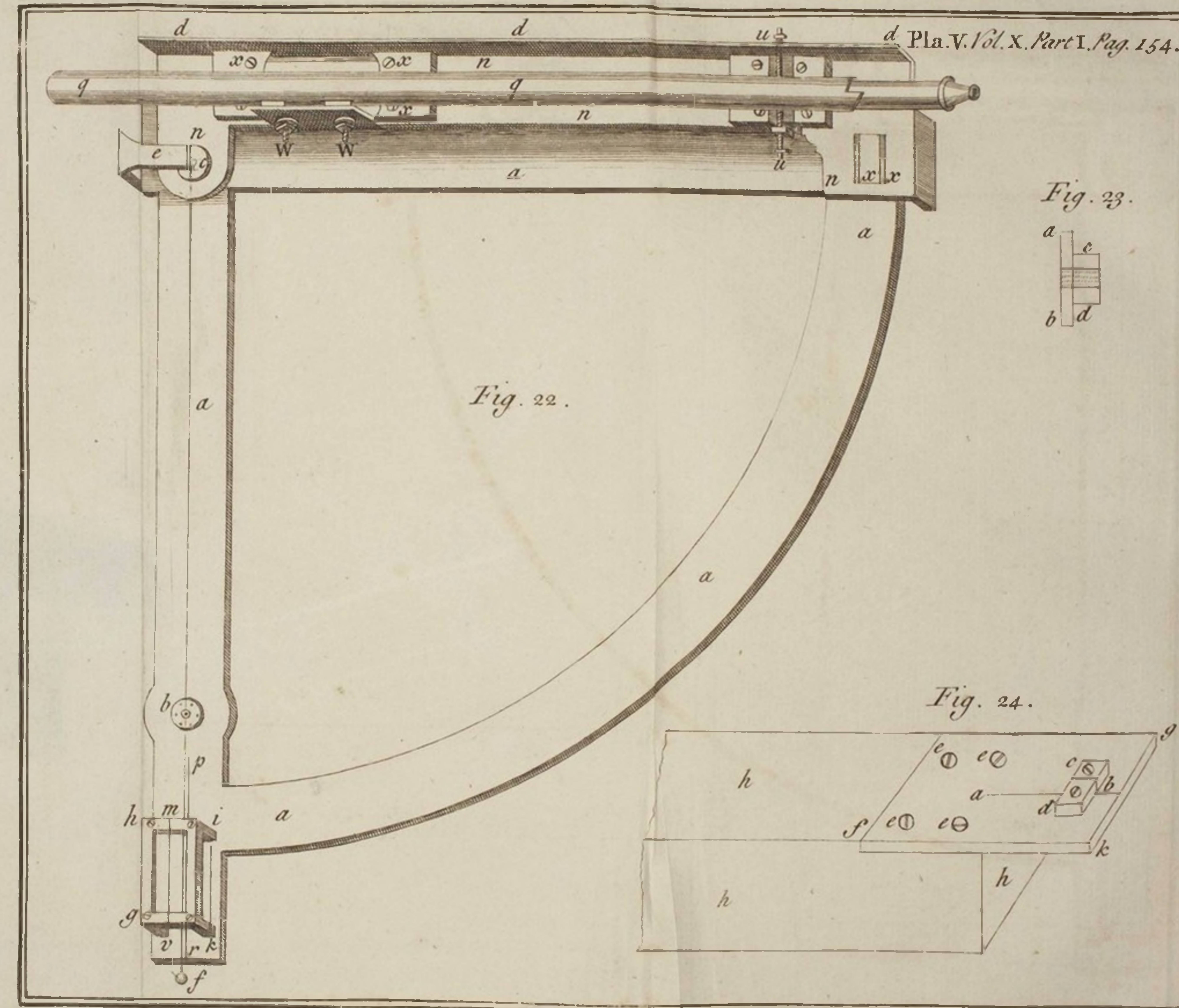
Fig. 22.
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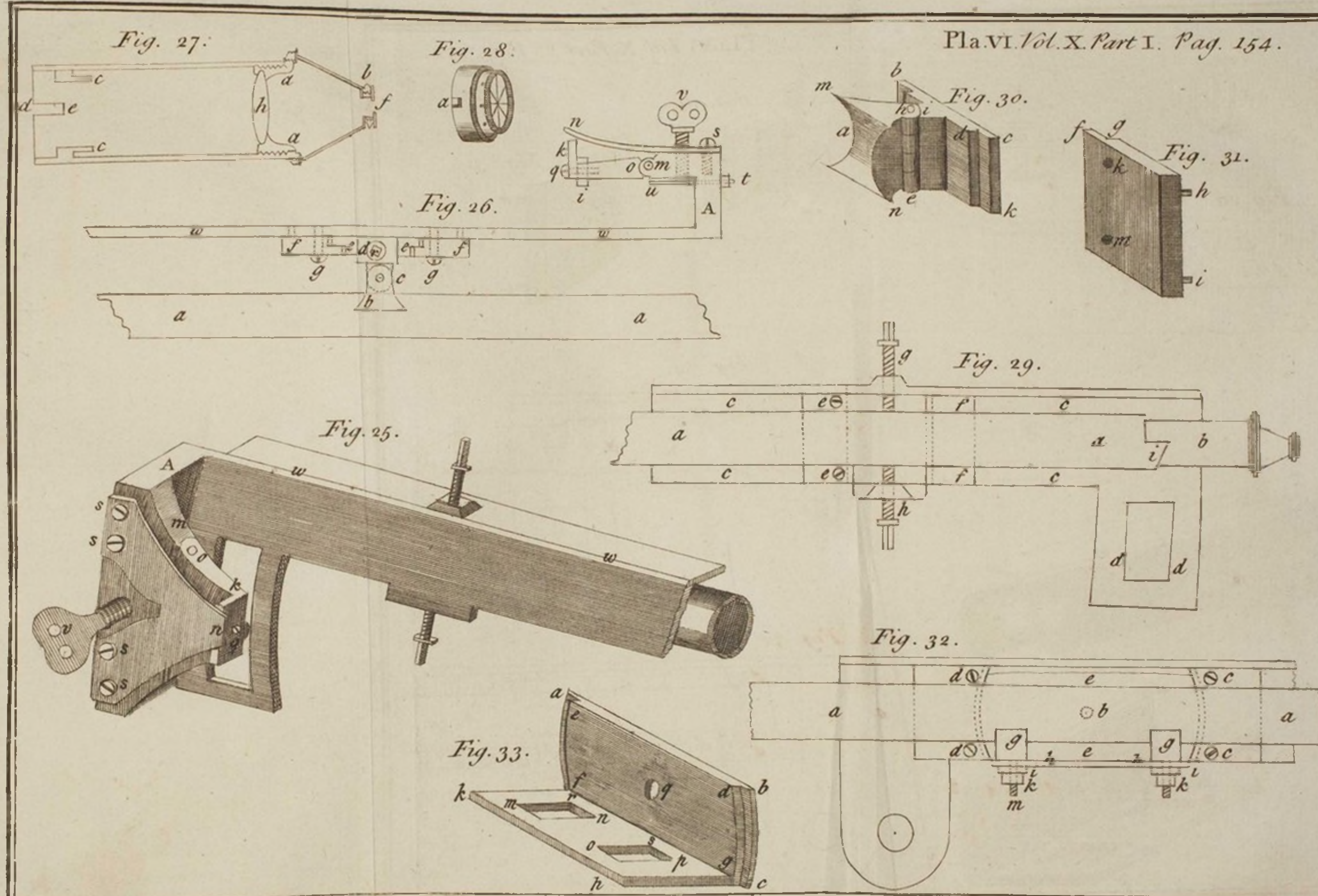
Fig. 24.


Pla. VI. Vol. X. Part I. Pag. 154.
Fig. 27.
Fig. 28.
Fig. 26.
Fig. 30.
Fig. 31.
Fig. 29.
Fig. 25.
Fig. 32.
Fig. 33.

dle *K*, which is called the declination-handle. It has also a *Nonius* index for subdividing into every 3′.

Above this declination-semicircle is fastened a reflecting telescope *LL*, of the *Gregorian* construction, the focal length of it's great speculum being 18 inches.

In order to adjust the instrument for observation, the first thing to be done, is to make the horizontal plates level or horizontal, by means of the 2 spirit-levels, and the 4 screws in the cross-pedestal. This being done, you move the meridian semicircle, by means of the meridian handle so as to raise the equatorial plates to the elevation of the equator of the place; which is equal to the complement of the latitude (and which, if not known, may likewise be found by this instrument, as shall be afterwards shewn). And thus the instrument is ready for observation.

To find the hour of the day, and Meridian of the place.

First find, from astronomical tables, the sun's declination for the day, and for that particular time of the day; then set the declination-semicircle to the declination of the sun, taking particular notice whether it is N. or S. and set the declination-semicircle accordingly.

You then turn about the horizontal handle, and the equatorial handle, both at the same time, till you find the sun precisely concentrical with the field of the telescope. If you have a clock or watch at hand, mark that instant of time; and by looking upon the equatorial plate, and *Nonius* index, you will find the hour and minute of the day, which comparing with the time shewn by the clock or watch, shews how much either of them differ from the sun. In this manner you find the hour of the day.

Now, in order to find the meridian of the place, and consequently to have a mark, by which you may always know your meridian again, you first move the equatorial plate by means of the equatorial handle, till the meridian of the plate, or hour-line of 12, is in the middle of the *Nonius* index; and then, by turning about the declination-handle till the telescope comes down to the horizon, you observe the place or point which is then in the middle of the field of the telescope; and a supposed line drawn from the center of this field to that point in the horizon, is your meridian line. The best time of the day for making this observation for finding your meridian, is about three hours before noon, or as much after noon. The meridian of the place may be found by this method so exact, that it will not differ at any time from the true meridian above 10″ of time; and if a proper allowance be made for the refraction at the time of observation, it may be found much more exact. This line thus found will be of use to save trouble afterwards; and is indeed, the foundation of all astronomical observations.

To find a star or planet in the day-time, even at noon-day.

The instrument remaining as rectified in the last experiment, you set the declination-semicircle to the declination of the star or planet you want to see; and then you set the equatorial plate to the right ascension of the star or planet at that time, and, looking thro' the telescope, you will see the star or planet; and after you have once got it into the field, you cannot lose it: for as the diurnal motion of a star is parallel to the equator by your moving the equatorial handle ſo as to follow it, you will at any time, while it is above the horizon, recover it, if it be gone out of the field.

The eaſieſt method for ſeeing a ſtar or planet in the day-time is this: your inſtrument being adjuſted as before-directed, you bring the teleſcope down ſo as to look directly at your meridian mark; and then you ſet it to the declination, and right aſcenſion, as before-mentioned.

By this inſtrument moſt of the ſtars of the firſt and ſecond magnitude have been ſeen even at mid-day, and the ſun ſhining bright; as alſo *Mercury*, *Venus*, and *Jupiter*: *Saturn* and *Mars* are not ſo eaſy to be ſeen, upon account of the faintneſs of their light, except when the ſun is but a few hours above the horizon.

And in the ſame manner in the night-time, when you can ſee a ſtar, planet, or any new phænomenon, ſuch as a comet, you may find it's declination and right aſcenſion immediately, by turning about the equatorial handle and declination-handle, till you ſee the ſtar, planet, or phænomenon; and then, looking upon the equatorial plate, you find it's right aſcenſion in time; and you find, upon the declination-ſemicircle, it's declination in degrees and minutes.

In order to have the other uſes of this inſtrument, you muſt make the equatorial plates become parallel to the horizontal plates: and then this inſtrument becomes an *equal altitude inſtrument*, a *tranſit inſtrument*, a *theodolite*, a *quadrant*, an *azimuth inſtrument*, and a *level*. The manner of applying it to theſe different purpoſes is too obvious to need any explanation.

As there is alſo a box with a magnetic needle faſtened in the lower plate of this inſtrument, by it you may adjuſt the inſtrument nearly in the meridian; and by it likewiſe you may find the variation of the needle: if you ſet the horizontal meridian, and the equatorial meridian, in the middle of their *Nonius* indexes, and direct your teleſcope to your meridian mark, you obſerve how many degrees from the meridian of the box the needle points at; and this diſtance or difference is the variation of the needle.

Improvement of the Celeſtial Globe, *by Mr* James Ferguſon. No. 483. p. 535. Mar. &c. 1747. *Read* May 14. 1747. Fig. 35.

XXVI. On the *axis* of the globe above the hour-circle, is fixed the arch *A* at one end by the ſcrew *D*, ſo as to leave ſufficient room for turning the hour-index occaſionally: the other end at *B*, being always over the pole of the ecliptic, has a pin fixed into it, whereon the collets *C* and *B* are moveable by their wires *F* and *G*, when the ſcrew *E* is ſlackned, and may be made faſt at pleaſure by this ſcrew; ſo that the turning of the globe round will carry the wires round with it, ſhewing thereby the apparent motions of the ſun and moon by the little balls on their ends at *H* and *I*. On the collet *C*, in which the ſun's wire is fix'd, there is alſo fix'd the circular plate *L*, whereon the 29½ days of the moon's age are engraven, which have their beginning juſt below the ſun's wire; conſequently the plate *L* cannot be turned without carrying the ſun's wire along with it; by

which

which means the moon's age is always counted from the ſun; and the moon's wire being turned ſo as to be under the day of her age on this plate, will ſet her at her due diſtance from the ſun for that time. Theſe wires being quadrants from *C* to *H*, and from *B* to *I*, muſt ſtill keep the ſun and moon directly over the ecliptic; becauſe the center of their motions at *C* and *B* is perpendicularly over the pole of the ecliptic; in the *arctic* circle. But, becauſe the moon does not keep her courſe in the ecliptic, as the ſun appears to do, but has a declination of 5 $\frac{1}{3}$ degrees on each ſide of it in every lunation, ſhe is made to ſcrew on her wire as far on both ſides as her declination or latitude amounts to. For this purpoſe *K* is a ſmall piece of paſteboard, to be applied over the ecliptic at right angles; the middle line *o o* ſtanding perpendicularly thereon. From this line there is 5 $\frac{1}{3}$ degrees marked on each ſide upon the outward limb; which reaching to the moon, makes her to be eaſily adjuſted to her latitude at any time.——*N. B.* The horizon ſhould be ſupported by two ſemicircular arches, inſtead of the uſual way of doing it by pillars: becauſe the arches will not ſtop the progreſs of the balls, when they go below the horizon in an oblique ſphere.

To rectify the Globe. Elevate the pole to the latitude of the place; then bring the ſun's place in the ecliptic to the brazen meridian, and ſet the hour-index to XII at noon: keeping the globe in this poſition, ſlacken the ſcrew *E*, and ſet the ſun directly over his place in the meridian; which done, ſet the moon's wire under the day of her age for that time on the plate *D*, and ſhe will ſtand over her place in the ecliptic for that time, and you will ſee in what conſtellation ſhe then is. Laſtly, faſten the wires by the ſcrew *E*, and the globe will be rectify'd.

To find the riſing and ſetting of the ſun and moon, with their amplitudes on the horizon.

The globe being rectify'd as above to the given time, turn it round in the uſual way, and you will ſee the ſun and moon riſe and ſet for that day on the ſame points of the horizon as they do in the Heavens. The times of their riſing and ſetting are ſhewn by the hour-index, which likewiſe ſhews the time of the moon's paſſing over the meridian. If you want to ſee to greater exactneſs the riſing and ſetting of the moon, find her latitude for that day by the *Ephemeris*; and as it is S. or N. ſcrew her ſo many degrees from the ecliptic, meaſuring them by the paſteboard *K*, appling it to the ecliptic as abovemention'd; and then turning the globe round, you will ſee the time of the moon's riſing and ſetting by the hour-index, and her amplitude on the horizon for that time, as it is affected by her latitude, which will ſometimes be very conſiderable.

This may be very uſeful, eſpecially in giving lectures upon the globes; becauſe a large company at ſome diſtance will eaſily ſee this ſun and moon going above and below the horizon as they riſe and ſet, and likewiſe their appulſes to different fix'd ſtars: whereas in the uſual way, when there is only the ſun's place in the ecliptic ſhewn, it is not eaſy for any one to keep his eye upon that part of the ecliptic as the globe is turned round, unleſs it be in ſome remarkable circle of longitude; and it is not very

very eaſy to know the moon's place, unleſs at her conjunction, oppoſition, or quadratures.

This ſimple *apparatus* ſhews all the varieties that can happen in the riſing and ſetting of the ſun and moon, which are very curious, eſpecially about the poles, where the ſun is preſent for one half of the year, and abſent for the other half; the moon in winter ſhining conſtantly without ſetting from the firſt to the third quarter, in the ſun's abſence; and in ſummer the full moon is never ſeen at the poles; for ſhe ſets at the firſt quarter, and riſes not till the third, ſave what may happen on account of her latitude.

All the *phænomena* of the harveſt-moon become very plain by this additional part: and in making ſome trials I find, that, to ſome places of the earth, the moon will not differ above an hour in her riſing for 15 nights together, but will differ ſometimes 23^h in her ſetting, within the compaſs of that 15 days; and for the next 15 ſhe will ſet within 1^h of the ſame time, and differ 23^h in her riſing. This is taken in round numbers, but may be conſider'd with more exactneſs by thoſe who are better acquainted with the celeſtial motions. I ſhall only add, that the places of the earth where theſe *phænomena* happen, are thoſe lying under the polar circles.

A letter from the Widow of the late Mr John Senex, *F. R. S. to* M. Folkes, *Eſq;* P. R. S. *concerning the large Globes prepared by her late Huſband, and now ſold by herſelf, at her Houſe over-againſt* St Dunſtan's Church *in* Fleet-Street. No. 493. p. 290. Oct. &c. 1749. *Read* Jan. 19. 1748-9.

XXVII. The Royal Society being lately acquainted with ſome improvements that were ſaid to have been made upon the globes at *Nuremberg*, and deſired to encourage and recommend the ſame *, I am obliged to return you my moſt grateful acknowledgments for your kind interpoſition in behalf of mine. It is ſufficiently known, that works of art, made in our own country, have, for the moſt part, a degree of exactneſs much ſuperior to thoſe of foreign countries: and I hope I may be allowed to ſay in particular, and without diſparagement to the performances of others, that my globes will be found, upon examination, as truly made, as accurate, and as well adapted for the purpoſes of Geography and Aſtronomy, as any now extant. For (not to mention that the *terreſtrial* is formed from the beſt maps that could be made or procured, and contains no material error in the ſituation of any places where obſervations have been really and truly made) the *celeſtial*, upon the niceſt examination, will be found to have this advantage above all others, that the figures of the *conſtellations* there given, were originally dilineated by a gentleman, whoſe ſkill in performances of this nature was very well known and allowed; under the direction of the Great Dr *Halley*, to whoſe kindneſs my late huſband was upon all occaſions particularly indebted. And beſides this, to each ſtar are added *Bayer*'s letters of reference; a circumſtance extremely uſeful, either for the tracing out the path of a comet, or for deſcribing any new *phænomenon* in the Heavens.

It may be further obſerved, that celeſtial globes, as they are commonly fitted up, are adjuſted only to one particular year; though indeed they

* See Vol. VIII. p. 217.

may

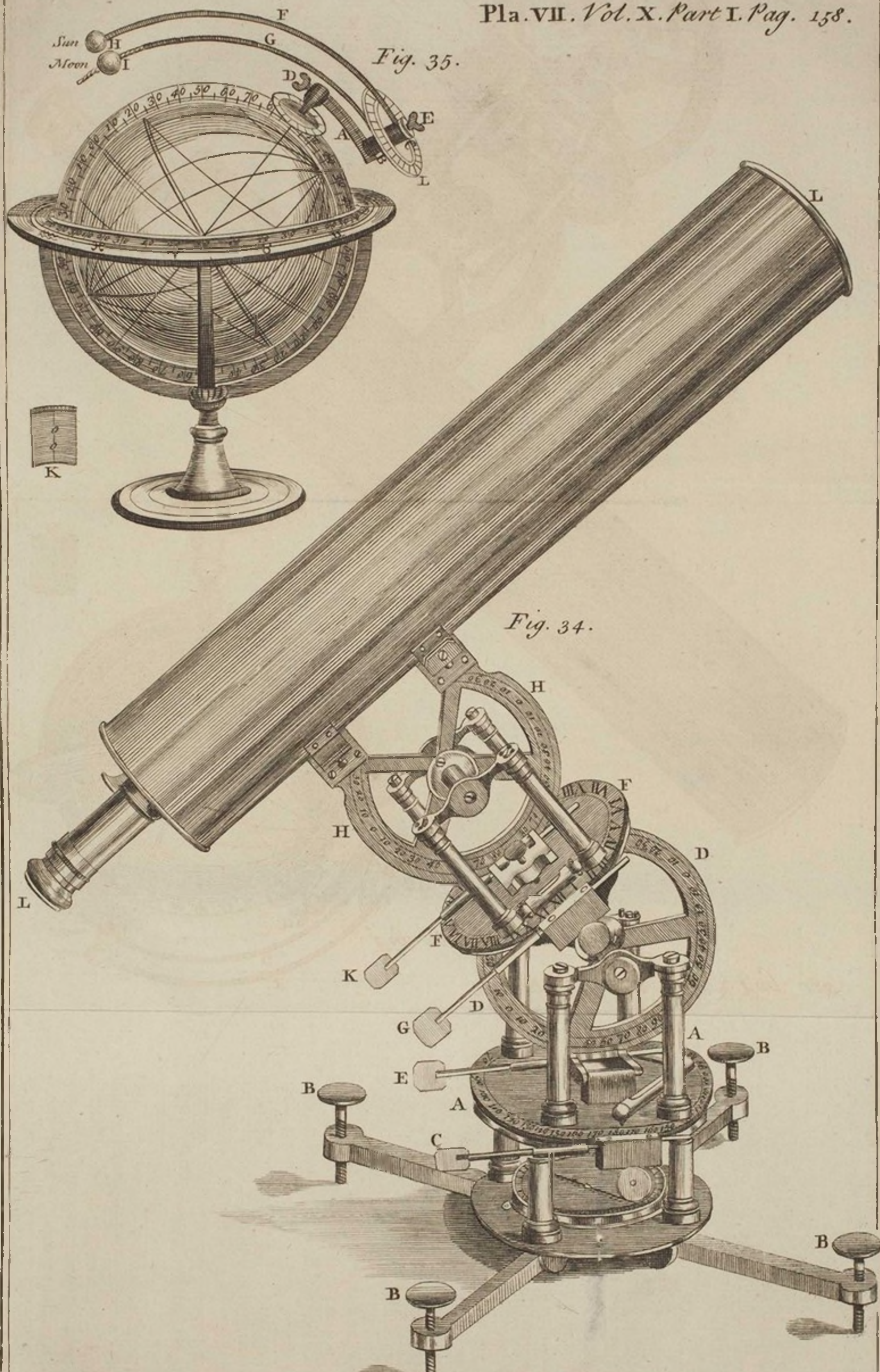


Fig. 35.

Fig. 34.

may ſerve without any ſenſible error, during the life of any ſingle perſon; whereas mine, particularly the two greateſt, *viz.* of 17 and 28 inches in diameter, have this further advantage, that they ſerve indifferently for any age paſt or to come. For by means of a nut and ſkrew, which will be hereafter deſcribed, the globe is made to turn round an iron axle; whereby the pole of the equator (though fixed in common globes is made here to revolve about the pole of the ecliptic, and repreſents the ſlow motion *forwards* obſerved among the fixed ſtars, but really owing to the ſlow motion *backwards* of the equinoctial points.

Upon this account it is, that the conſtellation of *Aries* is got into the ſign of *Taurus*, and the conſtellation of *Taurus* into that of *Gemini*, and ſo of the reſt. Hence likewiſe it is, that ſtars which roſe or ſet at particular ſeaſons of the year in the times of *Heſiod*, *Eudoxus*, *Virgil*, *Pliny*, &c. by no means anſwer at this time to their deſcriptions; but by the improvement I am here ſpeaking of, my globes (allowing for the *preceſſion of the equinox*, as it is called, *i. e.* one degree in 72 years) may, without any trouble, be adjuſted to the accounts given by any of thoſe writers.

By this means likewiſe, every one may judge of the truth of ancient obſervations without the labour of a tedious *calculus*, which ſome are not able, and others are not willing, or at leiſure, to go through. By this means likewiſe, ſome paſſages in thoſe ancient writers may be corrected, when manuſcripts afford no aſſiſtance. For theſe frequently ſuffer by the hands they go through, whilſt the Heavens remain invariably the ſame.

As by this apparent motion in the Heavens, not only the *longitudes*, *declinations*, and *right aſcenſions*, of the fixed ſtars are affected, but the poſition of the *colures* is of courſe altered; yet by the help of this contrivance all may be reſtored, and the age of an author in ſome ſort be aſcertained.

The famous aſtronomical argument likewiſe of Sir *I. Newton*, in his *Chron. p.* 86, 87, *&c.* may hereby be more particularly enquired into, and conſidered; all which uſes will be ſpeedily ſhewn and demonſtrated by a regular ſeries of propoſitions, in a treatiſe, as I am well aſſured, that is preparing for the preſs, by the Rev. and Learned Mr *George Coſtard*, Fellow of *Wadham College*, in *Oxford*.

Theſe, Sir, are ſome of the great advantages of my globes over others; and I therefore hope they will merit the encouragement of a Society founded for promoting real and uſeful learning; and that the importation of any globes from abroad may be rendered leſs neceſſary, if not entirely uſeleſs.

PAPERS omitted.

1. A Catalogue of the immerſions and emerſions of the ſatellites of *Jupiter*, that will happen in 1750, by *James Hodgſon*, F. R. S. Maſter of the Royal Mathematical School, in Chriſt's Hoſpital. N° 487. p. 373.

2. The ſame, for the year 1751. N° 493. p. 282.

CHAP.

CHAP. IV.

MECHANICKS, ACOUSTICKS.

The Action of Springs. *In a letter from* James Jurin, *M. D. F. R. S. & Coll. Med.* Lond. *to* M. Folkes, *Esq;* P. R. S. No. 472. p. 46. Jan. *&c.* 1744. *Presented* April 12. 1744.

I. THE theory of ſprings not only is of great uſe in the more curious parts of mechanicks, as the ſtructure of watches, *&c.* but may give light to many operations of nature, there being few ſubſtances but what are endued with ſome degree of elaſticity; and particularly the bodies of animals, and of vegetables likewiſe, being known to conſiſt, in a great meaſure, if not wholly, of organs ſtrongly elaſtic. For which reaſon it is not to be wondered at, that this theory has engaged the thoughts of ſeveral eminent Mathematicians of the laſt and preſent age; as Dr *Hook*, Mr *John Bernoulli*, M. *Camus*, &c. But, as all that I have yet ſeen upon this ſubject goes no further, than to compare the effects of different ſprings one with another in one caſe only, where they are ſuppoſed to be bent to the ſame degree, and that without ſhewing how the effect of any of them may be reduced to, or compared with, that of any other natural cauſe, I flatter myſelf, that the general propoſition I am going to lay down may merit your attention, both on account of its ſimplicity, and of its comprehending all poſſible caſes of a body acting upon a ſpring, or a ſpring upon a body, where no other power intervenes: and alſo of its reducing the effect to that moſt known and ſimple one, the effect of gravity upon falling bodies. In order to which to prevent any miſapprehenſion, it will be proper to fix the meaning of ſuch terms as I ſhall have occaſion to make uſe of.

1. By a ſpring, I mean a body of any ſhape perfectly elaſtic.

2. By the natural ſituation of a ſpring, I mean the ſituation it will reſt in, when not diſturbed by any external force.

3. By the length of a ſpring, I mean the greateſt length, through which it can be forced inwards. This would be the whole lengh, were the ſpring conſidered as a mathematical line; but in a material ſpring is the difference between the whole length when the ſpring is in its natural ſituation, and the length or ſpace it takes up when wholly compreſſed or cloſed.

4. By the ſtrength of a ſpring, I mean the leaſt force or weight, which, when the ſpring is wholly compreſſed or cloſed, will reſtrain it from unbending itſelf.

5. By the ſpace through which a ſpring is bent, I mean that ſpace or length through which one end of the ſpring is removed from its natural ſituation.

6. By the force of a ſpring bent or partly cloſed, I mean the leaſt force or weight, which, when the ſpring is bent through any ſpace leſs than its whole length, will confine it to the ſtate it is then in, without ſuffering it to unbend any farther.

This

This being premised, I shall next, for the foundation of what follows, lay down a principle, which was verified by experiment, in the presence of our Royal Founder about 70 years ago, by * Dr *Hook*; and has been lately confirmed by the accurate hand of Mr *George Graham*.

Ut Tensio, sic Vis: That is, if a spring be forced or bent inwards, or drawn outwards, or any way removed from it's natural situation, it's resistance is proportional to the space by which it is removed from that situation. PRINCIPLE.

Thus, if the spring *C L*, *Fig*. 36. resting with the end *L* against any immoveable support, but otherwise lying in it's nature situation, and at full liberty, shall, by any force, *p*, be pressed inwards, or from *C* towards *L*, through the space of one inch, and can be there detained by that force *p*, the resistance of the spring, and the force *p*, exactly counterbalancing one another; then the force 2 *p*, will bend the spring thro' the space of 2 inches, 3 *p* thro' 3 inches, 4 *p* thro' 4 inches, &c. the space *C l*, *Fig*. 37. thro' which the spring is bent, or by which the end *C* is removed from it's natural situation, being always proportional to the force which will bend it so far, and will detain it so bent.

And if one end *L* be fastened to an immoveable support, *Fig*. 38. and the other end *C* be drawn outwards to *l*, and be there detained from returning back by any force *p*, the space *C l*, thro' which it is so drawn outwards, will be always proportional to the force *p*, which is able to detain it in that situation.

And the same principle holds in all cases, where the spring is of any form whatsoever, and is, in any manner whatsoever, forcibly removed from it's natural situation.

Here, Sir, I presume, you will think it material to take notice, that the elastick force of the air is a power of a different nature, and governed by different laws, from that of a spring. For supposing the line *L C*, *Fig*. 36. to represent a cylindrical volume of air, which, by compression, is reduced to *L l*, *Fig*. 37. or, by dilatation, is extended to *L l*, *Fig*. 38. it's elastick force will be reciprocally as *L l*, *Fig*. 36, and 37. whereas the force or resistance of a spring will be directly as *C l*.

I now proceed to my general proposition, and it's corollaries; in which if I shall happen at any time to express myself with less accuracy, as in making weights, times, velocities, &c. to become promiscuously the subjects of geometrical or arithmetical operations, I desire, once for all, to be understood, not as speaking of those quantities themselves, but of lines, or numbers, proportional to them.

If a spring of the strength *P*, and the length *C L*, *Fig*. 39. lying at full liberty upon a horizontal plane, rest with one end *L* against an immoveable support; and a body of the weight *M*, moving with the velocity *V*, in the direction of the axis of the spring, strike directly THEOREM.

* Lectures *de Potentia restitutiva*, 1678.

upon the other end C, and thereby force the ſpring inwards, or bend it thro' any ſpace CB; and a middle proportional, CG, be taken between the line $CL \times \frac{M}{P}$, and $2a$, a being the height to which a heavy body would aſcend *in vacuo* with the velocity V; and, upon the radius $R = CG$, be erected the quadrant of a circle GFA; I ſay,

1. When the ſpring is bent thro' any right ſine of that quadrant, as CB, the velocity v of the body M, is, to the original velocity V, as the co-ſine to the radius: that is, $v = V \times \frac{BF}{R}$.

2. The time t of bending the ſpring thro' the ſame ſine CB, is to T the time of a heavy body's aſcending *in vacuo* with the velocity V, as the correſponding arch to $2a$: that is $t = T \times \frac{GF}{2a}$.

DEMONSTRATION. 1. While the ſpring is bending thro' the ſpace CB, let the ſpace, thro' which it is at any time bent, be called x, τ the time of bending it thro' the ſpace x, and v the velocity of the body at the end of the time τ; and let $CL = L$, $CB = l$.

Then, if p be the force, with which the ſpring, when bent thro' the ſpace x, reſiſts the motion of the body; by Dr *Hook*'s principle, $L : x :: P : p = \frac{Px}{L}$.

And ſince, in the caſe of forces that act uniformly, the quantities of motion generated are proportional to the generating forces, and the times jointly, if $M\dot{v}$ be the naſcent quantity of motion taken from the body by the reſiſtance $\frac{Px}{L}$ in the naſcent time $\dot{\tau}$, $MV : -M\dot{v} :: MT :: \frac{Px\dot{\tau}}{L}$ or, $-\dot{v} = \frac{VPx\dot{\tau}}{MLT}$.

Alſo, ſince, in the ſame caſe of forces acting uniformly, the ſpaces are proportional to the velocities, and the times jointly, $\dot{x} : 2a :: v\dot{\tau} : VT$, or $\dot{\tau} = \frac{TV\dot{x}}{2av}$.

Therefore, $-\dot{v} = \frac{VPx}{MLT} \times \frac{TV\dot{x}}{2av}$, or, $2v\dot{v} = -\frac{V^2Px\dot{x}}{MLa}$; and the fluents of theſe two quantities are v^2 and $-\frac{V^2Px^2}{2MLa}$. But the former of of these was V^2, when x, and consequently, the latter was nothing; therefore $v^2 - V^2 = -\frac{V^2 \cdot P x^2}{2MLa}$, or $v^2 = V^2 - \frac{V^2 P x^2}{2MLa}$.

But, by the construction, $\frac{2MLa}{P} = R^2$; therefore, $v^2 = V^2 - \frac{V^2 x^2}{R^2}$, or, $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; and, when x becomes equal to l, and v to v, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and, by the property of the circle, $R^2 - l^2$ being equal to BF^2, $v^2 = V^2 \times \frac{BF^2}{R^2}$, or $v = V \times \frac{BF}{R}$. *Q. E. D.* 1°.

2. We have above, $\dot{\tau} = \frac{TV\dot{x}}{2av}$; and $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; or, $v = V \times \frac{\sqrt{R^2 - x^2}}{R}$: therefore, $\dot{\tau} = \frac{TV\dot{x}}{2a} \times \frac{R}{V \times \sqrt{R^2 - x^2}}$, or, $\dot{\tau} = \frac{T}{2a} \times \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$.

Now let CD, *Fig.* 40. be equal to x; and draw the co-sine DE, the radius CE, and the perpendicular $ed = \dot{x}$: then will the triangle DEC be similar to the nascent triangle deE; and consequently, $DE : de :: CE : eE = \frac{CE \times de}{DE} = \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$.

Therefore, $\dot{\tau} = \frac{T}{2a} \times \dot{eE}$, and $\tau = T \times \frac{GE}{2a}$. And when a becomes equal to CB, and τ to t, the arch GE becomes equal to the arch GF: therefore $t = T \times \frac{GF}{2a}$. *Q. E. D.* 2°.

Whereas I have represented the spring as resting against an immoveable support at L, it will, perhaps, be objected, that no support can be really immoveable; since any body, how great soever, may be moved out of it's place by the least force. But this objection may easily be removed, by supposing the spring to be continued till it becomes of twice the length CL, and that a second body, equal to M, strikes against the opposite end of the spring with the same velocity in a contrary direction; in which case the point L will be perfectly immoveable. SCHOLIUM I.

Under this theorem are comprehended the 3 following cases: SCHOLIUM II.

In case 1. The spring is bent thro' it's whole length, or is intirely compressed and closed, before the moving force of the body is consumed, and it's motion ceases.

In case 2. The moving force of the body is consumed, and it's motion ceases, before the spring is bent thro' it's whole length, or wholly closed.

In case 3. The moving force of the body is consumed, and it's motion ceases, at the instant that the spring is bent thro' it's whole length, and is intirely closed.

For this reason, and in order to make the following corollaries of more ready use, I shall take the liberty of distributing them into 3 classes, the first of which are as general as the theorem itself, extending to all the 3 cases, but are more particularly useful in case 1. The second class of corollaries extend to both the second and third case; but are more particularly useful in case 2. The third class extend only to case 3. and, by that means, are much more simple than either of the former.

Class I. *General corollaries, but of more particular use in case 1; wherein the spring is wholly closed, before the motion of the body ceases.* *Coroll.* 1.

When the spring is bent thro' any right sine CB, *Fig.* 39. the loss of velocity is to the original velocity, as the versed sine to the radius, or $V - v = V \times \frac{Gg}{R}$.

For, since $v = V \times \frac{BF}{R}$, $V - v = V - V \times \frac{BF}{R} = V \times \frac{R - BF}{R} = V \times \frac{Gg}{R}$.

Coroll. 2.

When the spring is bent thro' any right sine CB, the diminution of the square of the velocity is to the square of the original velocity, as the square of that right sine to the square of the radius, or $V^2 - v^2 = V^2 \times \frac{CB}{R^2}$.

For, since $v = V \times \frac{BF}{R}$, $v^2 = V^2 \times \frac{BF^2}{R^2}$, and $V^2 - v^2 = V^2 - V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - BF^2}{R^2} = V^2 \times \frac{CB^2}{R^2}$.

Coroll. 3.

When the spring is bent thro' any space l, v the velocity of the body is equal to $V \times \sqrt{\frac{2MLa - Pl^2}{2MLa}}$, or to $V \times \sqrt{\frac{2Ma - pl}{2Ma}}$; and is proportional to $\sqrt{\frac{2MLa - Pl^2}{ML}}$, or to $\sqrt{\frac{2Ma - pl}{M}}$.

For, since $v^2 = V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - l^2}{R^2}$; if, for R^2, we substi-

tute

tute it's value $\frac{2MLa}{P}$, we have $v^2 = V^2 \times \frac{2MLa - Pl^2}{2MLa}$, or $v = V \times \surd \frac{2MLa - Pl^2}{2MLa}$: and, as by Dr *Hook*'s principle, $L : l :: P : p$, or $Pl = pL$, $v = V \times \surd \frac{2MLa - pLl}{2MLa}$, or, $v = V \times \surd \frac{2Ma - pl}{2Ma}$.

But $\frac{V}{\surd a}$, by *Galileo*'s doctrine, is a constant quantity; and therefore v is proportional to $\surd \frac{2MLa - Pl^2}{ML}$, or to $\surd \frac{2Ma - pl}{M}$.

The time t of bending the spring thro' any space l, is proportional to the arch GF divided by $\surd a$; l being the right sine of the arch, and $R = \surd \frac{2MLa}{P}$, being the radius. *Coroll.* 4.

For, by the theorem, $t = T \times \frac{GF}{2a}$; and $\frac{T}{\surd a}$ is a constant quantity.

The diminution of the product of the weight of the body into the square of the velocity, or (to use the expression of some late writers) the diminution of the *Vis viva*, that is, $MV^2 - Mv^2$, by bending a spring thro' any space l, is always equal to $\frac{C^2 Pl^2}{2LA}$, or to $\frac{C^2 pl}{2A}$; where A is the height from which a heavy body will fall *in vacuo* in a second of time, and C is the celerity gained by that fall. *Coroll.* 5.

For, by *Coroll.* 2. $V^2 - v^2 = V^2 \times \frac{CB^2}{R^2} = \frac{V^2 l^2}{R^2}$; and R^2, by the construction, being equal to $\frac{2MLa}{P}$, $V^2 - v^2 = V^2 l^2 \times \frac{P}{2MLa}$.

But, by *Galileo*'s theory, $\frac{V^2}{a} = \frac{C^2}{A}$; therefore, $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA}$ and $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$.

The diminution of the *Vis viva*, by bending a spring thro' any space l, is always proportional to $\frac{Pl^2}{L}$, or to pl: and, if either the spring be given, or $\frac{P}{L}$ be given in different springs, the loss of the *Vis viva* will be as l^2, or as p^2. *Coroll.* 6.

For,

For, by *Coroll.* 5. $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$; and $\frac{C^2}{A}$ being a constant quantity, $MV^2 - Mv^2$ is as $\frac{Pl^2}{L} = pl$: And, if $\frac{P}{L}$ be given, $MV^2 - Mv^2$ will be as l^2; or as $l^2 \times \frac{P^2}{L^2}$; or as $l^2 \times \frac{p^2}{l^2}$; or as p^2.

Coroll. 7. The loss of the *Vis viva*, by bending a spring through it's whole length, or by wholly closing it, is equal to $\frac{C^2 PL}{2A}$, and is proportional to PL: and, if PL be given, the loss of the *Vis viva* is always the same.

This is evident from *Coroll.* 5. and 6.; forasmuch as l is now equal to L.

Class II. *Corollaries of more particular use in* Case 2. *wherein the motion of the body ceases before the spring is wholly closed.*

Coroll. 8. If the motion of the body cease when the spring is bent through any space l, the initial velocity V is equal to $Cl\sqrt{\frac{P}{2MLA}}$, or to $C\sqrt{\frac{pl}{2MA}}$.

For, by *Coroll.* 5. $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$. And here, the motion of the body ceasing, $v^2 = 0$. Therefore $V^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$, or $V = Cl\sqrt{\frac{P}{2MLA}} = C\sqrt{\frac{pl}{2MA}}$.

Coroll. 10. If the motion of the body cease, when the spring is bent through any space, l, the time, t, of bending it, is equal to $1''$ of time, multiplied by $\frac{m}{2}\sqrt{\frac{ML}{2PA}}$, or to $1'' \times \frac{m}{2}\sqrt{\frac{Ml}{2pA}}$, where m is to 1, as the circumference of a circle to the diameter.

For, by the theorem, $t = T \times \frac{GF}{2a}$; and, by *Galileo*'s theory, $\frac{T}{\sqrt{a}} = \frac{1''}{\sqrt{A}}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{GF}{2\sqrt{a}}$.

Also, by the theorem, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and therefore v^2 being now equal to 0, $R^2 = l^2$, and, *Fig.* 41. l becomes the radius of the circle; and l being likewise equal to the right sine of the arch GF, that arch becomes a quadrant, and is equal to $\frac{2l \times m}{4}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{2lm}{4 \times 2\sqrt{a}}$, or $t = 1'' \times \frac{lm}{4\sqrt{A} \times \sqrt{a}}$.

Fig. 41.

But

But l being equal to $R=\sqrt{\frac{2MLa}{P}}$, $\frac{l}{\sqrt{a}}=\sqrt{\frac{2ML}{P}}$; therefore $t=1''$ $\times\frac{m}{4\sqrt{A}}\times\sqrt{\frac{2ML}{P}}$, or, $t=1''\times\frac{m}{2}\sqrt{\frac{ML}{2PA}}=1''\times\frac{m}{2}\sqrt{\frac{Ml}{2pA}}$.

In the same case, the time of bending the spring is proportional to $\sqrt{\frac{ML}{P}}$, or to $\sqrt{\frac{Ml}{p}}$; and if $\frac{L}{P}$ be given, t will be as $\sqrt{M}$; and, if both $\frac{L}{P}$, and also M, be given, t will always be the same, whatever be the original velocity, or through whatever space the spring be bent. *Coroll.* 11.

If the motion of the body cease, when the spring is bent through any space l, the product of the initial velocity, and the time of bending the spring, or Vt, is equal to $1''\times\frac{mCl}{4A}$; and is proportional to l, the space through which the spring is bent. *Coroll.* 12.

For, by *Coroll.* 8. $V=Cl\sqrt{\frac{P}{2MLA}}$, and, by *Coroll.* 9. $t=1''\times\frac{m}{2}$ $\sqrt{\frac{ML}{2PA}}$; therefore, $Vt=1''\times\frac{mCl}{4A}\sqrt{\frac{MLP}{MLP}}=1''\times\frac{mCl}{4A}$; and, as $1''$, m, C and A, are given quantities, Vt is as l.

Hence, any two of the three quantities, V, t, and l, being given, the other is readily determined.

In the same case, the initial quantity of motion, or MV, is equal to $Cl\sqrt{\frac{PM}{2LA}}$, or to $C\sqrt{\frac{plM}{2A}}$. *Coroll.* 13.

For, by *Coroll.* 8. $V=Cl\sqrt{\frac{P}{2MLA}}=C\sqrt{\frac{pl}{2MA}}$; wherefore $MV=Cl$ $\sqrt{\frac{PM}{2LA}}=C\sqrt{\frac{plM}{2A}}$.

In the same case, MV is proportional to $l\sqrt{\frac{PM}{L}}$, or to $\sqrt{plM}$, or to $\frac{Plt}{L}$, or to pt: And, if $\frac{P}{L}$ be given, MV is as $l\sqrt{M}$, or as lt. *Coroll.* 14.

For, in the preceding *Coroll.* $\frac{C}{\sqrt{A}}$ is a given quantity; and, by *Coroll.* 11. t is as $\sqrt{\frac{ML}{P}}=\sqrt{\frac{ML}{p}}$.

Coroll. 15. If the quantity of motion MV bend a spring of the strength P, and length L, through the space l, and be wholly consumed thereby, no different quantity of motion equal to the former, as $nM\times\frac{V}{n}$, will bend the same spring through the same space, and be wholly consumed thereby.

For, by the preceding *Coroll.* if the spring be bent through the space l, and each of these quantities of motion be consumed thereby; $l\sqrt{M}:l\sqrt{nM}::MV:nM\times\frac{V}{n}$. But $MV=nM\times\frac{V}{n}$; and therefore, $l\sqrt{M}=l\sqrt{nM}$, or $1=n$, and $M=nM$, and $V=\frac{V}{n}$. Therefore the quantity of motion $nM\times\frac{V}{n}$ is not only equal to MV, but is composed of an equal mass, and an equal velocity.

Coroll. 16. But a quantity of motion less than MV, in any given *ratio*, may bend the same spring through the same space l, and the time of bending it will be less in the same given *ratio*.

For, let 1 to n be the given *ratio*; and let the lesser quantity of motion be $\frac{M}{nn}\times nV$; which is to MV, as 1 to n. Then, by *Coroll.* 14. the spring being given, $l\sqrt{M}:l\sqrt{\frac{M}{nn}}::MV:\frac{M}{nn}\times nV=\frac{MV}{l\sqrt{M}}\times l\sqrt{\frac{M}{nn}}=\frac{MV}{n}$. Therefore the quantity of motion $\frac{M}{nn}\times nV$, being equal to $\frac{MV}{n}$, will bend the spring through the same space l.

Likewise, by the same *Coroll.* MV is as lt; and l being given, the quantity of motion is as t: Therefore the time of bending the spring will be less in the same *ratio*, as the quantity of motion is less.

Coroll. 17. A quantity of motion greater than MV, in any *ratio* given, may be consumed in bending the spring through the same space; and the time of bending it will be greater in the same given *ratio*.

This appears after the same manner as the preceding, by making n a fractional number instead of a whole one.

Coroll. 18. If the motion of the body cease, when the spring is bent through any space l, the initial *Vis viva*, or MV^2, is equal to $\frac{C^2Pl^2}{2LA}$, or to $\frac{C^2pl}{2A}$: and $2aM=\frac{Pl^2}{L}=pl$.

For, by *Coroll.* 8. $V=Cl\sqrt{\frac{P}{2MLA}}=C\sqrt{\frac{pl}{2MA}}$, or $V^2=\frac{C^2l^2P}{2MLA}=\frac{C^2pl}{2MA}$: therefore $MV^2=\frac{C^2Pl^2}{2LA}=\frac{C^2pl}{2A}=\frac{V^2P^2l^2}{2La}=\frac{V^2pl}{2a}$.

In

In the ſame caſe, the initial *Vis viva* is proportional to $\frac{Pl^2}{L} = pl$ and Coroll. 19.
if $\frac{P}{L}$ be given, the *Vis viva* is as l^2, or as p^2.

For, in the preceding Coroll. $\frac{C^2}{A}$ being a given quantity, the *Vis viva* is as $\frac{Pl^2}{L} = pl$; and, if $\frac{P}{L}$ be given, it will be as l^2, or as p^2; forasmuch as p and l increaſe in the ſame proportion.

If the *Vis viva*, MV^2, bend a ſpring thro' the ſpace l, and be Coroll. 20.
totally conſumed thereby, any other *Vis viva*, equal to the former, as $nnM \times \frac{V^2}{nn}$, will bend the ſame ſpring thro' the ſame ſpace, and be totally conſumed thereby.

For, the ſpring being the ſame, $\frac{P}{L}$ is given; and therefore by *Coroll.* 19. the *Vis viva*, which will be conſumed in bending the ſpring thro' the ſpace l, is as l^2.

But the time, in which the ſame ſpring will be bent thro' the Coroll. 21.
ſame ſpace, by the *Vis viva* $nnM \times \frac{V^2}{nn}$, will be to the time, in which it is ſo bent by the *Vis viva* $M \times V^2$, as n to 1; n being any whole or fractional number.

For, by *Coroll.* 11. ſince $\frac{L}{P}$ is given, the time is as VM.

If the motion of the body ceaſe, when the ſpring is bent thro' it's Class III. *Corollaries in* caſe 3. *wherein the motion of the body ceaſes, at the inſtant that the ſpring is wholly cloſed.*
whole length, or is wholly cloſed, the initial velocity V is equal to $C\sqrt{\frac{PL}{2MA}}$.

Coroll. 22.

For, by *Coroll.* 8. $V = C\sqrt{\frac{pl}{2MA}}$; and l being now equal to L *Fig.* 42. p becomes equal to P; and therefore $V = C\sqrt{\frac{PL}{2MA}}$.

In the ſame caſe, the initial velocity V is proportional to $\sqrt{\frac{PL}{M}}$. Coroll. 23.

For $\frac{C}{\sqrt{A}}$, in the preceding *Coroll.* is a given quantity.

Coroll. 24. In the same case, if PL be given, either in the same, or in different springs, the initial velocity V is reciprocally as $\sqrt{M}$.

This is plain from the preceding *Coroll.*

Coroll. 25. If the motion of the body cease, when the spring is wholly closed, the product of the initial velocity, and the time spent in closing the spring, or Vt, is equal to $1'' \times \frac{mCL}{4A}$; and is proportional to L, the length of the spring.

For, by *Coroll.* 22. $V = C\sqrt{\frac{PL}{2MA}}$; and, by *Coroll.* 10. $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}}$: therefore, $Vt = 1'' \times \frac{mCL}{4A}$; and $1''$, m and $\frac{C}{A}$, being given quantities, Vt is as L.

Coroll. 26. In the same case, the initial quantity of motion, or MV, is equal to $C\sqrt{\frac{PLM}{2A}}$.

For, by *Coroll.* 23. $V = C\sqrt{\frac{PL}{2MA}}$.

Coroll. 27. In the same case, MV is proportional to $\sqrt{PLM}$, or to Pt: and, if PL be given, either in the same, or different springs, MV is as $\sqrt{M}$.

This appears, partly, from the preceding *Coroll.* where $\frac{C}{\sqrt{A}}$ is a given quantity; and, consequently, MV is as $\sqrt{PLM}$; and PL being given, MV is as $\sqrt{M}$: and, partly, from *Coroll.* 11. where t is as $\sqrt{\frac{ML}{P}}$, and, consequently, Pt is as $\sqrt{PLM}$.

Coroll. 28. In the same case, if $\frac{P}{L}$ be given, either in the same, or in different springs, the initial quantity of motion is as the length of the spring into the time of bending it.

For, by *Coroll.* 27. MV is as Pt; and, if P be as L, MV is as Lt.

Coroll. 29. If the quantity of motion MV bend a spring thro' it's whole length, and be consumed thereby, no other quantity of motion equal to the former, as $nM \times \frac{V}{n}$, will close the same spring, and be wholly consumed thereby.

This is proved in the same manner as *Coroll.* 15. putting only L for l.

Coroll. 30. But a quantity of motion less or greater than MV, in any given *ratio*, may close the same spring, and be wholly consumed in closing it: and the

the time ſpent in cloſing the ſpring will be reſpectively leſs or greater, in the ſame given *ratio*.

This is eaſily proved from *Coroll.* 16.

If the motion of the body ceaſe, when the ſpring is wholly cloſed, the initial *Vis viva*, or MV^2, is equal to $\frac{C^2 PL}{2A}$: and $2aM = PL$. *Coroll.* 31.

For, by *Coroll.* 22. $V = C\sqrt{\frac{PL}{2MA}}$, or $V^2 = \frac{C^2 PL}{2MA}$, or $MV^2 = \frac{C^2 PL}{2A} = \frac{V^2 PL}{2a}$.

In the ſame caſe, the initial *Vis viva* is as the rectangle under the ſtrength and length of the ſpring. *Coroll.* 32.

For, by the preceding *Coroll.* $MV^2 = \frac{C^2 PL}{2A}$, and $\frac{C^2}{A}$ is a given quantity; wherefore MV^2 is as PL.

In the ſame caſe, if $\frac{P}{L}$ be given, the initial *Vis viva* is as P^2, or as L^2. *Coroll.* 33.

This is evident from the preceding *Coroll.*

If the *Vis viva* MV^2 bend a ſpring thro' it's whole length, and be conſumed in cloſing it, any other *Vis viva* equal to the former, as $nn M \times \frac{V^2}{nn}$, will cloſe the ſame ſpring, and be conſumed thereby. *Coroll.* 34.

This is evident from *Coroll.* 32.

But the time of cloſing the ſpring by the *Vis viva* $nn M \times \frac{V^2}{nn}$, will be to the time of cloſing it by the *Vis viva* MV^2, as n to 1. *Coroll.* 35.

For, by *Coroll.* 11. ſince the ſpring is given, the time is as $\sqrt{M}$.

If the *Vis viva* MV^2 be wholly conſumed in cloſing a ſpring of the ſtrength P, and length L; the *Vis viva*, $nnMV^2$, will be ſufficient to cloſe, *Coroll.* 36.

1. Either a ſpring of the ſtrength nnP, and length L.
2. Or a ſpring of the ſtrength nP, and length nL.
3. Or of the ſtrength P, and length nnL.
4. Or, if n be a whole number, the number nn of ſprings, each of the ſtrength P, and length L, one after another.

For, $MV^2 : nnMV^2 :: PL : nnPL$; and therefore, by *Coroll.* 32. the *Vis viva*, $nnMV^2$, will cloſe any ſpring that has $nnPL$ for the product of it's ſtrength and length. But $nnPL$ is compoſed either of $nnP \times L$, or of $nP \times nL$, or of $P \times nnL$.

Alſo the loſs of the *Vis viva*, in bending a given ſpring, being always the ſame, by *Coroll.* 7. and the *Vis viva*, MV^2 being wholly loſt in in bending a single spring PL; the loss of the *Vis viva*, $nnMV^2$, in closing one such spring, will be MV^2; and it's loss in closing a second such spring, will again be MV^2, and so on: consequently, the number nn of such springs will be closed one after another, by that time the *Vis viva*, $nnMV^2$, is wholly consumed.

SCHOLIUM III. If the spring, instead of being at first wholly unbent, as we have hitherto considered it, be now supposed to have been already bent through some space CB, before the body strikes it; and the velocity of the body be required, after the spring is bent through any further space, BD, *Fig.* 43. this case, as well as the three other above-mention'd, will be found to come under our theorem.

For, if v be the velocity with which the body is supposed to strike against the bent spring at B, it is evident, that this may be considered, either as the original velocity, or as the remainder of a greater velocity V, with which the body might have struck upon the spring at C, and which, upon bending the spring from C to B, would now be reduced to v. For, in either case, the effect in bending the spring from B to D, will be exactly the same.

In order, therefore, to determine this imaginary velocity V, let a middle proportional, BF, be taken between $CL \times \frac{M}{P}$, and 2α, α being the height to which a body will ascend *in vacuo* with the velocity v; draw BF prependicular to CB, and, with the radius CF, describe the quadrant $CGFEA$. Then will our present case be exactly reduced to that of the theorem; CB, CD, representing the spaces through which the spring is bent; BF and DE the velocities in the points B and D; GF and GE the times of bending the spring through the spaces CB, CD; and CG representing the imaginary velocity V, with which the body might have struck the spring at C.

For, by the theorem, $BF^2 : CG^2 :: v^2 : V^2$; and $v^2 : V^2 :: \alpha : a$. Therefore $CG^2 = BF^2 \times \frac{a}{\alpha}$. But $BF^2 = 2\alpha \times \frac{LM}{P}$, by the construction; and, consequently, $CG^2 = \frac{2\alpha LM}{P} \times \frac{a}{\alpha} = \frac{2aLM}{P}$, as in the construction of the theorem.

From this case we shall draw a few corollaries, as well for their usefulness upon other occasions, as to shew how the theory of springs may be safely applied to the action of gravity upon ascending or falling bodies.

Coroll. 37. If the body M, with the velocity v, sufficient to carry it to the height α, strike at B, upon a spring already bent through the space $CB = l$; and do thereby bend it through some farther space $BD = s$; at the end of which space, or at D, the body has a velocity sufficient to carry it to some height, as ϵ; then $\epsilon = \frac{2\alpha ML - Ps \times \overline{2l + s}}{2ML}$.

For,

For, by the theorem, $\alpha : \varepsilon :: BF^2 : DE^2$, or $DE^2 = BF^2 \times \frac{\varepsilon}{\alpha} = \frac{2\alpha ML}{P} \times \frac{\varepsilon}{\alpha}$ or $DE^2 = \frac{2\varepsilon ML}{P}$.

Alſo, $DE^2 + CD^2 = CE^2 = CF^2 = BF^2 + CB^2$, that is, $\frac{2\varepsilon ML}{P} + l^2 + 2ls + s^2 = \frac{2\alpha ML}{P} + l^2$; or $\frac{2\varepsilon ML}{P} = \frac{2\alpha ML}{P} - 2ls - s^2$; or $2\varepsilon ML = 2\alpha ML - Ps \times \overline{2l+s}$.

If the motion of the body ceaſe upon bending the the ſpring through the ſpace $BD = s$, that is, if $\varepsilon = 0$; then the height to which the body might aſcend *in vacuo*, with the velocity v, or $\alpha = \frac{Ps \times \overline{2l+s}}{2ML}$. Coroll. 38.

For, by the laſt, when $\varepsilon = 0$, $2\alpha ML = Ps \times \overline{2l+s}$.

If p, the force of the ſpring when bent through the ſpace CB, be equal to M, the weight of the body; the height to which the body would aſcend *in vacuo* with the velocity v, is to the ſpace through which it will bend the ſpring, by ſtriking upon it at B with that ſame velocity, as $2l + s$ to $2l$, or $\alpha : s :: 2l + s : 2l$. Coroll. 39.

For, by the laſt, $\alpha = \frac{Ps \times \overline{2l+s}}{2ML}$; and $\frac{P}{L}$ being equal to $\frac{p}{l}$, $\alpha = \frac{ps \times \overline{2l+s}}{2Ml}$; and, if $p = M$, $\alpha = s \times \frac{\overline{2l+s}}{2l}$.

If $p = M$, and p do alſo continue conſtantly the ſame while the ſpring is bending from B to D (both which ſuppoſitions are neceſſarily made in reducing the action of a ſpring to that of gravity upon an aſcending body), the ſpring muſt be of an infinite length; and l, the ſpace through which it was bent before the body ſtruck it, muſt alſo be of an infinite length; and the ſpace BD, through which the ſpring will be further bent, muſt be equal to the height the body can aſcend to with the velocity v, or $\alpha = s$. Coroll. 40.

For, by the laſt, when $p = M$, $\alpha : s :: 2l + s : 2l$; and the reſiſtances of the ſpring at D and B being reſpectively as CD and CB, that is, as $l + s$ and l; ſince thoſe reſiſtances are now ſuppoſed equal to one another, we muſt, upon that ſuppoſition, conſider $l + s$ as equal to l; and adding l to each, $2l + s = 2l$, that is, l muſt be infinitely greater than s; and then $\alpha : s :: 2l : 2l$, or $\alpha = s$.

In this propoſition, and all it's corollaries, except the 4 laſt, we have conſidered the ſpring as being, at firſt, wholly unbent, and then acted upon by a body moving with the velocity V, which bends it through ſome certain ſpace: But, as we ſuppoſe the ſpring to be perfectly elaſtic, the propoſition and corollaries will equally hold, if the ſpring be ſuppoſed Scholium IV.

so

to have been, at firſt, bent through that ſame ſpace, and, by unbending itſelf, to preſs upon a body at reſt, and thereby to drive that Body before it, during the time of it's expanſion: Only, V, inſtead of being the initial velocity, with which the body ſtruck the ſpring, will now be the final velocity, with which the body parts from the ſpring when wholly expanded.

Scholium V. If the ſpring, inſtead of being preſſed inwards, be drawn outwards by the action of the body, we need only make L the greateſt length to which the ſpring can be drawn out beyond it's natural ſituation, without prejudice to it's elaſticity, l any leſſer length to which the ſpring is drawn outwards, P and p the forces, which will keep it from reſtoring itſelf when drawn out to thoſe lengths reſpectively, and the propoſition will equally hold good: as it will alſo, if the ſpring be ſuppoſed to have been already drawn outwards to the length l, and, in reſtoring itſelf, to draw the body after it: only, in this latter caſe, V, the initial velocity in the propoſition, will now be the final velocity, as in *Scholium* IV.

Scholium VI. Our propoſition equally holds good, when the ſpring is of any form whatſoever, provided L be always underſtood to be the greateſt length it can be bent or drawn to from it's natural ſituation, l any leſſer length, and P, p, the forces which will confine it to theſe lengths. For Dr *Hook*'s principle extends to ſprings of any form.

I have been at the trouble of drawing ſo great a number of corollaries from this propoſition, becauſe, in the controverſy about the force of bodies in motion, I have obſerved both parties to ſupport their opinion by arguments taken from the theory of ſprings; and I was willing impartially to furniſh them both with means to examine into the truth or falſehood of one another's reaſonings. I had thoughts myſelf of making uſe of ſome of theſe corollaries for that purpoſe, being far from thinking that the diſpute is about words only; but this letter is already drawn out to too great a length; and before I have leiſure to write again, I may poſſibly be prevented by a better hand, which, I hope, may put an end to a diſpute thas has too long peſter'd the Learned World.

An inquiry into the Meaſure *of the Force of Bodies in* Motion: *with a propoſal of an* Experimentum Crucis, *to decide the Controverſy about it; by the ſame.* No. 476. p. 423. April, &c. 1741. *Read* May 30. 1745.

II. Mechanical forces may be reduced to two ſorts; one of a body at reſt, the other of a body in motion. The force of a body at reſt, is that of a body lying ſtill upon a table, or hanging by a rope, or ſupported upon a ſpring, *&c.* This is called by the name of preſſure, tenſion, force, or *vis mortua*.

The *meaſure* of this force is the weight with which the table is preſſed, or the rope is ſtretched, or the ſpring is bent. And that *meaſure* being acknowledged by all writers, there is no diſpute about this ſort of force, notwithſtanding the diverſity of appellations by which it is called.

The force of a body in motion is on all hands agreed to be a power reſiding in that body, ſo long as it continues it's motion; by means of which it is able to remove obſtacles lying in it's way; to leſſen, deſtroy, or overcome, the force of any other moving body, which meets it in an oppoſite

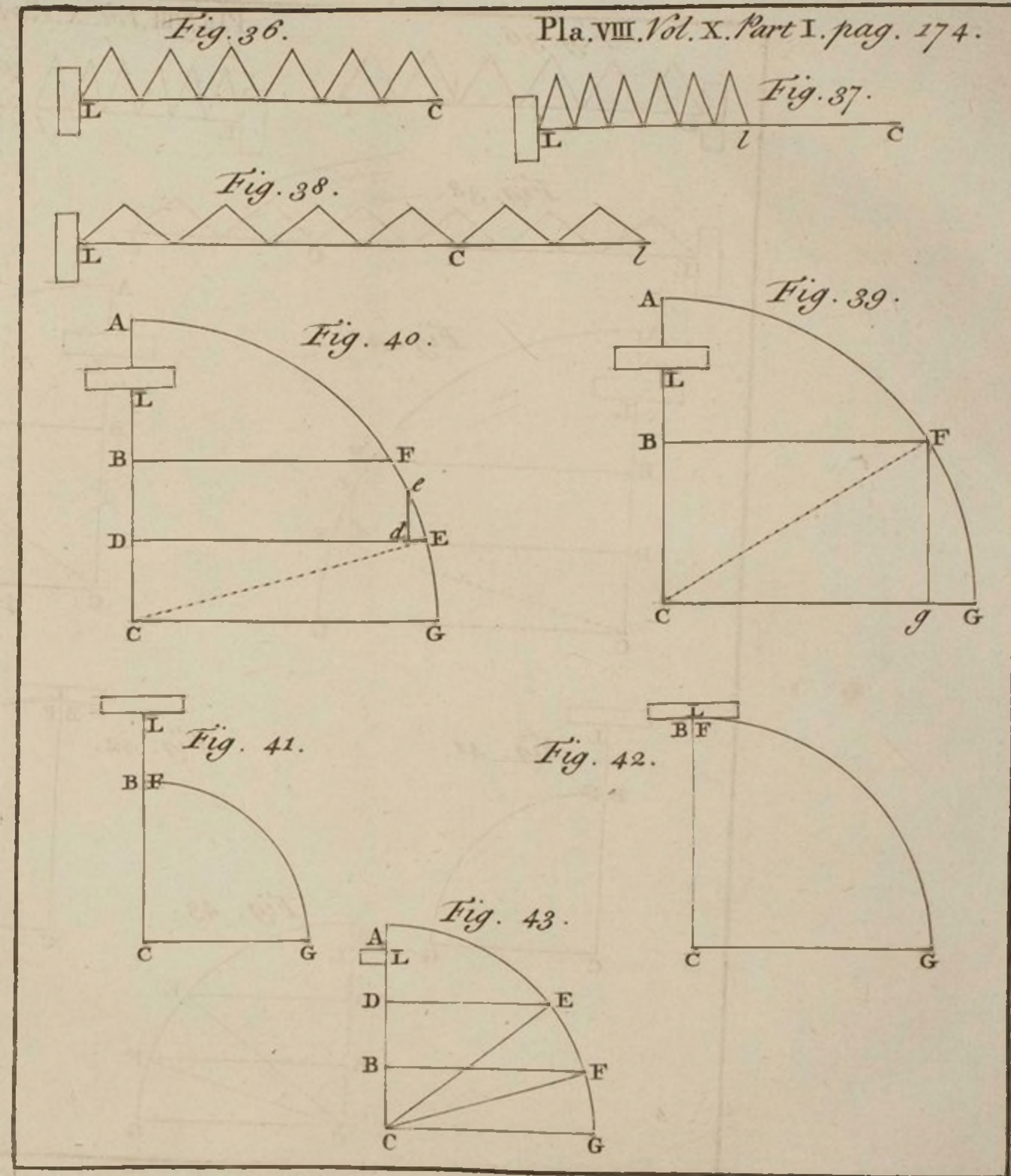


Fig. 36.
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oppoſite direction; or to ſurmount any dead preſſure or reſiſtance, as tenſion, gravity, friction, *&c.* for ſome time; but which will be leſſened or deſtroyed by ſuch obſtacles, or by ſuch reſiſtance, as leſſens or deſtroys the motion of the body. This is called moving force, *vis motrix*, and by ſome late writers, *vis viva*, to diſtinguiſh it from the *vis mortua* ſpoken of before: and by theſe appellations, however different, the ſame thing is underſtood by all Mathematicians; namely, That power of diſplacing obſtacles, withſtanding oppoſite moving forces, or overcoming any dead reſiſtance, which reſides in a moving body, and which, in whole or in part, continues to accompany it, ſo long as the body moves.

But about the *meaſure* of this ſort of force, mathematicians are divided into two parties: And, in order to ſtate the caſe fairly between them, it will be neceſſary to ſhew how far the two parties agree, and in what point their diſagreement conſiſts.

Both ſides agree, That the *meaſure* of this force depends partly upon the maſs, or weight, of the body, and partly upon the velocity with which it moves; ſo that, upon any increaſe either of the weight, or of the velocity, the moving force will become greater. They alſo agree, That if the velocity continue the ſame, but the maſs, or weight of the body, be increaſed in any proportion, the moving force is increaſed in the ſame proportion: ſo that, in this caſe, the *meaſure* of the moving force is the ſame with that of the weight: or, when two bodies move with the ſame velocity, if the weight of the ſecond be double, triple, quadruple, of that of the firſt, the moving force of the ſecond will alſo be double, triple, quadruple, of that of the firſt. But, when two bodies are equal, and the velocities with which they move are different, the two parties no longer agree about the *meaſure* of the moving force.

One ſide maintains, That, when the velocity of the ſecond body is double, triple, quadruple of that of the firſt, the *meaſure* of the moving force of the ſecond is alſo double, triple, quadruple, of that of the moving force, being the ſame with that of the velocity.

The other ſide pretend, That, in the ſame caſe, the moving force of the ſecond body is four times, nine times, ſixteen times, as great as that of the firſt; the *meaſure* of the moving force being the ſame with that of the ſquare of the velocity.

In conſequence of the agreement in the firſt of theſe two caſes, and the diſagreement in the ſecond, the one ſide pretends, That the *meaſure* of the moving force is, in all caſes, the product of the weight into the velocity; and the other, That it is the product of the weight into the ſquare of the velocity.

This controverſy was firſt ſtarted by the famous Mr *Leibnitz*, and has been carried on by him and his followers for near 60 years; during which time a great number of pieces have been publiſhed on both ſides of the queſtion, and a great number of experiments have been made, or propoſed to be made, in order to decide it. But though both parties agree in the event of the experiments, whether actually made, or only propoſed; yet as as the writers on each ſide have found a way of deducing from thoſe experiments a concluſion ſuitable to their own opinion, the diſagreement ſtill continues as wide as ever, to the great ſcandal of the learned world.

Now, if we examine carefully into the reaſon of this, and would ſee by what means it happens, that two oppoſite concluſions are ſo often drawn from the ſame experiment, we ſhall find it not ſo much owing to falſe reaſoning on either ſide, (that would be eaſily detected, and ſet right), as to another cauſe; namely, to their diſagreement in the principles upon which the reaſoning is founded.

For, whereas whatever is laid down on either ſide as a principle, ought to be ſomething all the world agrees in, at leaſt what is admitted by the other party; without which, all reaſoning upon it is to no purpoſe; this conduct has been ſo little obſerved in the preſent diſpute, that what has been offered on the one ſide as an undoubted principle or axiom, has commonly been ſomething that the oppoſite party does not admit, nay, even abſolutely denies.

Of this it were eaſy to produce a number of examples; but I ſhall content myſelf with two only, one taken from each ſide.

Thoſe who maintain, That the moving force is as the weight into the velocity, lay down for a principle, or axiom, That when two bodies meet one another in contrary directions, if their moving forces be equal, neither body will prevail over the other: and if their moving forces be unequal, the ſtronger will always prevail over the weaker.

This the *Leibnitian* party deny. They maintain, That one of theſe bodies may prevail over the other, though their moving forces be equal: nay, that, in many caſes, the weaker will prevail over the ſtronger.

It is therefore to no purpoſe to alledge, That the principle above laid down is founded on common ſenſe; or that it was always univerſally received, till this diſpute began: for, ſince the oppoſite party now reject it, all reaſoning upon it can have no weight with them; you muſt have recourſe to ſomething elſe.

On the other hand, thoſe who adhere to Mr *Leibnitz*'s ſentiment, lay down for a principle, That equal effects always ariſe from equal cauſes; provided the cauſes be intirely conſumed in producing thoſe effects.

This their opponents do not admit, unleſs in the caſe where thoſe equal effects are produced in equal times; and therefore, till both ſides ſhall agree in admitting this principle, no argument can be drawn from it by one party, that will be of any ſervice to convince the other.

But as this principle is chiefly made uſe of in reaſoning upon experiments made with ſprings, many of which have been produced by both parties, in ſupport of their opinions, it may be worth while more particularly to conſider, What right there is on the one ſide to impoſe this principle, and what reaſons may be given on the other for rejecting it.

When one end of a ſpring, wholly unbent, leans againſt an immoveable ſupport, and the oppoſite end is ſtruck upon by a body in motion, which, bending the ſpring to ſome certain degree, does thereby loſe it's whole

whole moving force; the moving force of the body may be conſidered as the cauſe of bending the ſpring; and the bending of the ſpring may be looked upon as the effect of that cauſe, which is wholly ſpent and conſumed in producing it.

Now if two unequal bodies, moving with unequal velocities, ſtrike in this manner upon two equal ſprings, and each of them bend the ſpring it ſtrikes upon, exactly to the ſame degree; and by ſo doing, the moving force of each body be intirely conſumed; here, ſay the *Leibnitzian* writers, are two equal effects produced; for the ſprings are equal, and are now equally bent; and the moving forces, which are the cauſes of thoſe effects, are wholly conſumed in producing them; and therefore, by virtue of the principle above laid down, thoſe cauſes muſt be equal; that is, the moving forces of the two bodies muſt be equal.

But their antagoniſts will reply, that this principle is not admitted by them, except the times of producing thoſe effects are equal; and that they are not ſo in the caſe before us: for the greater body will take up a longer time in producing it's effect, or in bending it's ſpring.

If therefore the *Leibnitzian* party pretend, that equal effects, when produced in unequal times, do always ariſe from equal cauſes, they muſt not impoſe this upon their opponents by way of principle or axiom, but muſt demonſtrate it. Till this be done, there will be room to doubt, at leaſt, whether the two bodies have equal moving forces, though they bend equal ſprings to the ſame degree.

For the larger and ſlower of theſe two bodies will bend the one ſpring more ſlowly; and, conſequently, will be reſiſted for a longer time, than the ſmaller and ſwifter body will be reſiſted in bending the other ſpring to the ſame degree. May not therefore the total reſiſtance of a ſpring be greater, if that reſiſtance continues for a longer time? and if the total reſiſtance be greater, muſt not the moving force, which is deſtroyed and conſumed by that reſiſtance, be alſo greater? is there not reaſon then to doubt, whether the moving forces of theſe two bodies be equal, though they bend equal ſprings to the ſame degree?

In like manner, when a ſpring, already bent to ſome certain degree does, by unbending, drive before it a body which gives way to it's preſſure, is there not room to doubt, whether the preſſure of the ſpring may not produce a greater effect, when that preſſure continues for a longer time?

That preſſure may be ſaid to produce three effects, all of which may if we pleaſe, be conſidered as different from one another.

1. The preſſure carries the body thro' a certain ſpace; by which ſpace the length of the bent ſpring is increaſed, in returning to it's natural ſituation.

2. The preſſure gives to the body a certain quantity of motion.

3. It gives the body a certain moving force.

Now, the firſt of theſe effects is greater, when the preſſure acts for a longer time. For, if the preſſure of a bent ſpring, by acting for one ſecond upon the body 1, carry that body 1 thro' the ſpace 1; the preſſure of the ſame, or of an equal ſpring equally bent, by acting for two ſeconds upon the body 4, will carry that body 4 thro' the ſame ſpace 1.

Likewiſe the ſecond effect is greater, when the preſſure continues for a longer time.

For, in the caſe juſt now mentioned, the body 4 will have twice the quantity of motion that the body 1 has; though theſe two quantities of motion ariſe from the preſſure of the ſame, or, which is all one, of equal ſprings equally bent. Why therefore are we to take it for granted, or to have it impoſed upon us by way of principle or axiom, that the third effect is not greater, when the time, in which it is produced by the preſſure of the ſame, or equal ſprings, is longer, nay, infinitely longer?

But we are told, that all the force, which reſided in the ſpring, while bent, is now, upon the unbending of the ſpring communicated to the body moved. I aſk therefore, what was that force, or what kind of force was that, which reſided in the ſpring, while bent, and without motion? was it a bare preſſure, or a moving force? A *Vis mortua*, or a *Vis viva?* you muſt acknowledge, it was a *Vis mortua*, a bare preſſure, and nothing more. But the force communicated to the body, and which now reſides in the body in motion, is a *Vis viva*, a moving force. This therefore is not the ſame force, nor a force of the ſame kind, as that which reſided in the bent ſpring.

It will be ſaid, however, that the force of the bent ſpring is intirely exhauſted in giving the body it's moving force. I aſk therefore again, what is it I am to underſtand by theſe words, the force of the ſpring is intirely exhauſted? If the meaning be, that the ſpring could not poſſibly give that ſame body any greater moving force, than what it has already given, I allow it: but this does not prove, that the ſame ſpring, bent afreſh to the ſame degree, or an equal ſpring equally bent, cannot give a greater force to a greater body.

But if the meaning of theſe words be, that the ſpring cannot give a greater moving force to any body whatſoever, I muſt anſwer, that this is taking for granted the very point which is in diſpute. For the oppoſite party pretend, that a body of four times the bulk, will receive twice the moving force in twice the time, from the preſſure of the ſame ſpring in unbending itſelf, or, if you pleaſe, in exhauſting all it's force.

It is plain, therefore, that the followers of Mr *Leibnitz* have no right to ſay, a body has ſuch or ſuch a force, becauſe ſuch or ſuch a ſpring has put it in motion by unbending itſelf, or can be bent by it. This is not a poſition to be taken for granted, but ſtands in need of a demonſtration, which nobody has as yet attempted to give, at leaſt from any uncontroverted principle; and, till this be done, the laying down any ſuch poſition can have no other effect, than to perplex the controverſy more and more, without hopes of ever coming to an end of it.

For which reaſon I propoſe to take a quite different method in what follows, and to lay down nothing, by way of principle or axiom, but

what

what is allowed of by all the world, or, at leaſt, has never yet been contradicted *a priori.*

When a bent ſpring does, by unbending itſelf, puſh a body before it, the greater the body is, the more ſlowly will the ſpring unbend itſelf. *Axiom* I.

The more any ſpring is bent, the greater is it's preſſure. *Axiom* II.

A greater preſſure produces a greater moving force, if the time be given. *Axiom* III.

Moving forces are not proportional to the maſſes of the bodies, and the ſquares of their velocities. *Propoſition* I.

Let there be two ſprings, equal, and equally bent, *A* and *B*, which, by unbending themſelves, puſh before them two unequal bodies; the ſpring *A* puſhing before it the greater body. Now, by *Axiom* I. the ſpring *A* will unbend more ſlowly than the other: from which it follows, that, at every inſtant of the time which the ſpring *B* takes up in unbending itſelf, the ſpring *A* will have unbent itſelf leſs than *B*, or will be more bent than *B*. *Demonſtration*

Therefore, by *Axiom* II. the preſſure of the ſpring *A* will, at any inſtant of that time, be greater than the preſſure of the ſpring *B* at that ſame inſtant. Hence, by *Axiom* III. the naſcent, or infinitely ſmall moving force, which is produced by the preſſure of the ſpring *A* in every infinitely ſmall part of that time, will be greater than that produced by the preſſure of the ſpring *B* in the ſame infinitely ſmall part of the time. Therefore, the ſum of the infinitely ſmall moving forces; that is to ſay, the whole moving force, which is produced by the ſpring *A*, during that time, will be greater than the moving force produced by the ſpring *B* in that ſame time: or the moving force of the greater body will be greater than that of the leſſer, at the inſtant that the ſpring *B*, being now wholly unbent, ceaſes to act any longer upon the body it has puſhed before it: and as, after that inſtant the ſpring *A*, not being yet wholly unbent, continues to act upon the greater body, the moving force of the greater body will ſtill continue to increaſe, and conſequently will more and more exceed the moving force of the ſmaller body.

But every one knows, that the products of the maſſes and ſquares of the velocities are equal in the two bodies. Therefore the moving forces which we have proved to be unequal, are not proportional to the products of the maſſes and ſquares of the velocities. Which was to be demonſtrated.

To conſider this in a particular example, let us ſuppoſe the maſſes of the two bodies expoſed to the preſſure of the ſprings *A* and *B*, to be 4 and 1 reſpectively; and let the ſpring *B* unbend itſelf, and thereby give the body 1 it's whole moving force in one ſecond of time. Then, at the end of that ſecond, the moving force of the body 4 will already exceed that of the body 1, and will ſtill grow greater during another ſecond of time. For the times are as the ſquare roots of the maſſes.

Alſo if the maſſes be 100 and 1, the moving force of the body 100, will, at the end of the firſt ſecond of time, be greater than that of the

 body

body 1, and will continue to increaſe during the ſpace of nine other ſeconds.

Corollary. When a bent ſpring does, by unbending itſelf, drive a body before it, the larger that body is, the greater will be the moving force which it receives from the ſpring.

Having now clearly proved, that the moving forces are not proportional to the ſquares of the velocities, I proceed next to demonſtrate, that they are proportional to the velocities themſelves: and, in order thereto, I ſhall, as I have hitherto done, make uſe of no other principles or axioms than ſuch as are admitted on both ſides, or, at leaſt, have never yet been controverted *a priori* by either party.

Axiom IV. Springs of unequal lengths, when bent alike, have equal preſſures.

We ſpeak here of ſprings equal in all reſpects, except the length only; and, by being bent alike, we mean, that they are ſo compreſſed, as that the lengths they are now reduced to, are exactly proportional to their natural lengths, or to the lengths they are of when no way compreſſed. In this condition, if one be directly oppoſed to the other, they will mutually ſuſtain each other's preſſure, ſo as to maintain a perfect *æquilibrium:* or, if each be placed ſeparately in a vertical ſituation, they will ſuſtain equal weights. And in one or the other of theſe caſes, it is evident, that they muſt exerciſe equal preſſures.

Axiom V. Equal preſſures in equal times produce equal moving forces.

Propoſition II. Moving forces are proportional to the maſſes and velocities jointly.

Demonſtration. Let there be two ſprings, of the lengths 1 and 2, but equal in all other reſpects, and bent alike: and, in unbending themſelves, let the ſpring 1 drive before it a body whoſe maſs is 2; and the ſpring 2 another body of the maſs 1. Now, by *Coroll.* 11. of my general theorem concerning the action of ſprings, theſe two ſprings will unbend themſelves exactly in the ſame time; and, conſequently, the ſpring 2 will unbend itſelf with a velocity double of that of the ſpring 1: and by *Coroll.* 12. of the ſame thorem, it will give to the body 1 a velocity double of that, which the body 2 will receive from the ſpring 1. Alſo, as the two ſprings were ſuppoſed to be bent alike at firſt, and the ſpring 2 unbends itſelf with a velocity double to that of the ſpring 1, it is manifeſt, that, during the whole time of their expanſion, they will be always bent alike, one to the other. Therefore, by *Axiom* IV. their preſſures will be conſtantly equal one to the other: and hence, by *Axiom* V. the infinitely ſmall moving forces produced by each of theſe ſprings, in every infinitely ſmall part of time, will be equal one to the other. Conſequently, the ſums of thoſe infinitely ſmall moving forces, that is, the whole moving forces, produced by the two ſprings, will be equal one to the other. And the maſſes of the two bodies being 2 and 1, and their velocities being 1 and 2 reſpectively, it is plain, that the moving forces are proportional to the maſſes and velocities jointly. Which was to be demonſtrated.

For

For the greater facility of examining this demonſtration, we have ſuited it to a ſingle caſe only, and that the moſt ſimple that can be ſuppoſed: but every body will ſee, how eaſy it is to form a general one upon the ſame principles.

As we do not think, that any flaw can be found in either of the demonſtrations above laid down; and the axioms, upon which they are founded, have never yet been diſputed, as far as we know; we preſume, that the *Leibnitzian* opinion about the meaſure of moving forces, is inconteſtably overthrown by the firſt propoſition, and the oppoſite ſentiment is as evidently eſtabliſhed by the ſecond.

But, if any reader ſhall be of a different opinion, we muſt beg leave to propoſe to his conſideration the following experiment, which we hope may juſtly deſerve the name of an *experimentum crucis*; and, as ſuch, may put a final period to this controverſy.

It is not new indeed, having been propoſed before by myſelf and others; but, as the manner, in which it was formerly offered, has given occaſion to ſome objections, which, tho' not affecting the ſubſtance of the argument drawn from it, may yet have amuſed and embarraſſed the leſs attentive readers, I ſhall now propoſe it in ſuch a manner, as may obviate all thoſe difficulties, and, I think, will render it abſolutely deciſive. To me, I am ſure, it will be ſo; ſince I ſhall immediately embrace the *Leibnitzian* doctrine, if my argument drawn from it can receive a clear and ſatisfactory anſwer.

Upon an horizontal plane, at reſt, but moveable with the leaſt force, *Experiment.* ſuppoſe upon a boat in a ſtagnant water, let there be placed, between two equal bodies, a bent ſpring, by the unbending of which the two bodies may be puſhed contrary ways.

In this caſe it is evident, that the velocities, which the two bodies receive from the ſpring, will be exactly equal, and their moving forces will alſo be exactly equal; and that the plane they move upon, and alſo the boat upon which it lies, will have no motion given them either way. Let us call the velocity of each body 1, and the moving force alſo 1.

Now, let us ſuppoſe the ſpring to be bent afreſh to the ſame degree as before, and to be again placed between the two bodies lying at reſt; then let the plane, upon which the ſpring and the bodies lie, be carried uniformly forwards, in the direction of the length of the ſpring, with this ſame velocity 1. In this caſe it is manifeſt, that each of the bodies will have the velocity 1, and the moving force 1, both in the direction of the *axis* of the ſpring.

During this motion, let the ſpring again unbend, and puſh the two bodies contrary ways, as before, the one forwards, the other backwards: then the ſpring will give to each of theſe bodies the velocity 1, as before, when the plane was at reſt.

By this means the hindmoſt body, or that which is puſhed backwards, will have it's velocity 1, which it had before by the motion of the plane, now intirely deſtroy'd, and will be abſolutely at reſt.

But

But the body, which is pushed forwards, will now have the velocity 2, namely 1 from the motion of the plane, and 1 from the action of the spring.

Thus far every body agrees in what will be the event of this experiment.

But the question is, what will be the moving force of the foremost body, or of that which is pushed forwards, and which has the velocity 2; *viz.* 1 from the motion of the plane, and 1 from the action of the spring.

By the *Leibnitzian* doctrine, it's moving force must be 4: and, if so, it must have received the moving force 3 from the action of the spring; for it had only the moving force 1 from the motion of the plane.

Let us examine, whether this be possible, or reconcileable to their own doctrine.

Their doctrine is, that equal springs, equally bent, will, by unbending themselves, give equal moving forces to the bodies they act upon, whatever those bodies are.

We agree to this, not generally indeed; but in the case before us, where the bodies are of equal masses or weights, we agree to it.

Let us therefore imagine the bent spring, which is placed between the two bodies, to be divided transversly into two equal parts. In this case it is plain, that the two halves of the spring, may be considered as two intire springs, equal, and equally bent, each of which rests at one end in *æquilibrio* against the other spring, and at the opposite end, presses against the body it is to move.

Consequently, by the *Leibnitzian* doctrine, to which, in this particular case, where the bodies are equal, we also agree, the two springs will give equal moving forces to the two bodies.

But the moving force received by the hindmost body from the hinder spring, was undoubtedly the moving force 1: for by that force given it in the direction backwards, the moving force 1, which it had before from the motion of the plane in the direction forwards, is exactly balanced and destroyed, the body remaining, as was observed before, in absolute rest.

Therefore the moving force received by the foremost body from the foremost spring, was also the moving force 1. And this, added to the other moving force 1, which it had before from the motion of the plane, makes the moving force 2, and not the moving force 4, as the *Leibnitzian* philosophers pretend.

Consequently, that body, which had before the velocity 1, and the moving force 1, and now has the velocity 2, has also the moving force 2: that is, the moving forces are proportional to the velocities.

Dynamical Principles, or Metaphysical Principles of

III. When the famous *Leibnitz* published * his new doctrine, by which he determines, that the force of a body in motion is to be measured by the square of the velocity, it raised a great controversy in the Mathematical World. The same author, in *April* 1695, published his

* *Act. Erud. Lips.* 1686.

Specimen

Specimen Dynamicum, in confirmation of this doctrine: and in one place makes uſe of the following expreſſion;

Mechanicks by the ſame. No. 479. p. 103. Mar. &c. 1746. *Preſented* Mar. 13. 1746.

" I arrived at the ſame true eſtimation of forces by different ways:
" one *a priori*, by a moſt ſimple conſideration of ſpace, time, and ac-
" tion, which I ſhall explain in another place. The other *a poſteriori*, by
" eſtimating the force by the effect which it produces in exhauſting it-
" ſelf."

He ſeemed to intend the publication of his *a priori*, which he promiſed to *explain in another place*, in *May* following: for towards the end of his *Specimen Dynamicum* he adds the following words;

" And now, having diſpelled error, we ſhall produce the true and
" really admirable laws of Nature, ſomewhat more diſtinctly, in the ſe-
" cond part of this eſſay, to be publiſhed in the month of *May*."

But, to our great misfortune, this ſecond part never made it's appearance in publick, either in the month of *May*, or in any ſubſequent month, or year, either in the *Leipſick Acts*, or any where elſe, tho' the author ſurvived above 20 years.

However, to clear this great man from the imputation of not having performed his promiſe, the world has lately been favoured with it in the *Commercium Literarum* between himſelf and another famous Mathematician, *John Bernoulli*.

Bernoulli, it ſeems, upon ſeeing the *Specimen Dynamicum*, wrote to *Leibnitz*, in *June* following, applauding ſome things, but at the ſame time being ſo far from approving his eſtimation of forces, that he even endeavoured to demonſtrate, that the forces of moved bodies are not in proportion to the ſquares of their velocities, but to the velocities themſelves. But at laſt, after ſeveral letters had paſſed between them, *Bernoulli* came over to *Leibnitz*, who, being willing to reward the docility of ſo eminent a diſciple, communicates to him his argument *a priori*, which he had hitherto kept to himſelf, and at the ſame time aſſigns the reaſon why he did not divulge it ſooner;

" I would not honour, ſays he *, with this clear light of truth, thoſe
" who did not receive as they ought thoſe arguments drawn from the
" affections of heavy, or other ſenſible bodies; wherefore I would not
" make them publick; but reſerved them to be communicated to
" thoſe, who had ſhewn themſelves to be equal judges."

Bernoulli therefore, *having ſhewn himſelf to be an equal judge*, and having *received as he ought*, thoſe arguments *a poſteriori*, that is, having come over entirely to the opinion of *Leibnitz*, was thought worthy of the *honour* to be admitted into theſe ſecret receſſes of Science.

" Becauſe, ſays the author †, I ſee you are on our ſide, I will
" freely communicate to you my principle of demonſtrating *a priori*
" the true eſtimation of forces; which I have ſometimes mentioned as
" being in my hands, but have never yet produced. For communi-

* *Jan.* 1696. † *Jan.* 1696.

" cating to you is committing a ſeed to a moſt fruitful ſoil, that it " may grow up into a large plant."

I cannot but commend the good gentleman for *committing* his *ſeed* to ſo *fruitful a ſoil:* and yet I cannot think him wholly free from deſerving ſome cenſure. For though he could work no effect on the *Papins*, *Catelans*, and other oppoſers of his doctrine, who *ſeemed to be incapable of converſion, by any demonſtrations, how ſtrong ſoever*, though he might think them unworthy of *this clear light of truth*, yet why did he envy it to the reſt of the learned world? I will not ſay, that it was the part of a good, and humane man, and of one who was deſirous to increaſe knowledge, to lay open to all an affair of ſuch moment, but if he had only ſtudied his own glory, preferably to every thing elſe, he ſhould have acted in this manner, that thoſe detractors might either have been immediately ſilenced, or condemned by all the world. Laſtly, as great men are not born for themſelves alone, or for a friend or two; but for all; is it not a little unjuſt, that *Bernoulli* and his diſciples ſhould alone enjoy *this clear light*, when we poor wretches are condemned to live in more than Cimmerian darkneſs. But it is well for us, that after 50 years of darkneſs, that light at laſt ſhines forth upon all. But behold the argument!

" 1. An action making duple, in a ſimple time, is duple, virtually " of an action making the ſame duple in a duple time; or the walking " of 2 miles in 1 hour, is duple, virtually, of the walking of 2 miles " in 2 hours.

" 2. An action making duple in a duple time, is duple, formally of " an action making ſimple in a ſimple time; or the walking of 2 miles " in two hours is duple, formally, of the walking of 1 mile in 1 hour.

" 3. Therefore an action making duple in a ſimple time is quadruple " of an action making ſimple in a ſimple time; or the walking of 2 " miles in 1 hour is quadruple of the walking of 1 mile in 1 hour.

" 4. If for duple we had ſubſtituted triple, quadruple, quintuple, &c. " the action would have come out noncuple, ſedecuple, 25ple; and " generally it appears, that equable, equitemporaneous, moving ac- " tions, are to equal moveable, as the ſquares of the velocities; or, " which is the ſame thing, that in the ſame or an equal body, the for- " ces are in a duplicate ratio of the velocities." *Q. E. D.*

Having read this argument, and out of regard to the great fame of the author, having conſidered it with much attention, I muſt confeſs, I could not diſcover the leaſt ſpark of truth in it, or even of common ſenſe. I ſhould have ſuſpected, that this had been owing to the weakneſs of my own eyes, which perhaps were dazzled by the too great brightneſs of the light, if a doubt of *Bernoulli* himſelf had not raiſed my ſpirits.

This ingenious perſon was ſo far from acquieſcing in *this clear light of truth*, that he not only made an objection, but even produced a double demonſtration.

" I do

" I do not see, says he *, what can be said by an adversary to the con-
" trary; unless perhaps, that the virtual action seems to be confounded
" with the formal action; denying the consequence, that *A* is the qua-
" druple of *C*, because *A* is the duple of *B*, virtually, and *B* the duple
" of *C* formally.

Having proposed this objection, he adds his demonstrations.

" 1. An action making duple in a simple time is virtually duple of
" an action making the same duple in a duple time."

" 2. An action making duple in a duple time is simple virtually of
" an action making simple in a simple time.

" 3. Therefore an action making duple in a simple time, is duple of
" an action making simple in a simple time. Or,

" 1. An action making duple in a simple time is simple formally of
" an action making the same duple in a duple time.

" 2. An action making duple in a duple time, is duple formally of
" an action making simple in a simple time.

" 3. Therefore an action making duple in a simple time, is duple of
" an action making simple in a simple time.

" You see the 2 arguments, which plainly conclude the same thing,
" but are quite contrary to your conclusion, and depend on that com-
" mon axiom, that those things which are equal to the same are equal
" amongst themselves, which indeed holds only in homogeneous quan-
" ties, as here in comparing a virtual action with a virtual, and a for-
" mal one with a formal, but not one with the other."

Thus *Bernoulli* with no less acuteness than modesty. But *Leibnitz*, in his letter dated in *March*, in the first place endeavours to take off *Bernoulli*'s objection.

" I do not well understand, says he, what you mean, when you say
" a virtual action is confounded with a formal one. For I do not here
" treat of an action as being either virtual or formal; but one action is
" duple of another, either virtually or formally. Virtually, when it is
" duple in estimation, tho' it is not duple in bulk, or congruence, as
" a ducat is the duple of a dollar: but formally, as a dollar is the du-
" ple of a half dollar. And you must know, that what is duple for-
" mally is duple also in virtue or estimation. Therefore as the inquiry
" here is only concerning virtue or estimation, there is no confusion of
" the different kind of quantities or estimations; for by virtually duple,
" I understand that which is so only virtually; but I call that formally
" duple, which is duple both formally and virtually."

It is not to be denied, that *Leibnitz* has a right to assign what sense he pleases to the words made use of by him, and that by this means he plainly takes off *Bernoulli*'s objection. But I could wish he had explained one thing, either of his own accord, or at *Bernoulli*'s request, by what virtue or by what estimation an action making duple in a simple

* *Feb.* 1696.

time, is duple of an action making the fame duple in a duple time. For if I am not greatly miftaken nothing can be more falfe.

He proceeds, " I might abftain from words ufed only for the fake of " a certain harmony, for as becaufe a ducat is the duple of a dollar, " and a dollar of a half dollar, I conclude that a ducat is the quadru- " ple of a half dollar; fo becaufe the walking of 2 miles in one hour " is the duple of walking 2 miles in 2 hours, and the walking of 2 " miles in 2 hours is the duple of walking 1 mile in 1 hour, it will fol- " low that the walking of 2 miles in 1 hour is the quadruple of walk- " ing 1 mile in 1 hour."

Thefe troubling words, *virtually* and *formally*, being new removed which had hitherto fouled *this clear fountain of truth*, Leibnitz not only took off *Bernoulli*'s objection, but brought him over entirely to his fide. " Your anfwer, fays he in his next letter, quite fatisfies me; for I fee " what you mean by thofe 2 terms: but your argumentation appears to " me very elegant, and that it ought no longer to be detained from " the publick; for it will give great weight to the arguments *a pofte-* " *riori.*"

Thus *Bernoulli* in his letter dated in *April*, and I would likewife ac- quiefce in the fame argument, if any one will fhew me, that it is as plain that the walking of 2 miles in 1 hour is the duple of walking 2 miles in 2 hours, as that a ducat is the duple of a dollar. For I fee that walk- ing 2 miles in 1 hour has duple the velocity of walking 2 miles in 2 hours; but I do not find it to be duple, but equal, fince the fame fpace is gone over in each walk.

But perhaps *Bernoulli* would not urge the matter any farther, as *Leib- nitz* feemed to be in a more than ordinary commotion, " I, fays he *, " dare not promife any thing great; but I hoped to be not guilty of a " moft open paralogifm, in an argumentation, which did not flip from " me on a fudden, but had been confidered by me for feveral years, " and was vaunted by me as a thing of fome moment." However, that *Leibnitz* was *guilty of a moft open paralogifm*, will be fhewn prefently, if I am not greatly miftaken.

I need not dwell upon *Leibnitz*'s examination of both *Bernoulli*'s de- monftrations, becaufe they depend upon the fenfe of the words *virtually* and *formally*, underftood differently from the meaning of *Leibnitz*. " I " took the terms, fays *Bernoulli* †, in a different fenfe from that in " which you now explain them."

But *Leibnitz*, being ftill in doubt what weight his firft demonftration would have with *Bernoulli*, adds another to it. " I add another, fays " he ‖, which, if you examine it to the bottom, comes to the fame as " the former, and yet it has it's own proper weight. Moving actions, " I mean equable ones, of the fame moveable are in a ratio compounded " of the immediate effects, namely the lengths run thro' and the velo-

* *Mar.* 1696. † *Apr.* 1696. ‖ *Mar.* 1696.

" cities.

" cities. Now the lengths, equably run thro' are in a ratio compound-
" ed of the times and the velocities. Therefore moving actions are in
" a ratio compounded of a fimple ratio of the times, and a duplicate
" one of the velocities; and fo, in the fame times, or elements of times,
" the moving actions of the fame moveable are in a duplicate ratio of
" the velocities, or if the moveables are different, in a ratio compounded
" of a fimple ratio of the moveables, and duplicate one of the velo-
" cities.

As *Bernoulli* had faid, in his letter dated in *April*, that he acquiefced in the former demonftration, but did not fay a word of the latter; *Leibnitz* afked him in *May*, " what he thought of the other demonftration " of the fame propofition, which (fays he) is a little more according to " the received form, tho' they both agree in the root."

Bernoulli therefore, when he could no longer avoid opening his mind, in his letter of *June*, thus expreffes himfelf.

" Your other demonftration of the propofition concerning the ratio of " moving actions, which you had alledged in the former, feems to me " to be contrived no lefs ingenioufly than the former, and, as you " exprefs it, more according to form, tho' in the bottom of the thing " they both coincide. For nothing is more evident to me, than that " moving actions ought to be meafured by their immediate effects; if " therefore the lengths gone thro' and the velocities, unlefs any one will " obftinately have the velocity to be rather the caufe, are the effects of " an immediate action, and indeed the only ones, of which one does " not depend on the other, or is not included in the other, the mov- " ing actions will neceffarily be in a ratio compounded of the lengths " and the velocities; and fo in equal times in a duplicate ratio of the " velocities."

It is plain that *Bernoulli* in this anfwer approves of the fecond demonftration in appearance, but in reality condemns it, tho' with the greateft caution and modefty. For he not only hints that the velocity is rather the caufe than the effect of an action, but he reftrains his affent to this condition, that *one* of the effects mentioned by *Leibnitz*, namely of the length gone thro' and the velocity, *does not depend on the other, or is not included in the other.* Now therefore as it is very evident, that the length gone thro' does depend on the velocity, and is included therein, it is plain that the demonftration is faulty in the opinion of *Bernoulli.*

Leibnitz, in his next letter dated in *June*, gave a copious and diftinct anfwer to many other things, but to thefe tacit objections of *Bernoulli*, he anfwers lightly, diffembling their force, and as if he was treating of fomething elfe, only juft fays,

" But as I now eftimate an action by the compound ratio of it's prin- " ciples, power and time; fo I had eftimated it a little before by the " compound ratio of what it performs; an extenfive or material effect, " namely of the length, which I ufually call an effect κατ' ἐξοχήν, and

" an intensive or formal effect. For it is required that much should be " performed and soon. You see now that both the estimations agree " together."

By the obscurity of this answer, whether affected, or natural to *Leibnitz*, it is easily seen that he would have the velocity to be taken for the effect of an action, which *Bernoulli* had hinted was rather the cause, but that he did not dare to name it openly, tho' he understands it under the name of an intensive or formal effect, which the action *performs*. Besides as to the other objection of *Bernoulli*, that tho' the velocity is in the highest degree the effect of an action, as well as the length gone thro'; yet as one of these effects depends on the other, or is included in the other, and certainly the length gone thro' depends on the velocity, an action ought not to be measured by those effects; as to this, I say, he observes a profound silence.

The second demonstration therefore seems to be given up by *Leibnitz* as well as *Bernoulli*; and indeed in all their subsequent letters, I do not find the lest mention of it.

Moreover, that first demonstration, which comes to the same with the other, that is, a true one with a false one does not seem to be wholly free from exception, either with *Bernoulli*, or with *Leibnitz* himself.

For *Bernoulli*, tho' he had declared in *April*, that *it quite satisfied* him, that *he acquiesced* in it, and that it was *very elegant, and ought no longer to be denied to the publick*, in *August* however did not know what *Leibnitz* meant by the word *action*, on which that whole demonstration depends. " You ought, says he, to define what you mean by action; " otherwise nothing can ever be demonstrated." This was a just admonition, but to no purpose; for in the letter which *Leibnitz* wrote in answer to this, you will not find a tittle of that definition so highly necessary.

But *Leibnitz* himself, in his letter dated in *June*, expresses himself thus; " my demonstration *a priori*, for our estimation of forces, depends upon a certain supposition. Namely, *that an action which does* " *any thing uniformly, in a simple* time, is duple of an action doing the " same thing uniformly, *in a duple time*. This supposition ought to " have been granted by *Catelan* and the rest, with whom I had disputed." But what if they will not grant it? why then the demonstration, which depends upon this supposition, falls to the ground, at least till you demonstrate that supposition.

But, " I have not yet indeed found out a way of demonstrating this " proposition *a priori* by the way of congruency, nay not even this, " that *an action doing the same thing, in a shorter time, is greater*; which " ought to have been the beginning."

Therefore since that so much boasted demonstration *a priori* stood in need of another demonstration, which *Leibnitz* had not yet discovered, nor ever after did discover, nor any mortal ever will discover, it is no

wonder

wonder that this *ſeed*, tho' *committed to a moſt fruitful ſoil*, did not *grow up into a large plant*. For *Bernoulli* took a final leave of this *clear light of truth*, when he ſaw it dwindle a way to a meer ſnuff.

But a gentleman of much higher courage, the learned *Chr. Wolfius*, having attempted to treat the theory of forces after a geometrical manner, communicated to the publick, in the firſt volume of the *Comment. Acad. Imp. Petropol.* under the title of *Principia Dynamica*. "When he had "communicated," part of this "*in* 1710 *to the moſt illuſtrious* Count "de Herberſtein, *to the moſt illuſtrious* Leibnitz *and others*, Leibnitz, "*in a letter* 1711, *ſaid that it agreed with his, which he had communi-* "*cated to the famous* John Bernoulli, Jacob Herman, *and others, con-* "*firming it in theſe words:* I lay down this calculus of pure forces or "actions. Let the ſpace be s, the time t, the velocity v, the body c, "the effect e, the power p, the action a. Then tv will be in equal "motion as s, e as cs, tp as a. And theſe may be aſſumed without a "demonſtration. Add, what is to be demonſtrated, ev as a. Hence "many other theorems may be demonſtrated; for inſtance p as cv^2. "For tp as ev: but e as cs, and s as tv. Therefore tp as ctv^2, or p "as cv^2. And in theſe is contained part of my *Dynamick*, abſtracted "from ſenſible things, tho' it is afterwards verified by experiments." "I do not doubt therefore, ſays *Wolfius*, but I have here propoſed *Dy-* "*namical* principles, which are conformable to the ſentiment of *Leib-* "*nitz*."

And this indeed is manifeſt of itſelf, as the theorems of *Wolfius* exactly agree with the algebraical notations of *Leibnitz*; but whether theſe principles are as conformable to truth as they are to the ſentiment of *Leibnitz*, is worth the while to examine. But I find one thing particularly worthy of obſervation in theſe notations, a as ev, *which is to be demonſtrated:* whence *Leibnitz* ſeems, not even then, after 16 years, to have found out a demonſtration of the ſuppoſition formerly put off to *Bernoulli*; that an action doing any thing, in a ſimple time, is duple of an action doing the ſame thing in a duple time; ſince an action doing any thing in a ſimple time, does it with twice the velocity of an action doing the ſame thing in a duple time. But how *Wolfius* demonſtrates this, we ſhall examine preſently.

For the moſt illuſtrious Imperial Academy of Sciences at *Peterſburg* was pleaſed to make me a preſent of the 3 firſt volumes of their Commentaries, and at the ſame time to ſignify that it would not be diſagreeable to them, if I would ſend them any obſervations of mine to be inſerted in their Commentaries. In conſequence of this, having ſent a paper relating to my theory of the action of capillary tubes, which was well received by the moſt illuſtrious Academy, and publiſhed in their Commentaries, I ſoon after took the liberty of ſending another under the title of *Principia Dynamica*.

For as I ſaw that the celebrated *Wolfius* propoſed to explain clearly and diſtinctly, and after a geometrical manner, thoſe things which had been

been less perspicuously handled by *Leibnitz*, so that it was not easy to discover truth from falsehood; and yet that they agreed exactly together in the main; I was willing to take the opportunity of bringing that theory to an accurate examination.

With this view I transcribed exactly all that I thought was rightly delivered by *Wolfius*, and inserted them in my *Dynamical Principles*; what was wanting I supplied; and what seemed to be false I corrected. When I had done this, I sent it 12 years ago with all due respect to the Imperial Academy. That it was read in their publick assembly, and that thanks were ordered for the communication, I was informed by the learned *Muller*, who was then at *Petersburg*, and setting out for the expedition to *Kamkatschi*.

But afterwards, when after so many years I found no mention of that paper in the Commentaries, I inquired last year of a friend, what was become of it. He answered me at first, that no such paper had ever been presented to the Academy. I answered, that it had certainly been presented, that it was read in a publick assembly in *June* 1735, and that thanks were returned me for it. At last, on examining their registers, it was found to be true; but the paper itself could no where be found, nor could any one imagine by what accident it was lost. However the most illustrious Academy were pleased to give me my choice either of sending another copy to *Petersburg*, to be inserted in their Commentaries, or of publishing it in our Philosophical Transactions.

When I had examined my own papers, I could not find a perfect copy of it any where, whether it had been lost by some accident in moving twice from one house to another, or whether I had written only that copy which I had sent to *Petersburg*. I found however an imperfect copy, which I supplied as well as I could, and now present and dedicate it to the Royal Society of *London*, and I hope with better success.

Dynamical Principles.

We often see, when persons are engaged in law suits, that a thing which was at first easy and plain, has by the ill management of the advocates been carried thro' all the turnings and windings of the law, till it has ended in a difficult and almost inextricable cause. In such a case, if any lawyer shall shew a short and plain way of coming to a conclusion, I shall think he deserves very well of both parties, on which side soever the question is decided.

In this light I consider the behaviour of the famous *Wolfius* with regard to the controversy concerning moving forces, which has now for many years engaged the learned world. For if he has not attained to the truth, he has certainly shewn the way by which others may with safety and ease arrive at the truth.

Treading therefore in his steps and those of the illustrious *Leibnitz*, whom he professes to follow. I shall endeavour to explain the *Dynami-*

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cal Principles, to use their own term, with as much perspicuity as is possible.

To which end I resolved to consider only one very simple case, of a body endued with a *Vis viva*, which is moved with an uniform motion, that is, without any impediment, either of a resisting medium, or of any opposite bodies whatsoever, plainly according to the positions of *Wolfius*. And if the candid reader shall observe, that I have taken this learned gentleman's definitions and axioms, nay and the subsequent propositions, excepting 1 or 2, and their demonstrations, without changing a word, I must give him to understand, that I did this professedly, because I think they can neither be more clearly expressed, nor more certainly demonstrated.

" I call that *Vis viva* with *Leibnitz*, or merely *vis* or *force*, which " adheres to a local motion." *Definition* 1.

" A *pure force* is that which is not resisted in acting by any con- " trary force." *Def.* 2.

" Therefore a pure force remains unvaried in the whole time of " action. *Corollary*

" Such a force exerts itself in an equable motion, if it be con- " ceived to be made in an unresisting medium. For in whatsoever " interval the moveable body is moved forwards, the same celerity al- " ways subsists, consequently the moving force is the same. Therefore " the effect, which it produces, does not in the lest exhaust it. *Scholium.*

" A *pure action* is that which is exercised by a pure moving force." *Def.* 3.

" Such is the action of a moveable carried with an equable motion " in an unresisting medium. *Schol.*

" An uniform action is that, which is duple in a duple time, triple " in a triple, &c. or in general, which is as the time. *Def.* 4.

" Such an action has place in an equable motion, when a moveable " continues to be moved with the same celerity, namely if the motion " is conceived to be made in an unresisting medium. *Schol.*

" *The effect of a moving force beyond the conflict* is the translation of a " moveable thro' a space." *Def.* 5.

" If 2 or more equal moveables are moved with equal celerity, " the force of them is the same." *Axiom* 1.

" The same action is performed by the same force in the same " time." *Axiom* 2.

" That a greater action is performed by the same force in a longer " time than in a shorter, and that a greater action is performed in the " same time by a greater force than by a less, no one doubts. There- " fore the quantity of an action depends on the quantity of forces and " time. Wherefore if the forces are equal, and the time the same, the " action also must be the same." *Schol.*

" If the same moveable is transferred thro' the same space, the effect " is the same." *Ax.* 3.

" We

Schol. " We suppose the motion to be made in an unresisting medium, or " at least abstract it from the action, which is spent in overcoming the " resistance of the medium: which may be, whilst we take no account " of the time in which the effect is produced."

Theorem 1. " If unequal bodies are moved with the same celerity, the forces are " as the masses."

The demonstrations of this and the 8 following theorems, about which we have no controversy with the *Leibnitzians*, were set down in *Wolfius*'s own words in the paper sent to *Petersburg*: but here we thought proper to omit them, to avoid prolixity.

Theorem 2. " Uniform actions performed in the same time are to each other as " their forces."

Theorem 3. " Uniform actions, performed with equal forces, are to each other, " as the times in which they are performed.

Theorem 4. " Uniform actions are in a ratio compounded of the times and " forces."

Theorem 5. " Unequal forces perform the same action in times reciprocally pro- " portional to each other."

Theorem 6. " If 2 equal moveables are transferred thro' unequal spaces, the effects " are as the spaces."

Theorem 7. " If any 2 moveables are transferred thro' the same space, the effects " are as the masses."

Theorem 8. " If any 2 moveables are transferred thro' any spaces, the effects are " in a ratio compounded of the masses and spaces."

Theorem 9. " In an equable motion, the effects are in a ratio compounded of the " masses, celerities, and times.

Theorem 10. " Actions, by which the same effect is produced, are as the celeri- " ties."

We are now come to that theorem, on which the whole affair turns. If this is true, the *Leibnitzian* doctrine is to be embraced, if not, it is to be rejected. Therefore the demonstration of this theorem must be diligently examined.

It is divided by *Wolfius* into 3 cases; but as the second and third depend on the first, we shall consider only this one.

Demonstration of the first case. " If moveables are equal, and the same effect is produced in a differ- " ent time, the velocities will be as the times reciprocally in which it is " produced; that is, a body, which produces an effect in the time $\frac{1}{2}$ *T*, " is moved with the velocity 2 *C*, when another, which produces the " effect in the time *T*, is moved with the simple velocity *C*, and so " on. Now it is evident, that an uniform action is duple, which pro- " duces the effect in $\frac{1}{2}$ the time, triple, which in subtriple, and so on."

But do you say, Mr *Wolfius*, that this is evident? what if I should deny it? what if I should say that any action, which produces the same effect, is the same in what time soever it produces it. This is the very supposition of *Leibnitz*, of which, in his letter to *Bernoulli* dated in 1696, he says he has not discovered a method of demonstrating

a priori,

a priori, and in his letter to your ſelf dated 1711, ſays is ſtill to be demonſtrated. And yet you do not endeavour to demonſtrate it, but ſay it is evident, I deny it's being evident, and ſo your demonſtration falls to the ground, and the ſuppoſition itſelf therewith.

But before we ſubſtitute a new one, let us ſee a little, what is underſtood by action, and what by effect.

Wolfius, after the example of *Leibnitz*, has omitted the definition of action. He has only ſhewn what is a *pure* action, namely that which is free from all impediment; and what is an *uniform* action, namely that which increaſes in proportion to the time: but what he means by action itſelf he has no where determined. But till this is done, *nothing can ever be demonſtrated*, as *Bernoulli* adviſed *Leibnitz* long ago, but in vain.

If I might venture to ſupply this defect, I would aſcribe the ſame definition to action, which *Wolfius* has given of effect; ſince there ſeems to be no other difference between action and effect, than that action, if I may ſo ſpeak, is an effect *in fieri*, and effect an abſolute action, or one that is perfected. For in *Wolfius*'s example, a *Vis viva* is that which transfers a moveable thro' a ſpace; therefore the action of a *Vis viva* is the *tranſlation of a moveable thro' a ſpace*; and the effect of a *Vis viva* is alſo the *tranſlation of a moveable thro' a ſpace*; or rather, an effect is a moveable already transferred thro' the ſame ſpace.

But generally, an action is the preceder of an effect; or rather, an action is that by which any thing is effected, but an effect is the thing itſelf which is effected.

I do not boaſt of theſe definitions as being perfect: but yet I think they are without any danger of being miſtaken, eſpecially if I make the thing a little plainer by ſome examples.

If I write a page, my action will be the writing of a page, and the effect will be a page written.

If a workman whitens a wall, his action will be the whitening of a wall, and the effect will be a wall whitened.

If a labourer digs a garden, his action is the digging of a garden; and the effect is a garden digged.

Any one may eaſily conceive an infinite number of examples; and indeed I ſhould have been aſhamed to dwell ſo long on things ſo plain, and in a manner frivolous, if theſe very things falſly conceived had not thrown ſo many great men into the moſt grievous errors. For

— — — Hæ nugæ ſeria ducunt
In mala.

Of equal actions the effects are equal. *Our 10th*
Let any *Vis viva A* perform any action, and let there be ſuppoſed any *Theorem.* other *Vis viva B*. Now that the *Vis viva B* may perform an action equal to the *Vis viva A*, it is neceſſary that the *Vis viva B* ſhould act exactly as much as the *Vis viva A* has acted. Therefore after the com-

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pletion of the action of B, as much will be acted by the force B, as has been acted by the force A; that is, the effect of the *Vis viva B* will be equal to the effect of the *Vis viva A*, the actions of which were equal. *Q. E. D.*

Our 11th *Theorem.* Actions are in proportion to the effects.

Demonstration. Let the effect e be produced by the action a. Therefore another effect e equal to the first will, by *Theor.* 10, be produced by another equal action a: consequently, the effect b will be produced twice by the action a. In like manner it appears that the effect thrice e must be produced by the action thrice a, &c. Nay *in genere*, that the effect ne $(=E)$ must be produced by the action na $(=A)$. Therefore $A : a :: E : e$, that is, the actions are in proportion to the effects.

Our 12th *Theorem.* Forces are in a ratio compounded of the masses and velocities.

Demonstration. By *Theor.* 4. actions are in a ratio compounded of the times and forces. By *Theor.* 11. actions are in proportion to the effects. Therefore effects are in a ratio compounded of the times and forces. But by *Theor.* 8. effects are in a ratio compounded of the masses and spaces. Therefore a ratio compounded of the times and forces, is equal to a ratio compounded of the masses and spaces. Wherefore forces are in a ratio compounded of the masses and spaces directly, and of the times reciprocally, that is, in a ratio compounded of the masses and velocities. *Q. E. D.*

Part of a letter from Mr Turberville Needham *to* James Parsons, *M. D. F. R. S. of a new* Mirror, *which burns at* 66 *feet distance, invented by M.* de Buffon, *F. R. S. and Member of the* R. Acad. *of* Sciences *at* Paris. N°. 483. p. 493. Mar. *&c.* 1747. *Read* April 30. 1747.

IV. 1. I have been at the king's garden, and am just returned: I there learned, that this morning they have been trying some experiments with a new-constructed reflecting mirror or mirrors with success: I knew indeed some time ago, that they had been upon the design; and M. *de Buffon* had acquainted me with the theoretical part of the whole. I had even seen a part of it executed; but as they had not then essayed it, I would take no notice of it: In one word, it is *Archimedes* revived; and the credit of antiquity, in this point, is in some measure re-established. This machine, for so I must call it, consists of 140 small plain mirrors, each of about 4 by 3 Inches square; they are fixed at about $\frac{1}{4}$ of an Inch distance from each other, upon a large wooden frame about 6 feet square, strengthened with many cross bars of wood for the mounting of these mirrors. Each of them has three moveable screws, which the operator commands from behind, so contrived, that the mirror can be inclined to any angle in any direction that meets the sun; and by this means the solar image of each mirror is made to coincide with all the rest.

There are in all, as I told you, 140 mirrors; but they tried the experiment this morning with 24 only; for so many, and no more, were then ready for the purpose: the effect was, that, in very few seconds of time, a combustible matter they had prepared with pitch and tow, daubed upon a deal-board, was set on fire, and burn'd vigorously at the distance of 66 *French* feet. Judge now of the effect 140 will produce; and

and whether the invention may not be improved to the height of all that has been advanced of *Archimedes* by the Ancients. The only difficulty they found was, to make the ſolar images of the mirrors coincide; but this is owing to the yet imperfection of their method of mounting, which may be eaſily improved.

The dimenſions I have given in of the mirrors and frame were only gueſſed at from view, for I have not meaſur'd them; ſo you muſt not expect they will ſquare or tally mathematically in the utmoſt rigour. Nor indeed did I think it neceſſary to do any more; for the dimenſions of themſelves are purely arbitrary.

Extract of a letter from the Marquis Nicolini, *F. R. S. to the* Pref. *concerning the ſame* Mirror *burning at* 150 *Feet Diſtance.* ibid. p. 495. *Read* April 30. 1747.

2. You know that the affair of *Archimedes* ſetting the *Roman* fleet on fire by means of burning-glaſſes, has been look'd upon as a thing impoſſible and romantic. *Deſcartes* poſitively denied the fact, which had been believed for ſo many ages; and our modern philoſophers, after many trials, and various reaſonings, have been of the ſame opinion. But M. *de Buffon*, being aſked if it might be poſſible to invent a *Phaometer*, or *machine* for meaſuring the *intenſity* of *Light*, hath diſcovered by trial, that light was able to produce great effects in a *focus* at a great diſtance, if one made uſe of a great numbers of diſks, which would reflect ſo many images of the ſun and fling them all into one place. He put together therefore a ſort of *Polyedron*, conſiſting of 168 ſmall mirrors, or flat pieces of looking-glaſs, each 6 inches ſquare; by means of which, with the faint rays of the ſun, in *March*, he ſet on fire ſome boards of beech wood at 150 feet diſtance. By increaſing the numbers of mirrors, he hopes to be able to do the ſame 900 feet off.

His machine has beſides, the conveniency of burning downwards or horizontally, as one pleaſes; and it burns either in it's greater *focus*, or in any nearer interval, which our commonly known burning-glaſſes have not, their *focus* being fix'd and determined.

Perhaps this machine may afford a manner of meaſuring either light, or the different degrees of heat of burning bodies. The difficulty is to find the method of marking the degrees, and of fixing a point of compariſon; for the point of kindling will not determine it; becauſe that chiefly depends upon the greater or leſs degree of inflammability of different combuſtible bodies*.

Abſtract from a letter ſent by M. Buffon, *Memb. of the* R. Acad. *of* S. *at* Paris, *&c. to* M. Folkes, *Eſq; Pr. R. S. concerning his Re-invention of* Archimedes's burning

3. As what I read ſome time ſince to our *Royal Academy* upon the ſubject of my re-invention of *Archimedes*'s burning *Specula*, cannot appear in our Memoirs before the year 1747, I think of publiſhing by themſelves my obſervations upon theſe mirrors, as ſoon as I ſhall ſatisfy myſelf upon certain particulars, by ſome new experiments I am now prepar-

* Mr *Maupertuis*, in a letter to the *Preſident*, dated at *Potzdam*, *May* 20. 1747. ſays, that his friend *Buffon* has recovered the burning-glaſſes of *Archimedes*; that with 168 plane glaſſes, each 6 inches ſquare, he has melted a ſilver plate, at the diſtance of 60 feet, and fired pitch'd boards at 150. Each *ſpeculum* is moveable, ſo as, by the help of 3 ſcrews, to be ſet to a proper inclination for directing the rays towards any given point.

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Specula. No. 489. p. 504. Oct. *and* Nov. 1748. *Read* Oct. 27. 1748.

ing to make. The *speculum* I have already constructed, and which is but 6 feet broad and as many high, burns wood at the distance of 200 feet, it melts tin and lead at the distance of above 120 feet, and silver at 50. The theory which led me to this discovery is founded upon two important remarks; the one, that the heat is not proportional to the quantity of light; and the other, that the rays do not come parallel from the sun. The first of those, which appears to be a paradox, is nevertheless a truth of which one may easily satisfy one's self, by reflecting that heat propagates itself even within bodies; and that when one heats at the same time a large superficies, the firing is much quicker than when one only heats a small portion of the same.

The motion of Projectiles *near the Earth's Surface consider'd, independent of the properties of the Conic Sections; in a letter to* M. Folkes, *Esq; Pr. R. S. by Mr* Tho. Simpson, *F. R. S.* No. 486. p. 137. Feb. *and* Mar. 1748. *Read* Feb. 4. 1747.

V. After so much as has been already said upon the motion of projectiles *in vacuo*, it may seem needless to attempt any thing further on that head; nevertheless, as a thorough knowledge in the art of Gunnery is become more than ever necessary, and as gentlemen employ'd in the practice of that art are (I am sensible) too often deterr'd from applying themselves to the theory, by the difficulties they imagine they shall meet with in the conic sections, you will, I hope, pardon the liberty I have taken, in troubling you with my thoughts on a subject, in which little or nothing new is to be expected besides the method.

When I first drew up this paper (which was about two years ago) I did intend, had health permitted me to make the proper experiments, to have also attempted something with respect to the resistance of the atmosphere, whereof the effects are indeed too considerable to be intirely disregarded: but if the amplitude of the projection, answering to one given elevation, be first determined by experiment (which our method supposes) the amplitudes in all other cases, where the elevations and velocities do not very much differ from the first, may be determined, by the proportions here laid down, to a sufficient degree of exactness. Because, in all such cases, the effects of the resistance will be nearly as the amplitudes themselves; and were they accurately so, the proportions of the amplitudes, at different elevations, would be exactly the same as *in vacuo*; which proportions I now proceed to determine.

Prob. I. *Let two balls be projected with the same celerity at different, but given elevations, 'tis proposed to determine the ratio of the times of their flight, of their greatest altitudes, and of their horizontal amplitudes.*

Fig. 44. Let Pq, Fig. 44. represent the plane of the horizon, PEQ and peq the paths of the projectiles, described in the flight; moreover let QPT and qpt be the given angles of elevation, and let PQ and pq be bisected in H and h; drawing HE, QT he and qt, all perpendicular to Pq: and making the sine of $QPT = S$, it's co-sine $= C$, the sine $qpt = s$, it's co-sine $= c$, and radius $= r$.

Therefore,

Therefore, since the distances descended by heavy bodies (whether from a point at rest, or from the right lines in which they *would* move, if not acted upon by gravity) are known to be as the squares of the times, QT will be to qt, as the square of the time of describing PEQ (or of that wherein the ball would move uniformly over the space PT with it's first velocity at P) is to the square of the time of describing peq (or of that wherein the other ball would move uniformly thro' the length pt). But the celerities at P and p being equal, by hypothesis, the times in which the said lines PT and pt would be uniformly described, are manifestly, as the lines themeselves: whence the squares of those lines must, also, be as the squares of the times, and, consequently, as the distances descended: that is, $Pt^2 : pt^2 :: TQ : tq$.

Now, by plane trigonometry $TQ = \frac{S \times PT}{r}$ and $tq = \frac{s \times pt}{r}$; therefore $PT^2 : pt^2 \left(:: \frac{S \times PT}{r} : \frac{s \times pt}{r} \right) :: S \times PT : s \times pt$; whence, by dividing the antecedents by PT, and the consequents by pt, we have $PT : pt :: S : s$; from which it appears, that the times of flight are directly as the sines of elevation.

Again, the times of describing EQ and eq (which are the halves of the wholes) being also to one another as $S : s$, and the distances EH, eh descended in them, as the squares of the times, it likewise follows, that $S^2 : s^2 :: EH : eh$; or that the greatest altitudes are as the squares of the sines of elevation.

Moreover, because (by Trigonometry) $PT = \frac{r \times PQ}{C}$ and $pt = \frac{r \times pq}{c}$, and it has been already proved, that, $S : s :: PT : pt$, it follows, that $S : s :: \frac{r \times PQ}{C} : \frac{r \times pq}{c}$; whence, by multiplying the antecedents by $\frac{2C}{r}$ and the consequents by $\frac{2c}{r}$, it will be $\frac{2SC}{r} : \frac{2cs}{r}$ $(:: 2PQ : 2pq)$ $:: PQ : pq$. But $\frac{2SC}{r}$ is known to be the sine of double the angle whose sine is S, and co-sine C, &c. Therefore the horizontal amplitudes are to one another, as the sines of the double elevations.

Hence it follows, that the greatest amplitude possible will be, when the elevation is ½ a right angle, or 45° (because the sine of 90° is the greatest of all others). *Coroll.* 1.

Therefore, if the greatest amplitude be given (from experiment) the amplitude answering to any proposed elevation, above, or below, 45°, may from hence be found: for it will be as the radius, to the sine of double *Corol.* 2.

double the given elevation, so is the greatest, to the required, amplitude.

Corol. 3. Hence, also, the altitude of the projection may be known; for QT, when the angle QPT is half a right angle, will be $= PQ$; and therefore HE ($\frac{1}{4}TQ$) $= \frac{1}{4}PQ$; also, in this case, $S^2 = \frac{1}{2}r^2$; whence our proportion $S^2 : s^2 :: HE : be$ will here become $\frac{1}{2}r^2 : s^2 :: \frac{1}{4}PQ : be$; from whence it appears, that, as the square of the radius is to the square of the sine of any given elevation, so is half the greatest horizontal amplitude, to the altitude of the projection. Hence it also follows, that the height to which the ball would ascend, if projected directly upwards, is just half the greatest amplitude.

Corol. 4. Therefore, since it is well known, that a body *in vacuo* ascends and descends with the same velocity; and that the distances descended are as the squares of the velocities; it follows, that the amplitudes, at the same elevation, with different velocities, will also be to one another as the squares of the velocities; because they are as the greatest amplitudes, with the same velocities (by *Corol.* 2.) and these are as the distances perpendicularly descended (*by the precedent*). Whence, *universally*, if both the elevations and the velocities differ, the amplitudes will be to each other in a *ratio* compounded of the *ratio's* of the sines of double the angles of elevation, and of the duplicate *ratio's* of the velocities, or impelling forces.

Prob. II. *The angle of elevation, and the greatest horizontal amplitude, being given, to find at what distance the piece ought to be planted, to hit an object, whose distance, above or below the plane of the horizon, is also given.*

Fig. 45, 46. Let AB, *Fig.* 45, 46. be the plane of the horizon, BC the perpendicular height or depression of the object, and AB the required distance: also let BC be produced to meet the line of direction AD in D, and let P be the place where the path of the projectile would meet the horizon; moreover, let PQ be perpendicular to AP, and CN parallel to AD. Then, by the preceding problem, it will be as radius : the sine of $2BAD$: : the given (or greatest) amplitude : AP; which therefore, is known.

Moreover, the areas of similar triangles being as the squares of their homologous sides, we have $AP \times PQ : AB \times BD :: AQ^2 : AD^2$. But $AQ^2 : AD^2 :: AB \times BD :: QP : DC$ (from principles already explained) therefore, by equality, $AP \times PQ : AB \times BD :: QP : DC$; and consequently $AP : AB :: BD : CD$; but (because of the parallel lines CN and AD) $BD : CD :: AB : AN$; whence, again by equality, $AP : AB :: AB : AN$; therefore, by division, $AP : BP :: AB : BN$; and, consequently $AP \times BN = BP \times AB$.

Let AP be now bisected in O; then $BP \times AB$ being $= AO^2 - OB^2$ (in the first case) and $= OB^2 - AO^2$ (in the second case), we shall

therefore

therefore have $OB^2 = AO^2 \mp AP \times BN = AO \times \overline{AO \mp 2BN}$: whence the distance AB is likewise known. *Q. E. I.*

Hence, if the elevation, and the greatest amplitude, together with the distance AB of the object be given, the height or depression of the ball in the perpendicular BCD will be known: for it is proved, that $AP : BP :: BA : BN$; whence BN is known: but, as the radius to the tangent of BNC (BAD): so is BN to BC. *Corol.*

The greatest horizontal amplitudes of the piece, together with the distance and height (or depression) of the object being given, to find the direction or angle of elevation. Prob. III.

Let BC, *Fig.* 47, 48. be the perpendicular height or depression of the object, AB it's given horizontal distance, and AH the required direction; also let PQ *Fig.* 49. be the greatest amplitude (answering to 45° of elevation); draw AC, in which produced (if need be) take $AG = PQ$; make MGO perpendicular to AG, meeting AB produced (if need be) in O; and from the centre O, with the interval OA, let a circle be described, intersecting AG, produced in E, and the line of direction AD in H; join E, H, and let HI, AN and QR, be perpendicular to AE, AO, and PQ respectively, and let BC, produced, meet AH in D. Fig. 47, 48. Fig. 49.

It will appear, from what has been said above, that $AD^2 : PR^2 :: DC : RQ$; therefore PR^2 being $= 2PQ^2 = 2AG^2 = \frac{1}{2}AE^2$, and $RQ = PQ = \frac{1}{2}AE$ (by construction), we have $AD^2 : \frac{1}{2}AE^2 :: DC : \frac{1}{2}AE$, and therefore $AD^2 = AE \times DC$.

Now, the triangles ADC, AEH, being equiangular (because $ADC = DAN = AEH$, and DAC common to both) we likewise have $AD : DC :: AE : EH$, and consequently $AE \times DC = AD \times EH = AD^2$ (*per* above); whence $EH = AD$. Therefore, as the triangles ADB and EHI are equiangular, they are equal in all respects; and so $HI = AB$: whence follows this easy construction.

Having described the circle AEF, as above directed, and drawn MG perpendicular to AE, take Gn equal to AB, and thro' n, parallel to AE, draw Hh, cutting the circle in H and h; join A, H, and A, h; then either of the directions AH or Ah, will answer the conditions of the problem. From this construction we have the following calculation, *viz.* *Construction.*

As AB is to BC, so is AG to OG; which added to, or subtracted from, Gn (AB) gives On: then, it will be, as $AG : On ::$ the co-sine of OAG : co-sine of HOn ($= HAh$) the difference of the two required elevations; whence the elevations themselves are known. *Q. E. I.*

Hence, if the elevation of the piece, with the distance and the height (or depression) of the object be given, the greatest horizontal amplitude may be found: for it will be $AB : BC ::$ radius : tang. of BAC; whence CAD is also known. *Corol.* 1.

Then, $S.CAD : S.ACD\ (AHE) :: AD\ (HE) : AE$.

And, $S.ADC$: radius $:: AB : AD$.

Therefore,

Therefore, by compounding these proportions, we have $S.CAD \times S.ADC$: radius $\times S.ACD :: AB : AE$; which is equal to twice the required amplitude, by construction.

Corol. 2. Moreover, if the elevation, and the greatest horizontal amplitude be given, the amplitude of the projection on any ascending or descending plane AE, whose inclination FAE is also given, may from hence be derived. For, $S.AHE$ (ACD) : $S : EAH$ $(CAD) :: AE$ $(2PQ)$: EH (AD) and $S.ACD : S.ADC :: AD : AC$; whence, by compounding the two proportions, $Sq^r. S.ACD : S.CAD \times S.ADC :: 2PQ : AC$; from which AC is known.

Corol. 3. Since it appears, that the triangles ADB and EHI are equal and alike in all respects, and therefore, the horizontal distance AB, *universally*, equal to the perpendicular HI, it is manifest, that, when HI is the greatest possible, AB will also be the greatest possible; in which circumstance AC (if the angle FAE be given) will likewise be the greatest possible: and this, it is evident, must be, when HI coincides

Fig. 50, 51. with MG, or when the angles HEA and HAE are equal, *Fig.* 50, 51. at which time the point D coincides with H; because AD and EH are always equal to each other. Therefore, since, in this case, HAE (HEA) is $= NAH$, it follows, that the amplitude, on any inclined plane AE, will be the greatest possible, when the line of direction AH bisects the angle made by the plane and zenith.

Corol. 4. Hence the greatest amplitude on any inclined plane may also be known; for the right-angled triangles AOG and HOB, having $AO = HO$ and the angle O common, are equal in all respects; and therefore, as tang. of AHG (BAH the angle of elevation) : tang. of CHG (CAB the plane's inclination) $:: AG : GC$; whence $AC = AG \mp GC$ is also known.

Corol. 5. Hence, also, if the greatest amplitude on an inclin'd plane be given, the greatest horizontal amplitude may be determined: for, radius : $S.BAC :: AC : BC = CG =$ the difference of the given, and the required, amplitudes.

Corol. 6. But if, instead of the plane's inclination, the perpendicular height, or depression, of the object be given; then, AC $(AG \mp BC)$ being to BC, as radius to the sine of BAC, and radius : co-tang. $BAC :: BC : AB$; the greatest distance AB, at which the ball can possibly hit the object, will from hence be given: which distance (because $AC = AG \mp BC$, and $AB^2 = \overline{AC + CB} \times \overline{AC - BC}$) will also be expressed by $\sqrt{AG \times \overline{AG \mp 2BC}}$. Hence the greatest horizontal amplitude of a ball, projected from a given height above the plane of the horizon is

Fig. 50. known: for ST, *Fig.* 51. may here be supposed to represent, the plane of the horizon, and SA the given height; and then SC, being equal to AB, is given from above $= \sqrt{AG \times \overline{AG \mp 2BC}}$.

But,

But, if the horizontal distance AB be given, and it be required to find the greatest height the ball can possibly reach in the perpendicular BCD; we shall have HG (AB) : AG : : radius : tang. of the elevation (BAH or AHG); and radius : tang. BAC ($2BAH \backsim 90°$) : : AB : BC; which therefore is known. But (because $AC \pm BC = AG$, and $\overline{AC + CB} \times \overline{AC - CB} = AB^2$) the same will also be truly exhibited by $\frac{AG^2 \backsim AB^2}{2AG}$. *Corol.* 7.

Lastly, let the height, or depression, of the object be given, together with it's distance AB, to determine the direction, and the least *impetus* possible, to hit the object: then $AB : BC$: : radius : tang. BAC; whence the elevation BAH is known; and as radius : tang. AHG (BAH) : : MG (AB) : AG; whence the *impetus* is also known. *Corol.* 8.

VI. 1. The use of rockets is, or may be, so considerable in determining the position of distant places to each other, and in giving signals for naval or military purposes, that I thought it worth while to examine what height they usally rise to, the better to determine the extent of the country, thro' which they can be seen. I therefore, at the exhibition of the late fire-works, desir'd a friend of mine, who I knew intended to be only a distant spectator, to observe the angle of elevation to which the greatest part of them rose, and likewise the angle made by the rocket or rockets, which should rise the highest of all.

Observations on the height to which rockets ascend; by Mr Benj. Robins, *F. R. S.* N°. 492. p. 131. Apr. &c. 1749. *Read* May 4. 1749.

My friend was provided with an instrument, whose *radius* was 38 inches; and, to avoid all uncertainty in it's motion, it was fixed in an invariable position; and it's field, which took in ten degrees of altitude was divided by horizontal threads. The station my friend chose was on the top of Dr *Nisbett*'s house in *Kingstreet* near *Cheapside*, where he had a fair view of the upper part of the building erected in the *Green Park*. There he observed that the single rockets which rose the most erect, were usually elevated at their greatest height about $6° \frac{1}{4}$ above his level; and that amongst these there were 3 which rose to $7° \frac{1}{2}$; and that in the last great flight of rockets, said to be of 6000, the crest of the arch, formed by their general figure, was elevated about $8° \frac{3}{4}$. From the care and dexterity of my friend, and the nature of the instrument, I doubt not but these observations are true within a few minutes.

The distance of this station from the building in the *Green Park* is 4000 yards, according to the last great map of *London:* and hence it appears, that the customary height, to which the single, or honorary rockets, as they are styled, ascended, was near 440 yards: that three of these rose 526 yards; and that the greatest height of any of those fired in the grand girandole was about 615 yards: all reckon'd above the level of the place of observation, which I esteem to be near 25 yards higher than the *Green Park*, and little less than 15 yards below the chests whence the great flight of rockets was discharged.

 It

It ſeems then there are rockets which riſe 600 yards from the place whence they are diſcharged: and this being more than a third part of a mile, it follows, that if their light be ſufficiently ſtrong, and the air be not hazy, they may be ſeen in a level country at above 50 miles diſtance.

The obſervations on the ſingle rockets are ſufficiently conſonant to ſome experiments I made myſelf about a fortnight ſince: for then I found that ſeveral ſingle pound rockets went to various heights between 450 and 500 yards, the altitude of the higheſt being extremly near this laſt number, and the time of their aſcent uſually ſhort of 7″.

But though from all theſe trials it ſhould ſeem as if good rockets of all ſizes had their heights limited between 400 and 600 yards; yet I am diſpoſed to believe, that they may be made to reach much greater diſtances. This I in ſome degree collect from the nature of their compoſition, and the uſual imperfect manner of forming them.

Nor is this merely matter of ſpeculation; for I lately ſaw a dozen of of four pound rockets fired; the greateſt part of which took up near 14″ in their aſcent, and were totally obſcured in a cloud near 9 or 10″ of the time; ſo that the moment of their burſting was only obſervable by a ſudden glimmering through the clouds: and as theſe rockets, during the time they were viſible, were far from moving with a languid motion, I cannot but conceive, that the extraordinary time of their aſcent muſt have been attended by a very unuſual riſe.

An account of ſome Experiments, made by Benj. Robins, *Eſq; F. R. S. Mr* Samuel Da Coſta, *and ſeveral other Gentlemen, in order to diſcover the height to which* Rockets *may be made to aſcend, and to what diſtance their* Light *may be ſeen; by Mr* John Ellicott, *F. R. S.* N°. 496. p. 578. Nov. &c. 1750. *Read* Dec. 13. 1750.

2. Mr *Robins* not having been able to obtain any certain account to what diſtance any of the rockets mentioned in the preceding paper were actually ſeen, reſolved to order ſome rockets to be fired at an appointed time, and to deſire ſome of his friends to look out for them at ſeveral very diſtant places.

The places fix'd upon for this purpoſe, were *Godmarſham* in *Kent*, about 50 Miles diſtant from *London*; *Beacon-Hill* on *Tiptery-Heath* in *Eſſex*, at about 40 miles; and *Barkway*, on the borders of *Hertfordſhire*, about 38 miles from *London*.

Mr *Robins* accordingly order'd ſome rockets to be made by a perſon many years employ'd in the Royal Laboratory at *Woolwich*; to which ſome gentlemen, who had been inform'd of his intentions, added ſome others of their own making. *Sept.* 27, 1749. at 8 in the evening, was the time appointed for the firing of them; but, thro' the negligence of the engineer, they were not let off till above ½ an hour after the time agreed upon. There were in all a dozen rockets fired from *London Field* at *Hackney*; and the heights were meaſur'd by Mr *Canton*, Mr *Robins* being preſent, at the diſtance of about 1200 yards from the poſt from whence the rockets were fir'd. The greateſt part of them did not riſe to above 400 yards; one to about 500, and one to 600 yards nearly.

By a letter I receiv'd the next day from the Rev. Dr *Maſon*, of *Trin. Coll. Cambridge*, who had undertaken to look out for them from *Barkway* on the borders of *Hertfordſhire*, I was informed, that, having waited upon

upon a hill near the town with ſome of his friends till about half an hour paſt the time appointed, without perceiving any rockets, as they were returning to the town, ſome of the company ſeeing thro' the trees what they took to be a rocket, they immediately haſten'd back out of the cloſes into the open fields, and plainly ſaw 4 riſe, turn and ſpread: he judged they roſe about 1° above the horizon, and that their lights were ſtrong enough to have been ſeen much farther.

From *Eſſex* I was inform'd, that the perſons on *Tiptery-Heath* ſaw 8 or 9 rockets very diſtinctly, at about ½ an hour paſt 8; and likewiſe greatly to the eaſtward of theſe 5 or 6 more. The gentlemen from *Godmarſham* in *Kent* having waited till above half an hour paſt 8, without being able to diſcern any rockets they fir'd half a dozen; which, from the bearings of the places were moſt probably thoſe ſeen to the eaſtward by the perſons upon *Tiptery-Heath*; and if the ſituations, as laid down in the common maps, are to be depended upon, at about 35 miles diſtance.

The engineer being of opinion that he could make ſome rockets, of the ſame ſize as the former, that ſhould riſe much higher, Mr *Robins* order'd him to make half a dozen. Theſe laſt were fired *Oct.* 12. following, from the ſame place, and in general they roſe nearly to the ſame heights with the foregoing; excepting one, which was obſerved to riſe 690 yards. The evening prov'd very hazy, which render'd it impoſſible for them to be ſeen to any conſiderable diſtance.

It being obſerv'd in theſe trials, that the largeſt of the rockets, which were about 2 inches and a half in diameter, roſe the higheſt, Mr *Robins* intended to have made ſome more experiments, in order to a farther diſcovery what ſiz'd rockets would riſe higheſt: but his engagements with the *Eaſt-India* company preventing him, Mr *Samuel Da Coſta*, late of *Devonſhire-Square*, a gentleman of an extraordinary genius in Mechanicks, and indefatigable in the application; Mr *Banks*, a gentleman who had for many years practis'd making rockets, and two other perſons, undertook the proſecuting theſe enquiries; and having made ſeveral experiments as well with regard to the compoſition, as the length which rockets might be made to bear, in proportion to their diameters, and of different-ſiz'd rockets from 1 ½ to 4 inches diameter, they intended this winter to have made trial of ſome of a yet greater diameter, had not the death of Mr *Da Coſta* prevented it.

I ſhall therefore beg leave to give ſome account of the ſucceſs which has hitherto attended their undertaking, ſo far as they went: and as it has been much beyond what was expected, I am in hopes this ſhort relation will not prove unacceptable.

Amongſt ſome rockets fired in the laſt ſpring, there were two made by Mr *Da Coſta* of about 3 ½ inches diameter, which were obſerved to riſe, the one to about 833, the other 915 yards. At a ſecond trial, made ſome time after, there was one made by Mr *Da Coſta*, of 4 inches diameter, which roſe to 1190 yards. The laſt trial was made the latter end of *April* 1750, where 28 rockets were fir'd in all, made by different perſons, and of different

ferent ſizes, from 1 $\frac{1}{2}$ to 4 inches diameter; the moſt remarkable of each ſize were as follows; one of 1 $\frac{1}{2}$ inch roſe to 743 yards; one of 2 to 659; one of 2 $\frac{1}{2}$ to 880; another of the ſame ſize, which roſe to 1071; one of 3 to 1254; one of 3 $\frac{1}{2}$ to 1109; and one of 4 inches; which, after having riſen to near 700 yards, turned, and fell very near the ground before it went out. Theſe were all made by Mr *Da Coſta*. Beſides theſe, there was one of the rockets of 2 $\frac{1}{2}$ inches in diameter, which roſe to 784 yards, and another made by Mr *Banks* of the ſame ſize to 833.

As the making of large rockets is not only very expenſive, but likewiſe more uncertain than thoſe of a leſſer ſize; ſo from the laſt experiments it is evident, that rockets from 2 $\frac{1}{2}$ to 3 $\frac{1}{2}$ inches diameter, are ſufficient to anſwer all the purpoſes they are intended for; and I doubt not may be made to riſe to an height, and to afford a light capable of being ſeen to conſiderably greater diſtances than thoſe before-mention'd.

Before I conclude this account, it may not be improper to take notice, that, tho' the heights of the rockets are ſet down to a ſingle yard, it is not pretended the method made uſe of (tho' ſufficient for all the purpoſes of theſe experiments) is capable of determining the heights to ſo great an exactneſs: for as they were meaſur'd by only one obſerver, it is evident that, if any of the rockets deviated from the perpendicular, ſo as either to incline towards the place of obſervation, or to decline from it, the height would be given either greater or leſs than the truth; but as the the baſe upon which they were meaſur'd was 1190 yards, the greateſt error that can ariſe on this account will be but very inconſiderable. If we ſhould ſuppoſe there might be an error of 30 or even 50 yards, which is very highly improbable, it muſt then be allowed, that ſeveral of theſe rockets roſe to 1000 yards, one to 1100, and another to 1200 yards, or double to any of thoſe fired in the *Green Park*.

I have been informed that the relation of this affair has appeared ſo very extrodinary to ſome gentlemen converſant in ſuch matters, that they have mention'd it as their opinion, that there muſt certainly have been ſome miſtake, either in placing the inſtrument, taking the heights, or otherwiſe. In anſwer to which I would obſerve, that, in all the experiments mentioned in this paper, the heights were all taken by the ſame perſon, *viz.* Mr *John Canton*, and that the laſt trial was made in the preſence of ſeveral very worthy members of this *Society*. That the inſtrument, being firſt fixed to a proper angle was not alter'd during the whole time of trial; and therefore, if there had been any miſtake in fixing it, that miſtake would have varied the height of all the rockets as much as thoſe of Mr *Da Coſta*'s; but it was thoſe of Mr *Da Coſta*'s only, and that at 3 different trials, which roſe to ſuch extraordinary heights; and therefore I think we have ſufficient reaſon to conclude that their meaſures were certainly taken very near the truth.

Fig. 52. *Deſcription of a Machine to*

VII. *ABCD* is a pit dug in the ground, whoſe ſurface is higher at *D* than on the other ſide at *A*. The bottom *BC* is ſtrongly ramm'd with clay, upon which are laid thin ſawen deals.

In

In this pit is fixed a tub *GHKI* without a bottom, having a hole *I* at the lower part of the ſide, and all round the tub is ramm'd with clay, except at the hole *I*. *blow fire by the fall of water; by* James Stirling, *F. R. S.* N°. 475. p. 315. Jan. *&c.* 1745. *Read* March 21. 1744-5.

In the middle of the upper end of the tub is fixed a pipe *PQRS*; at the higher end of which are four holes pointing downwards, whereof two are repreſented by *S* and *R*,

SRTU is a funnel fixed on the top of the pipe, with a throat *XZ* narrower than the bore of the pipe. In the upper end of the tub towards one ſide is fixed a crooked pipe at *L M*, tapering to the end at *N*. It is made of wood ſo far as *O*, but from *O* to *N* of iron, the fire being ſuppoſed at *N*. *EF* is the ſurface of a plain ſtone, raiſed up in the middle of the tub, directly under the pipe *PQRS*.

The running water, being let in at the top of the funnel, falls thro' the pipe upon *EF* the ſtone in the tub; it runs out at the hole *I*, but cannot get off till it riſes as high as *A*.

This raiſes it in the tub almoſt up to the ſurface of the ſtone, and it muſt not riſe higher.

So much water muſt run in at the top of the funnel, as will keep it always full, or nearly ſo.

This height of water ſqueezes it into the pipe with a great velocity; but, ſince it paſſes thro' the throat of the funnel, which is of a ſmaller bore than the pipe, room is left all round the vein of water for the air to enter at the air-holes.

It no ſooner enters but it mixes with the water, on the account of the rapidity of the motion; and both together make a white froth, and intirely fill the bore of the pipe. When this froth falls on the ſtone in the tub, it is daſhed into ſmall particles, which diſengages the air from the water. The air cannot get out at *PQ*, the end of the pipe, becauſe it is fill'd with the froth, which falls with a great force; neither can it get out at the hole *I*, becauſe the ſurface of the water is kept ſo high above it; and for that reaſon it ruſhes out at *N*; and if the hole *N* be ſtopped, the air will ſoon force all the water in the tub out at *I*, and then follow it.

The moſt convenient way of regulating the blaſt, is to bore a ſmall hole in the Blaſt-pipe; and, by the help of a pin in it, to let out what air there may be more than is wanted.

The dimenſions of ſuch an engine ſufficiently big to ſmelt harder ore than any in lead-hills, are ſet down at the Bottom.

	Feet.
Height of the funnel - - - - - - - - - - - -	5
Length of the pipe - - - - - - - - - - - - -	14, 15, or 16
Height of the tub - - - - - - - - - - - - -	6
Diameter of the tub - - - - - - - - - - - -	$5\frac{1}{2}$
Height of the ſtone in the tub - - - - - - - -	2

Diameter

	Inches.
Diameter of the throat of the funnel - - - - - - - - - -	$3\frac{1}{2}$
Diameter of the bore of the pipe - - - - - - - - - - - -	$5\frac{1}{2}$
Diameter of the blaſt-hole at *N* - - - - - - - - - - - - -	$1\frac{1}{2}$
Hole at *I* about 5 inches ſquare,	
Diameter of the air-holes - - - - - - - - - - - - - -	$1\frac{1}{4}$

This engine is likewiſe of admirable uſe to convey freſh air into the works; which ſaves the double drifts and ſhafts, and cutting communications between them.

A ſmall one will do very well for a Black-ſmith.

Tables of Specific Gravities, *extracted from various authors, with ſome obſervations upon the ſame; in a letter to* M. Folkes, *Eſq;* P. R. S. *by* Rich. Davis, *M. D.* N°. 488. p. 416. June 1748. *Preſented* Feb. 18. 1747.

VIII. The manifold applications which may be made, for the purpoſes of Natural Philoſophy, of the relations which bodies bear to each other, by their reſpective ſpecific gravities, engaged me ſome years ſince to collect all the experiments of this ſort I could meet with in the courſe of my ſtudies, and alſo to make ſeveral new ones of my own with the ſame deſign.

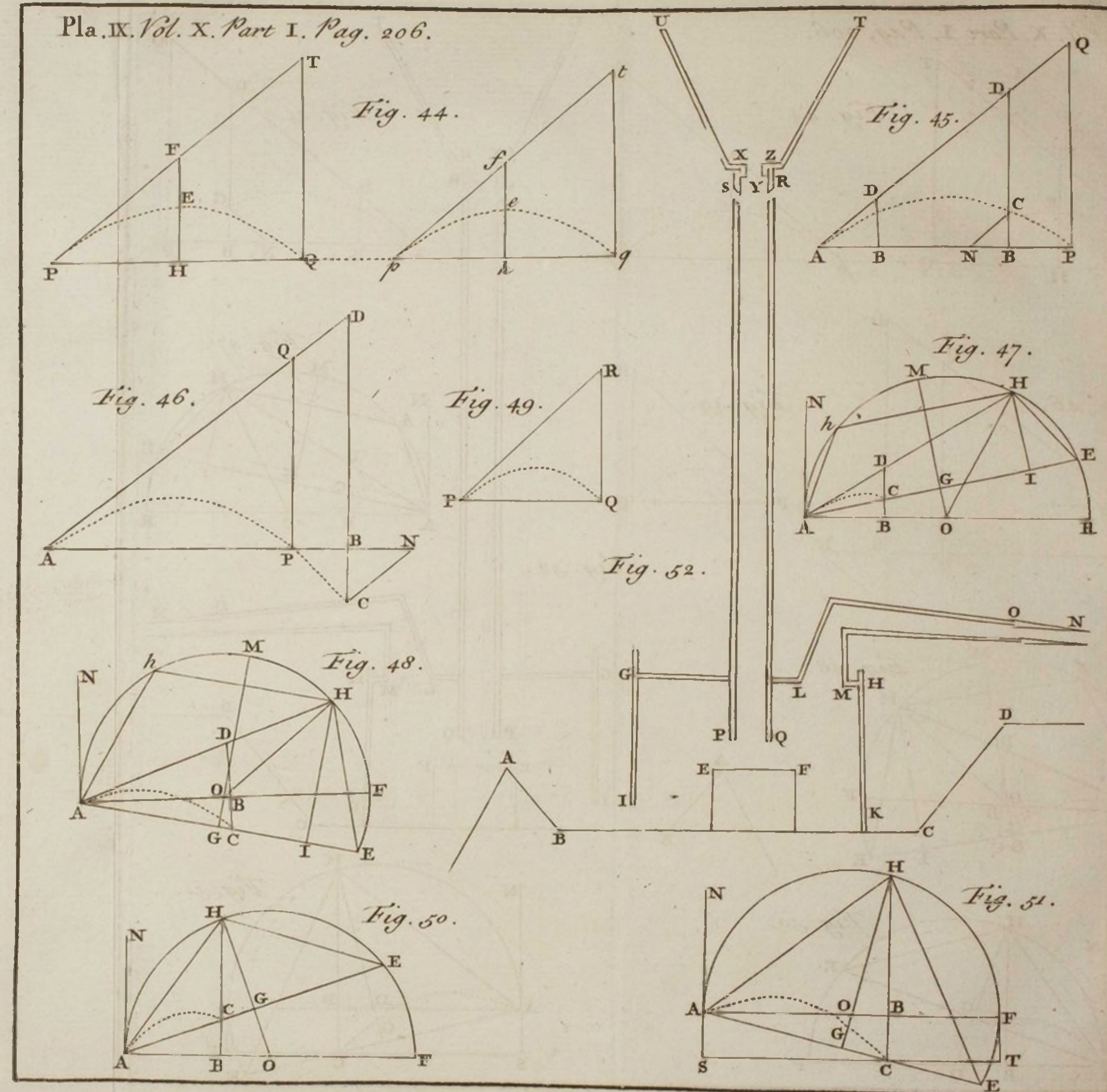
When my collection began to be ſomewhat conſiderable, I diſpoſed the ſeveral bodies in tables according to their ſpecies, which I found to be the moſt convenient method, as my tables were by this means capable of receiving additions in any part, without deſtroying the form of the whole: and as they were thereby eaſy and ready to be conſulted, and well diſpoſed for the forming of immediate compariſons between the ſeveral bodies of the ſame ſpecies.

But having now no farther opportunities of enlarging my collection, I hereby beg leave to recommend the proſecution of my deſign to others, as a ſubject well deſerving the attention of ſome of the members of the *Royal Society*, to whom I therefore preſent theſe my tables: wiſhing they may prove of ſome uſe and ſervice to the inquiſitive and philoſophical part of the world. As I perſuade myſelf they really will, when they ſhall be further rectified by the omiſſion of the erroneous or uncertain experiments; when they ſhall be enlarged by the addition of ſuch others, as may ſtill be found in good authors, or which yet remain unpubliſhed in the cloſets of the Curious: and eſpecially if ſome ſuch gentlemen as have ſkill, leiſure, and opportunities, ſhall pleaſe to ſupply their remaining defects, by the communication of their own obſervations, made upon thoſe bodies, whoſe ſpecific gravities have not as yet been carefully recorded.

Denique cur alias aliis præſtare videmus
Pondere res rebus, nihilo majore figura?
Nam, ſi tantundem eſt in lanæ glomere, quantum
Corporis in plumbo'ſt tantundem pendere par eſt. Lucret.

The

Pla. IX. Vol. X. Part I. Pag. 206.



The Antients have left but few particulars concerning the different specific gravities of bodies, tho' it is plain they were in the general sufficiently acquainted with them. It was by the knowledge of the various weights of gold and silver, that *Archimedes* is recorded to have detected the famous fraud committed in *Hiero*'s crown, as *Vitruvius* has at large related in his *Architecture*, l. ix. c. 13. and it is from the same great philosopher, that we have derived the demonstration of those hydrostatical rules, by which the proportions are best to be known, of the several weights or densities of different bodies, having the same bulk or magnitude: as may be seen in his tract *De insidentibus humido*, lost in the *Greek* original, but retrieved in great measure, as it is said, from an *Arabic* translation. It was published in *Latin*, with a Commentary by *Federicus Commandinus* at *Bononia* 1565, 4to, and the substance of it by Dr *Barrow* in his *Archimedes*, printed likewise in 4to at *London* 1675.

A short account of the Authors, from whose writings and experiments, the following tables have been collected, with some remarks upon the experiments themselves, and the manner in which they appear to have been made.

Pliny, in the xviii. book of his *Natural History*, has set down the proportional weights of some sorts of grain, among which he says that barley is the lightest. *Levissimum ex his hordeum, raro excedit,* [*in singulos nimirum modios*] xv *libras, et faba* xxii. *Ponderosius far magisque etiamnum triticum.* And a little further on, *ex his generibus* [*frumenti scilicet*] *quæ Romam invehuntur, levissimum est Gallicum, atque è Chersoneso advectum: quippe non excedunt in modium vicenas libras, si quis granum ipsum ponderet. Adjicit Sardum selibras, Alexandrinum et trientes: hoc et Siculi pondus. Bæoticum totam libram addit: Africum et dodrantes. In Transpadanâ Italiâ scio vicenas quinas libras farris modios pendere: circa Clusium et senas.* And the same author in his xxxiii. book, speaking of quicksilver, observes that it is the heaviest of all substances, gold only excepted. *Omnia ei innatant, præter aurum: id unum ad se trahit.* Which *Vitruvius* had also taken notice of, and had mentioned besides the weight of a known measure of it, that of four *Roman sextarii*. *Eæ autem* [*guttæ nempe argenti vivi quæ inter se congruunt et una confunduntur*] *cum sint quatuor sextariorum mensuræ, cum expenduntur, inveniuntur esse pondo centum. Cum in aliquo vase est confusum, si supra id lapidis centenarii pondus imponitur, natat in summo: neque eum liquorem potest onere suo premere, nec elidere, nec dissipare: centenario sublato, si ibi auri scrupulum imponatur, non natabit, sed ad imum per se deprimetur. Ita non amplitudine ponderis, sed genere singularum rerum gravitatem esse, non est negandum.* Archit. l. vii. c. 8.

Again, *Q. Rhemnius Fannius Palæmon*, in his fragment *De ponderibus et mensuris*, has given us an observation, of the proportional gravities of water, oil, and honey.

——*Libræ, ut memorant, bessem sextarius addet,*
Seu puros pendas latices, seu dona Lyæi,
Addunt semissem libræ labentis olivi,
Selibramque ferunt mellis superesse bilibri.

That

That is to ſay, that the *ſextarius* of either water or wine weighed 20 ounces, the ſame meaſure of oil 18, and of honey 30. Their ſpecific weights were therefore in proportion as 1.0, 0.9 and 1.5, exactly agreeable to what *Villalpandus* determined about the beginning of the laſt century: yet was this author himſelf ſenſible, that theſe were not to be look'd upon as very nice experiments.

Hæc tamen aſſenſu facili ſunt credita nobis.
Namque nec errantes undis labentibus amnes,
Nec merſi puteis latices, aut fonte perenni
Manantes, par pondus habent: non denique vina,
Quæ campi aut colles nuperve aut ante tulere,
Quod tibi mechanica promptum eſt depromere Muſa.

After which he proceeds to deſcribe a good pretty inſtrument for the ready finding of the different ſpecific gravities of fluids, and ſhews how thoſe of ſolids alſo may be hydroſtatically diſcovered. And ſo much ſhall ſuffice for what I had to mention from the Antients relating to this ſubject: I now come to thoſe who have written within theſe laſt hundred and fifty years.

Francis Bacon, *Lord Verulam*, &c. in his *Hiſt. denſi et rari*, printed in vol. ii of his works in *folio*, *Lond.* 1741. *p.* 69. has given a table, which he calls, *Tabula coitionis et expanſionis materiæ per ſpatia in tangibilibus (quæ ſcilicet dotantur pondere) cum ſupputatione rationum in corporibus deverſis.* This tract does not appear to have been publiſhed till after his death, which happened in the year 1626, but was probably written ſeveral years before; and the experiments were even as he tells us made long before that. *Hanc tabulam multis abhinc annis confeci, atque ut memini, bona uſus diligentia.* I therefore apprehend it to be the oldeſt table of ſpecific gravities now extant. The experiments therein mentioned were not made hydroſtatically, but with a cube of an ounce weight of pure gold, as he ſays, to which he cauſed cubes of other materials to be made equal in ſize: as he did alſo two hollow ones of ſilver, and of equal weights, the one to be weighed empty, and the other filled with ſuch liquid as he wanted to examine. He was himſelf ſenſible that his experiments of this ſort were, notwithſtanding his care, very defective, *poſſit proculdubio tabula multo exactior componi, videlicet tum ex pluribus, tum ex ampliore menſura: id quod ad exactas rationes plurimum facit, et omnino paranda eſt, cum res ſit ex fundamentalibus.* From among theſe, notwithſtanding their imperfection, as they appear to have been ſome of the firſt experiments of the ſort regularly digeſted, and as they were beſides made by ſo great a man, I have extracted the ſpecific gravities of the fixed metals, which I have inſerted as examples in the following tables: after reducing them to the common form, upon the ſuppoſition that pure gold was, according to *Ghetaldus*, juſt 19 times as heavy as water. And this I have rather choſen to do, than to make uſe of his Lordſhip's own weight of of water given in the table, which in the manner he took it could not be very exact, and which besides would not have brought out the specific gravity of pure gold more than 18 times as much; and that of the other metals in proportion. This table contains in all 78 articles.

There are also in the third volume of the same edition of his works, *p.* 223. *Certain experiments made by the Lord Bacon about weight in air and water*: These are truly *hydrostatical* but very imperfect, I have not therefore inserted any of them in the following collection.

Marinus Ghetaldus, a nobleman of *Ragusa*, published in *quarto* at *Rome*, in 1603, his treatise entitled, *Promotus Archimedes, seu de variis corporum generibus gravitate et magnitudine comparatis*, wherein he has given a comparison between the specific gravities of water and eleven other different substances, from his own hydrostatical experiments made with care and exactness. These I have inserted: expressing the numbers as they stand in his own book, but I have afterwards also for uniformity reduced them to the decimal form. I have besides at the end transcribed at large the two tables of this author, in which every one of the 12 sorts of bodies he treats about is successively compared with all the others, both in weight and magnitude.

F. *Joh. Baptista Villalpandus*, a Jesuit of *Cordoua* in *Spain*, in his *Apparatus Urbis et Templi Hierosolymitani*, printed in *folio* at *Rome* in 1604, exhibited a table of the proportional weights of the 7 metals and some other substances, from his own experiments, made with great care as he tells us, by the means of 6 equal solid cubes of the fixed metals, and a hollow cubical vessel 8 times as large, for the comparing mercury, honey, water, and oil with the same. His numbers, which are inserted under his name in the following tables, were also again published afterwards by *Joh. Henr. Alstedius* in his *Encyclopædia universa*, printed in 2 vols. in *folio*, at *Herborn* 1630, and by *Hen. Van Etten*, in his *Mathematical Recreations*, from whence they have been often transcribed into other books. *Villalpandus*'s book, which is only the third volume of a work begun to be published several years before, was itself printed so soon after *Ghetaldus*'s, that it is probable he either never saw that author, or not at least till after his own experiments were made.

Mr *Edm. Gunter*, in his *Description and Use of the Sector*, printed after his death by Mr. *Sam. Foster*, in 1626, having occasion to make mention of the specific weights of the several fixed metals, quoted *Ghetaldus*, and made use of his proportions, and so did also Mr *Will. Oughtred*, in his *Circles of Proportion*, first published in 4^to^ 1633, with this only difference, as to the form, that he changed *Ghetaldus*'s unit into 210, whereby he expressed all his relations in whole numbers. It is likewise probable that *D. Henrion* took from the same place the numbers he applied in his *Usage du Compas de Proportion*, printed at *Paris* in 1631, 8^vo^. although he has not given them all with exactness, for the sake as it seems of using simpler vulgar fractions.

F. *Marinus Merſennus*, a *French* Minim, in his *Cogitata Phyſico-Mathematica*, printed at *Paris* in 1644, 4to, has given from the obſervations of his accurate friend *Petrus Petitus*, a table of the ſpecific gravities of the metals and ſome other bodies, making gold 100, water $5\frac{1}{3}$, and the reſt in proportion. Theſe I have reduced to the common form, and inſerted under his name in the following tables. The ſame were afterwards made uſe of by F. *Francis Milliet de Chales*, Jeſuit, in his *Curſus Mathematicus*, Monſieur *Ozanam*, Profeſſor *Wolfius*, and ſeveral others. I have not ſeen *Petitus*'s own book, but it was entitled *L'Uſage ou le moyen de pratiquer par une Regle toutes les Operations du Compas de Proportion —— augmentées des Tables de la Peſanteur et Grandeur des Metaux*, &c. had a privilege dated in 1625. tho' it is ſaid not to have been printed till ſome years after. The ſame Father *Merſennus* has alſo taken notice, in his general preface, of a table of 20 ſpecific gravities, ſome time before publiſhed by M. *Alcaume*, which he there ſets down, but which he alſo obſeves to be very incorrect. I have not therefore inſerted any of them in this collection.

Mr *Smethwick*, one of the earlieſt members of the *Royal Society*, communicated to the ſame in *July* 1670, the weights of a cubic inch of ſeveral different ſubſtances; ſaid to have been formerly taken by Mr *Reynolds* in the *Tower* of *London*. This gentleman was the ſame who compoſed ſeveral tables relating to the price of gold and ſilver, which were publiſhed in a book entitled *The Secrets of the Goldſmith's Art*, at *London* 1676 *in* 8vo. Theſe weights are expreſſed in decimals of an *Averdupois* pound, are carried to 8 places of figures, and ſeem to have been carefully and accurately collected. I have therefore in the following tables reduced them to the common form, in order to give them their proper authority with the reſt. I am ignorant whether theſe weights were ever before printed or not, neither can I give any account, after what particular manner the experiments were made, from which they were taken. They were communicated to me from the regiſter-books of the *Royal Society*; and I ſhall only obſerve, that the abſolute weight here aſſigned of a cubic inch of common water, does not differ more than a ſmall fraction of a grain, from the weight of the ſame afterwards determined by Mr *Ward* of *Cheſter*.

The *Philoſophical Society*, meeting at *Oxford*, directed ſeveral experiments to be made hydroſtatically by their members, concerning the ſpecific gravities of various bodies; which being digeſted into a table, were by Dr *Muſgrave* communicated to the *Royal Society*, *March* 21, 1684. ſoon after which they were printed in the 169th number of the *Philoſ. Tranſ.* Theſe experiments were, according to Dr *Muſgrave*, made by Mr *Caſwell* and Mr *Walker*; they are all originals, and eſteemed ſome of the moſt accurate that are extant.

The honourable *Robert Boyle*, at the end of his *Medicina hydroſtatica*, firſt publiſhed at *London* in 1690, 8vo. ſubjoined a table of the ſpecific gravities of ſeveral bodies, accurately taken from his own hydroſtatical experiments.

experiments. Befides which, there are alfo in the fame tract, and in other parts of his works, feveral experiments of this excellent author's, which he has given occafionally, together with the ufes refulting from them. To fuch of thefe in the following collection, as were taken from the table juft mentioned, I have barely annexed his name, but to fuch of the others as occurred, I have alfo added the volume, page, and column, of the late *folio* edition of his works in 1744, where the fame are to be found. It may be noted, that in the firft edition of the *Medicina hydroftatica*, there were feveral errors of the prefs. Such of them as I could difcover by calculation, I have corrected in the following pages.

There is a table publifhed under the name of *J. C.* in the 199th number of the *Philof. Tranf.* An. 1693: and this is evidently a fupplement to that abovementioned of the *Philofophical Society* meeting at *Oxford*. The experiments were, according to the initials *J. C.* made by the fame curious perfon Mr *John Cafwell*, and are therefore of the fame eftimation as the others.

M. *Homberg*, of the *R. Acad. of Sc.* at *Paris*, read a memoir in 1699, wherein he took notice of the expanfion of all fubftances by heat, and the contraction of the fame by cold: from whence it muft follow, that the fpecific gravities of the fame bodies would conftantly be found lefs in the fummer and greater in the winter. And this he fhew'd from the experiments he had made upon feveral fluids, both in the fummer and the winter-feafons, by means of an inftrument he had contrived and called an *Aræometer*, being a large phial, to which he had adjufted a long and flender ftem, whereby he could to good exactnefs determine, when it was filled with equal bulks or quantities of the feveral fluids he propofed to examine. The refult of his trials with this inftrument he digefted into a fhort table, which was printed in the Memoirs of the *Academy* for the fame year 1699. This table *J. Cafpar Eifenfchmid* afterwards republifhed with feveral additions, in his tract *De Ponderibus et Menfuris*, printed at *Strafburg* in 1708, 8vo. changing it to a more convenient form for his purpofe, by reducing the different fluids therein named to the known bulk of a cubical *Paris* inch. So much of this table as I thought might be of fervice, I have here fubjoined to the others in the following collection, but I have alfo made an alteration in the form, the better to fit it for general ufe, by omitting the abfolute weights of the feveral bodies in fummer and winter, and placing inftead of them, after the name of each body a decimal number, expreffing the proportion of its weight in winter to its weight in fummer, fuppofed to be every-where reprefented by unity.

Sir *If. Newton*, in his *Optics*, printed in 4to. at *London* 1704, gave a table of the fpecific gravities of feveral *diaphanous* bodies. The experiments were made by him with a view chiefly to optical enquiries, and to enable him to compare their denfities with their feveral refractive powers: we may therefore be well affured, that they were made by the great author with the moſt ſcrupulous care and exctneſs. The table conſiſts of 22 articles.

John Harris, D. D. in his *Lexicon Technicum*, firſt printed at *London* 1704, *fol.* republiſhed at large the ſeveral tables of ſpecific gravities of the *Oxford Society* and *J. C.* from the *Philoſ. Tranſ.* and that of the Hon. *Rob. Boyle* from his *Medicina hydroſtatica*, to which laſt he alſo added ſome experiments of his own, made as it ſeems with good accuracy. Theſe are here extracted, and placed under his name in the following tables.

Mr *John Ward* of *Cheſter*, in his *Young Mathematician's Guide*, firſt printed, as I take it, in 1706, acquaints us, that he had himſelf for his own ſatisfaction, made ſeveral experiments upon the different ſpecific gravities of various bodies; and that he was of opinion, that he had obtained the proportion of the weight that one body bears to another of the ſame bulk and magnitude, as nicely as the nature of ſuch matter, as might be contracted or brought into a leſſer body (*viz.* either by drying, hammering, or otherwiſe) would admit of. And he has accordingly given us in the ſaid book the weight of a cubic inch of 24 different ſubſtances, both in *Troy* and *Averdupois* ounces and decimal parts of an ounce; which he further aſſures us required more charge, care, and trouble, to find out nicely, than he was at firſt aware of. This table appears to have been well eſteemed, and to have had the ſanction of Mr *Cotes*'s approbation, by his taking it, when reduced to the common form, into that collection which he drew up for his own hydroſtatical lectures.

Roger Cotes, M. A. and *Plumian Prof. Aſtron. and Exp. Philoſ.* at *Cambridge*, firſt giving about the year 1707 a *Courſe of Hydroſtatical and Pneumatical Experiments*, in conjunction with Mr *Whiſton* in that Univerſity, drew up, for the uſe of that courſe, a very accurate table of ſpecific gravities, collecting from ſeveral places ſuch experiments as he took to be moſt exact, and the beſt to be depended upon. And as the judgment of ſo great a man cannot but give a general reputation to ſuch experiments as he had ſo ſelected, I have thought proper, in the following tables, to diſtinguiſh all ſuch by the addition of the letter *C*, after the names of ſuch perſons from whom they firſt appear to have been taken, adding alſo the name of *Cotes* at length, to ſuch others as I have not met with elſewhere, and which I therefore take to have been tranſcribed from the *memoranda* of his own experiments. This table of Mr *Cotes*'s uſed firſt to be given in *M. S.* to thoſe who attended his lectures; but it was afterwards printed in a ſingle ſheet, relating to a *Courſe of Experiments* at *Cambridge* in 1720, and ſince in Mr *Cotes's Hydroſtatical and Pneumatical Lectures*, when they were publiſhed at large in 8vo by his ſucceſſor Dr *Smith*, now the worthy maſter of *Trinity College*. In theſe printed lectures were inſerted the gravities of human blood, its *ſerum*, &c. from Dr *Jurin*, inſtead of thoſe that had before been made uſe of from Mr *Boyle*.

Mr *Francis Haukſbee*, now Clerk to the *Royal Society*, did, about the the year 1710, begin, in conjunction with Mr *Whiſton*, who had then newly left the Univerſiy, to give hydroſtatical lectures, *&c.* in *London*; for

for the purpofe of which he reprinted in a thin volume in 4^{to}, in which are the fchemes of his experiments, Mr *Cotes*'s table of fpecific gravities abovementioned. To which he added, from tryals of his own, the weights of fteel, foft, hard, and temper'd, which are printed with his name in the following tables, as are alfo fome other experiments, which he has fince occafionally made, and communicated to me. Mr *Cotes*'s table, with the abovemention'd additions of Mr *Haukfbee*, was afterwards again publifhed by Dr *Shaw*, in his *Abridg.* of Mr *Boyle*'s *Philof. Works*, at *Lond.* 1725, 4^{to}. vol. ii. p. 345.

John Freind, M. D. at the end of his *Prælect. Chem.* printed at *Lond.* in 1709, 8^{vo}. has publifhed fome new tables of the fpecific gravities both of folid and fluid bodies, entirely taken from his own original experiments. And as thefe tables contain an account of a very ufeful fet of bodies, upon which few or no other experiments have been made: it is great pity that this truly learned and elegant writer was not more accurate in his tryals than he appears to have been. Many of his experiments having indeed been made in fo lax and improper a manner, and fo many errors having been committed in them, that one cannot with fecurity depend upon thefe tables, tho' containing otherwife facts one would fo much defire to be truly informed about. I have however here inferted the feveral particulars of his two laft tables, which immediately concern fpecific gravities, after correcting fuch errors in calculation as I could certainly come at: and I hope that I fhall be excufed for this free cenfure upon part of the works of a gentleman, who has fo well deferved of the learned world, and acquired fo juft a reputation in it.

James Jurin, M. D. and feveral years Secretary of the *Royal Society*, gave, in N°. 361 of the *Philof. Tranf.* An. 1719, fome original and very accurate experiments made by himfelf, upon the fpecific gravity of human blood, at feveral times during the fix preceding years. Thefe were accompanied with a very curious Difcourfe, which has fince been tranflated by himfelf, into *Latin*, and reprinted in his *Differt. Phyfico Math. Lond.* 1732, 8^{vo}.

This gentleman has alfo, in N°. 369 of the fame *Tranf.* obliged us with fome very judicious and ufeful remarks, relating to the *caution to be ufed in examining the fpecific gravity of folids, by weighing them in water*; for want of attending to which, feveral forts of bodies, fuch as human *calculi*, the fubftance of all woods, *&c.* have appeared, from their pores and fmall cavities filled up with air, to be confiderably lighter than they really are.

John Woodward, M. D. and Profeffor of Phyfic in *Grefham College*, had, as he acquaints us in feveral places of his works, made a great number of experiments upon the fpecific weights, of mineral and other foffil bodies, but which being probably contained in thofe of his papers which he ordered to be fuppreffed at his death, are thereby loft to the world, to which they would without all doubt have been very acceptable. All I have been able to pick up are a very few mentioned in the

Catalogue

Catalogue of the English Foſſils in his Collection, publiſhed ſince his deceaſe, in 8vo. *Lond.* 1729.

Mr *Gabriel Fahrenheit*, F. R. S. communicated, in N°. 383. of the *Philoſ. Tranſ. a table of the ſpecific gravities of* 28 *ſeveral ſubſtances*, from hydroſtatical experiments of his own, made with great care and exactneſs; to which he ſubjoined ſome obſervations upon the manner in which his trials were performed, together with a deſcription of the inſtruments in particular which he made uſe of to examine the gravities of fluids. To ſome of his experiments which he thought required a greater nicety, he has affixed an aſteriſk in his table, ſignifying ſuch to have been adjuſted to the temperature of the air, when his Thermometers ſtood at the height of 48 degrees. This gentleman, who is well known by the reputation of his Mercurial Thermometers, which he made with great curioſity, and which are now generally uſed, was in *England* in the year 1724.

Profeſſor *Peter van Muſſchenbroek*, of *Utrecht*, publiſhed in his *Elementa Phyſicæ* at *Leyden* in 8vo. 1734. a large table of ſpecific gravities, which he afterwards yet ſomewhat further enlarged in his *Eſſai de Phyſique* in *French*, at *Leyden* 1739. 4to. This table contains almoſt all the preceding ones, but without the names of the authors from whom they were collected. I have among thoſe which follow inſerted, under this author's name, ſuch experiments as I had not before met with elſewhere: making uſe of the *Latin* edition as the more correct, except in ſuch articles which are only to be found in the *French*.

Mr *John Ellicott*, F. R. S. having an opportunity in the year 1745, to examine the weight of ſome large diamonds, he accordingly, with the utmoſt care, and with exquiſite aſſay-ſcales which very ſenſibly turned with the 200th part of a grain, took the ſpecific gravities of 14 of thoſe diamonds, 4 of which came from the *Braſils*, and the other 10 from the *Eaſt-Indies*. Theſe experiments he communicated to the Preſ. of the Royal Society, who cauſed them to be read at one of their meetings, and afterwards publiſhed them in N°. 476. of the *Philoſ. Tranſ.* Among theſe *Braſilian* diamonds, one was of the abſolute weight of 92,425, another of 88,21; and among the *Eaſt-Indian* ones, one of 29,525 *Troy* grains. And as the ſize of theſe ſtones made them much fitter for theſe enquiries, than any others which had probably ever before been uſed for the ſame purpoſe, ſo the known accuracy of the author, the goodneſs of his inſtruments, and the conſiſtency of all his experiments, ſufficiently ſhew the ſpecific gravities he has delivered in his paper, may entirely be depended upon.

The ſame curious perſon alſo communicated the ſpecific gravities of fine and ſtandard gold, publiſhed under his name in the following tables, and which were deduced from experiments he was ſo kind as to make on purpoſe at my requeſt.

As I have juſt had occaſion to mention diamonds, it may poſſibly not be foreign to the purpoſe here to take ſome notice of the diamond carat

weight,

weight, uſed among Jewellers, which weight was originally the carat or 144th part of the *Venetian* ounce, equal to 3,2 *Troy* grains, but which is now, for want of an acknowledged ſtandard, ſomewhat degenerated from its firſt weight. I have myſelf found it, upon a medium of ſeveral experiments, equal to 3,17 *Troy* grains; and I have the rather taken notice of this weight here, becauſe there happens to be a miſtake about it, both in Dr *Arbuthnot*'s and Mr *Dodſon*'s tables, who have ſet down as it ſeems the number of diamond carats in a *Troy* ounce, inſtead of the weight of the diamond carat itſelf. This carat is again divided into four of its own grains, and thoſe into halves and quarters, commonly called the eighths and ſixteenths of a carat: and thus the largeſt of the diamonds juſt abovementioned, weighed, in the Jewellers phraſe, better than 29 carats and almoſt half a grain.

Mr *James Dodſon*, in his book called *The Calculator*, printed in 8vo. *Lond.* 1747, has inſerted a uſeful table of ſpecific gravities, in which he has by the firſt initial letter of their names diſtinguiſhed the ſeveral authors he has quoted: and amongſt theſe are ſeveral new experiments marked with an *L*, which I am told were communicated from his own trials, by Mr *Charles Labelye*, engineer, and which concern particularly the weights of ſeveral ſorts of ſtone and other materials uſed in building. Theſe I have alſo diſtinguiſhed by an *L.* as they ſtand in Mr *Dodſon*'s book.

Mr *Geo. Graham*, F. R. S. made for me, at the requeſt of a friend, ſome accurate trials upon the weight of gold and ſilver, both when reported fine, and when reduced to the *Engliſh* ſtandard: all which I have inſerted under his name in the following tables. Wherein I have beſides reported, ſome other ſingle experiments, which I occaſionally met with, from *Fred. Slare*, M. D. *John Keill* of *Oxford*, M. D. *Steph. Hales*, D. D. and *Edward Bayley* of *Havant* in *Hampſhire*, M. D.

Richard Davies, M. D. I have laſtly to this collection of experiments added ſome of my own, which I endeavoured to make with as much accuracy, as the inſtruments I was provided with would allow of. My hydroſtatical balance was one conſtructed ſeveral years ſince by Mr *Francis Haukſbee*, which I have conſtantly found to turn ſenſibly with half a grain: and the bodies upon which I made moſt of my trials, were taken from a collection of the *Materia Medica* formerly made by Signor *Vigani*, and ſtill preſerved in the library of *Queen's-College* in *Cambridge*.

☉ GOLD, fine. *Ward*, C.	19.640	Tab. I. Of
A medal eſteemed to be near fine gold. *J. C.*	19.636	Metals.
Or d'eſſai, ou de coupelle. *Muſſchenbr.*	19.238	
Fine gold hammered. *Ellicot.*	19.207	
D°. an ingot, ſo accounted, and again refined with antimony. *Ellicot.*	19.184	
D°. the ingot itſelf juſt mentioned. *Ellicot.*	19.161	
A medal of the Royal Society, reported fine gold. *Graham.*	19.158	

A gold

A gold medal of Queen *Elizabeth*. *J. C.*	19.125
D°. of Queen *Mary*. *J. C.*	19.100
Aurum. *Fahrenheit.*	19.081
Id. *Ghetaldus.* Aurum purum. *Bacon* (ex hyp.)	19.000
A gold Coin of *Alexander*'s. *J. C.*	18.893
Gold. *Reynolds.*	18.806
Aurum. *Villalpandus. Petitus.*	18.750
Standard gold (by which is underſtood gold of 22 carats, or ſuch of which our guineas are intended to be coined). *J. C. Ward. C.*	18.888
An old *Jacobus.* I ſuppoſe the ſceptered broad piece. *Harris.*	18.375
A *Mentz* gold ducat. *J. C.*	18.261
Aureus Ludovicus. *Muſſchenbr.*	18.166
A five guinea piece of K. *James* II. 1687, with an elephant. *Graham.*	17.933
A *Portugal* piece of 3*l.* 12*s.* 1731, ſuppoſed to be nearly the ſame as ſtandard. *Graham.*	17.854
Guineas, ten weighed together. *Davies.*	17.800
D°. on a mean of 7 trials upon thoſe of different reigns. *Ellicot.*	17.726
A piece of gold coin of the Commonwealth. *Harris.*	17.625
Guineas, two new ones. *Haukſbee.*	17.414
A grain of *Scotch* gold, ſuch as nature had made it. *Boyle* V. 30. *b.* $12\frac{2}{7}$	12.286
Electrum, a *Britiſh* coin. *J. C.*	12.071
☿ QUICKSILVER. Mercurius crudus. *Freind.*	14.117
Mercury, *Spaniſh.* *Boyle* V. 10. *b.* Mercure ſublimé 511 fois. *Muſſchenb.*	14.110
Quickſilver. *Oxford Soc.*	14.019
D°. *Ward. C.* revived from the ore. *Boyle.*	14.000
Fine mercury. *L.*	13.943
Quickſilver, another parcel. *Oxf. Soc.*	13.593
Mercure amalgamé avec de l'argent, affiné et ſublimé 100 fois. *Muſſchenb.*	13.580
Mercurius. *Fahrenheit.*	13.575*
Argentum vivum. *Ghetaldus.* $13\frac{4}{7}$	13.571
Mercure amalgamé avec de l'or affiné, et ſublimé 100 fois; le même meſlé avec du plomb, enſuite converti en poudre et revivifié. *Muſſch.*	13.550
Coarſe mercury. *L.*	13.512
Mercurius. *Petitus.*	13.406
Quickſilver. *Reynolds.*	13.147
♄ LEAD. *Reynolds.*	11.856
Plumbum. *Villalpand.*	11.650
Id. *Ghetaldus* $11\frac{1}{2}$.	11.500

Id.

Id. *Bacon.*	11.459
Lead. *Harris.*	11.420
Hardeſt lead. *L.*	11.356
Plumbum. *Fahrenheit.*	11.350
Lead. *Oxford Soc. Ward.*	11.345
Plumbum. *Petitus.*	11.343
Lead. *Harris.* (an ordinary piece)	11.330
D°. *Cotes.*	11.325
Plumbum Germanicum. *Muſſchenb.*	11.310
Caſt lead. *L.*	11.260
☽ SILVER, fine. *Ward. C.*	11.091
A medal of the Royal Society, reported fine ſilver. *Graham.*	10.484
Argentum. *Fahrenheit.*	10.481
Silver. *Reynolds.*	10.432
Argentum. *Villalpandus.*	10.400
Id. *Ghetaldus.* $10\frac{1}{3}$.	10.333
Id. *Bacon.*	10.331
Id. *Petitus.*	10.219
Sterling, or ſtandard ſilver (that is, ſilver 11 oz. 2d^{wt}. in the pound fine). A half crown of K. *William*'s coin. *Harris.*	10.750
D°. ſtruck into money. *L.*	10.629
D°. *J. C. Ward. C.*	10.535
D°. caſt. *L.*	10.520
A new crown-piece, 1746. LIMA under the head. *Graham.*	10.284
♀ COPPER. *Reynolds.*	9.127
Cuprum. *Villalpandus.*	9.100
Æs. *Ghetaldus.* Roſe copper. *Ward. C.* Fine copper. *L.* An old copper halfpenny, *Charles* II's coin. *Harris.*	9.000
Copper, in half-pence. *L.*	8.915
Æs; cuivre. *Petitus.*	8.875
Cuprum. *Bacon.*	8.866
Copper. *Oxf. Soc.*	8.843
Cuprum Suecicum. *Fahrenheit.*	8.834
Id. Japonenſe. *Fahrenheit.*	8.799
Id. Suecicum. *Muſſchenbr.*	8.784
Common copper. *L.*	8.478
BRASS. An old braſs gold weight, marked XXXIII. *Harris.*	8.830
Aurichalcum. *Bacon.*	8.747
A piece of hammered braſs. *Harris.*	8.660
Æs, airin, calaminæ mixtum. *Petitus.*	8.437
Aurichalcum. *Fahrenheit.*	8.412
Braſs hammered. *J. C.* Plate braſs. *Ward.*	8.349

Wrought

Wrought brafs. *J. C.*	8.280
Caft brafs. *L.*	8.208
D°. *J. C. Ward.*	8.100
D°. *Cotes.*	8.000
Brafs hammered. *Reynolds.*	7.950
D°. caft. *Reynolds.*	7.905
A piece of caft brafs. *Harris.*	7.666
♂ IRON. Ferrum. *Villalpandus.*	8.086
Id. *Ghetaldus.*	8.000
Iron, forged. *Reynolds.*	7.906
Ferrum. *Petitus.*	7.875
Id. *Bacon.*	7.837
Spanifh bar iron. *L.*	7.827
Swedifh D°. *L.*	7.818
Ferrum. *Fahrenheit.*	7.817
Iron. *Cotes.*	7.645
D°. of a key, *J. C.* Common iron. *Ward.*	7.643
A piece of hammered iron, perhaps part fteel. *Harris.*	7.600
Iron caft. *Reynolds.*	7.520
D°. caft. *L.*	7.135
Softeft caft iron, or *Dutch* Plates. *L.*	6.960
STEEL. *J. C. Ward.*	7.832
D°. *Cotes.*	7.850
D°. fpring temper. *Hawkfbee.*	7.809
D°. nealed foft. *L.*	7.792
D°. foft. *Haukfbee.*	7.738
D°. hard. *Haukfbee.*	7.704
D°. hardened. *L.*	7.696
♃ TIN. *Reynolds.*	7.617
Stannum. *Bacon.*	7.520
Id. *Villalpandus. Freind.*	7.500
Etain d'Angleterre. *Muffchenbr.*	7.471
Stannum. *Ghetaldus.* $7\frac{2}{5}$	7.400
Id. Provinciæ Indiæ Or Malaçca. *Fahren.*	7.364
Block tin. *Oxf. Soc. Ward. C.*	7.321
Stannum Anglicanum. *Fahrenheit.*	7.313
Id. commune. *Petitus.*	7.312
Id. purum. *Petitus.*	7.170
Block or grain tin. *L.*	7.156

Notes and obfervations.

As I thought the ufes that might be made of thefe tables, either in bufinefs or in philofophy, would beft be illuftrated by a few fhort notes, I have therefore here occafionally inferted fuch obfervations as occurred to me, whilft I was revifing them for the prefs: and, as many of thefe related chiefly to the prefent defects of my tables, thofe I thought would

would probably be of ſervice, to ſuch as might hereafter take the trouble of improving or correcting them.

As the particulars contained in the tables were extracted from different books, at different times, and at firſt only intended for my own private uſe, I was not ſollicitous to preſerve one uniform language, but generally ſet down every experiment in my common place, in the words of the author I took it from: and as I have ſince found, that by a tranſlation I might ſometimes happen not ſo juſtly to repreſent the body intended, I have upon the whole judged it beſt, here alſo to tranſcribe them in the ſame languages in which they were at firſt delivered.

To make experiments of this ſort, with a ſufficient degree of accuracy, requires a pretty deal of care and pains: and, as in ſuch as I have made myſelf, I have found great conveniency in the uſe of decimal weights, preferably to thoſe of the common form, I would alſo recommend the uſe of ſuch to others, who ſhall pleaſe to employ themſelves in the like enquiries. Thoſe I have provided for myſelf have a *Troy* ounce for their *integer*, and my leaſt weight is the thouſandth part of that quantity, differing conſequently from the half of a *Troy* grain only as 24 does from 25, which is inconſiderable ſo far as thoſe ſmall weights are concerned. My four ſmalleſt are reſpectively of 1, 2, 3, and 4 of thoſe thouſandth parts, and together make ten, or an unit of the next denomination, that of the 100th part of an ounce. I then have four others, making 1, 2, 3, and 4 100ths, and together the unit of the next denomination, or one tenth of an ounce, and ſo on. By theſe I ſave the trouble of reducing the common weights to their loweſt denomination in every experiment, and ſometimes perhaps avoid making miſtakes in that very trifling work.

Whenever two or more original writers nearly concur in their experiments upon any ſubject, the gravity ſo deduced may be well depended upon. But where they differ remarkably, it muſt either be imputed to the unequal gravity of the ſubject itſelf, or to ſome error in the trials, which may eaſily happen in matters that depend on the obſervation of ſo many minute particulars. All thoſe caſes that ſo ſenſibly differ would well deſerve to be re-examined.

The firſt table above, that of metals, as it is compoſed of the moſt perfect and uniform bodies in nature, ſeems capable of being adjuſted with the greateſt preciſion, both with relation to the pure metals themſelves, and to the ſeveral degrees of their mixtures one with another, if experiments in all theſe caſes were but made with a ſufficient degree of accuracy.

Gold, in the experiments I have made myſelf, I could never find to come up to the weight aſſigned it in ſome of the former tables, and particularly thoſe I have made upon our own coin, and ſome others have always remarkably fallen ſhort of the weight aſſigned to the ſtandard in thoſe ſame tables. I have inſerted that trial in which I found guineas to come out beſt; and I may venture to affirm, that that experiment, in

 particular,

particular, was made with as much accuracy as my inſtrument was capable of, the pieces were all waſhed in ſoap and water, cleaned with a bruſh, and the air-bubbles well freed, and the like. That experiment is beſides abundantly confirmed ſince, by the exact trials lately made by Mr *Graham* and Mr *Ellicot*, which were performed with the greateſt care; and the fine gold alſo mentioned by the laſt was choſen and prepared with the greateſt curioſity.

It may be obſerved, that the gold medals of Q. *Eliz.* and Q. *Mary*, quoted by *J. C.* were, without doubt, the large ſovereigns of thoſe queens, which were of the old ſtandard of *England*, or of gold appointed to be 23 carats, 3 grains and a half fine: that the *Mentz* ducat, mentioned by the ſame, if it was one of thoſe *ad legem imperii*, which are always in their own mints affirmed to be fine, come out conſiderably too light: and that the gold coin of the Commonwealth, and the piſtoles of *France*, were like our preſent gold money of the goodneſs of 22 carats.

Mercury is placed in this table among the metals, by reaſon of its near agreement with thoſe bodies in its ſpecific gravity; though it otherwiſe ſo widely differs from them in moſt of its properties.

Braſs is conſiderably condenſed by hammering; whether gold, ſilver, and the other metals are alſo condenſed in like manner, hardly appears yet to have been ſufficiently tried.

Of the mixed metals, hardly any except braſs, appear to have had their ſpecific gravities very carefully aſcertained: bell-metal, princes metal, however, and ſome others, might deſerve to be examined in that particular.

It might poſſibly be queried alſo, whether ſeveral mixed metals do not either rarify or condenſe upon mixture, ſo as thereby to acquire a more different ſpecific gravity, than the natural law of their compoſition, at firſt ſeems to require.

It may laſtly be obſerved, that the ſpecific gravities of all the known metals are ſuch, as that none of them come up to 20 times the weight of common water, or fall ſenſibly below 7 times the ſame weight.

Tab. II. *Of minerals, ſemi-metals, ores, preparations, and recrements of metals, &c.*

BISMUTH. *J. C.*	9,859
D°. *Cotes.*	9,700
D°. or tynglaſs. *Boyle.*	9,550
Tynglaſs. *Reynolds.*	7,951
Marcaſita alba. *Fahrenheit.*	9,850
Mineral, Corniſh, ſhining like a marcaſite. *Boyle.*	9,060
Calx of lead. *Boyle.*	8,940
Spelter ſolder. *J. C.*	8,362
Spelter. *J. C.*	7,065
Cinnabar common. *Boyle.*	8,020
Cinnabaris factitia. *Muſſchenbr.* (if not a miſtake for the laſt experiment)	8,200

Cinnabar

Cinnabar native, breaking in poliſhed ſurfaces like Talc. *Davies.*	7,710
D°. *Perſian*, breaking rough. *Davies.*	7,600
D°. native. *Boyle.*	7,576
Cinnabaris nativa. *Muſſchenbr.*	7,300
Cinnabar native, very ſparkling. *Boyle.*	7,060
D°. native, from *Guinea.* *Davies.*	6,280
Cinnabar of antimony. *Harris.*	7,060
D°. another piece. *Harris.*	7,043
D°. *Boyle.*	7,030
Cinnabar antimonii. *Freind.*	6,666
Cinnabre d'antimoine. *Muſſchenb.*	6,044
Lead ore, rich, from *Cumberland.* *Boyle.*	7,540
D°. *Boyle.*	7,140
The reputed ſilver ore of *Wales.* *J. C.*	7,464
The metal thence extracted. *J. C.*	11.087
Regulus antimonii. Item Martis et Veneris. *Freind.*	7,500
Id. *Fahrenheit.*	6,622
Id. *Harris.*	6,600
Id. per ſe. *Davies.*	4,500
Silver ore, choice. *Boyle.*	7,000
D°. another piece from *Saxony.* *Boyle.*	4,970
Lithargyrus argenti. *Freind.*	6,666
Lithargyrium argenti. *Muſſchenbr.*	6,044
Id. Auri. *Freind.*	6,316
Id. Auri. *Muſſchenb.*	6,000
Minera antimonii. *Davies.*	5,810
Cuprum calcinatum. *Freind.*	5,454
Glaſs of antimony. *Newton.* *C.*	5,280
Vitrum antimonii. *Freind.*	5,000
Id. per ſe. *Boyle.*	4,760
Tin ore, choice. *Boyle.*	5,000
D°. black, rich. *Boyle.*	4,180
New *Engliſh* tin ore, Mr *Hubert*'s. *Boyle.*	4,080
Tutty, a piece. *Boyle.*	5,000
Tutia. *Muſſchenb.*	4,615
Lapis calaminaris. *Freind.* Lapis cæruleus Namurcenſis. *Muſſchenb.*	5,000
Id. *Boyle.*	4,920
Loadſtone. *Boyle* V. 6. *b.*	4,930
Magnes. *Petitus.*	4,875
A good loadſtone. *Harris.*	4,750
Marcaſites, one more ſhining than ordinary. *Boyle.*	4,780
A golden marcaſite. *J. C.*	4,589
Marcaſites, from *Stalbridge.* *Boyle.*	4,500
D°. *Boyle.*	4,450

Antimonium Hungaricum. *Muſſchenbr.*	4,700
Antimony, good, and ſuppoſed to be *Hungarian. Boyle.*	4,070
D°. crude, which ſeemed to be very good. *Harris.*	4,058
Antimonium crudum. *Freind.*	4,000
Id. *Davies.*	3,960
Black Sand, commonly uſed on writing. *Boyle.* V. 33. *b.*	4,600
Crocus Metallorum. *Muſſchenb.*	4,500
Id. *Freind.*	4,444
Hæmatites. *Muſſchenbr.*	4,360
Id. *Boyle.* V. 6. *a.*	4,150
D°. *Engliſh. Boyle.*	3,760
Copper ore, rich. *Boyle.*	4,170
D°. *Boyle.*	4,150
Copper-ſtone. *Boyle.*	4,090
Emeri. *Boyle.* V. 26. *b.*	4,000
Manganeſe. *Boyle.*	3,530
A blew ſlate with ſhining particles. *J. C.*	3,500
Iron ore, a piece burnt or roaſted. *Harris.*	3,333
Ceruſſa. *Item* Chalybs cum ſulphure. pp. *Freind.*	3,158
Lapis lazuli. *J. C.*	3,054
D°. *Boyle.* V. 6. *b.*	3,000
D°. *Boyle.*	2,980
Gold ore. *Boyle.* V. 29. *b.*	2,910
D°. not rich, brought from the *Eaſt Indies. Boyle.*	2,652
Another lump of the ſame. *Boyle.*	2,634
A mineral ſtone, yielding 1 part in 160 metal. *J. C.*	2,650
The metal thence extracted. *J. C.*	8,500
Pyrites homogenea. *Fahrenheit.*	2,584
Black Lead. *Boyle.* V. 27. *a.*	1.860
Æs viride. *Freind.*	1,714
Plumbum uſtum. *Freind.*	1,666

The ſecond table is impoſed of ſubjects no way ſtrictly allied to each other, either by their gravities, or their other eſſential properties; and perhaps they might better, on that account, have been divided into different tables.

The bodies themſelves are chiefly of an uncertain and heterogeneous nature; being ſo far as appears compoſed of different elements, and thoſe alſo combined in various proportions, ſuch as ſulphur and arſenic, joined with ſtone, metal, and the like: and from theſe ſeveral degrees of mixture it muſt follow, that moſt of theſe kinds of bodies, tho' ſo far ſimilar as to be called by the ſame names, yet muſt neceſſarily admit of a conſiderable latitude in their ſpecific gravities. Many uſefull deductions may nevertheleſs be drawn from thoſe conſiderations, relating to the comparative goodneſs, &c. of ſuch bodies.

Cinnabar

Cinnabar native, appears to be a compound of mercury and ſulphur, with a portion of earthy or ſtony matter; and that which is heavieſt muſt abound moſt with the mercury. The different appearances which this body makes, would alſo give us a ſuſpicion that there are other varieties in its compoſition, beſides thoſe juſt taken notice of: ſome ſorts of cinnabar, ſuch as the *Hungarian*, breaking into poliſhed planes and ſquares like talc, whilſt others like the *Perſian* of this table, break rough and with ſhining *granulæ* or *micæ*; and that without any conſiderable difference in their gravities.

By the factitious cinnabar it may be determined, what proportion of mercury will ſo incorporate with ſulphur, as to make up an uniform body.

Antimony may in like manner be conſidered as a compoſition of its regulus and ſulphur.

The black ſand uſed on writing is ſaid by Mr *Boyle* to be a rich iron ore: he alſo ſays that emery, loadſtone, and all ſuch ponderous ſtones, contain ſome kind of metal, which he had himſelf ſeparated from them. IV. 120. *a.*

The great variety of ores of all kinds well deſerve to be accurately examined, for the ſake of the many concluſions that may be drawn from thence, concerning the natures of concrete bodies, and for many other purpoſes in metallurgy. But I have as yet met with a very ſmall number of experiments upon theſe ſubſtances. Dr *Woodward* has indeed mentioned a great many obſervations of this ſort which he had made, and kept exact regiſters of: but as they were probably among thoſe paper which he ordered to be deſtroy'd at his death we muſt look upon them as now loſt to the world.

The marcaſites and pyrites are very uncertain and ſtrange kinds of bodies, their gravities are often very great: a marcaſite here taken from *Fahrenheit* was found nearly to equal the heavieſt mineral biſmuth itſelf; and yet it is very ſeldom that any metal or ſemimetal can be obtained in any quantity from theſe ſubſtances, all that is in them being uſually deſtroyed, and carried away by their ſulphur.

Black lead is alſo a very odd kind of mineral, having all the appearance of a ſemimetal, and yet falling ſhort even of the weight of common earth.

The ſemimetals generally exceed in their ſpecific gravities even the baſer metals themſelves.

It may be obſerved, that it appears by this table, that the ſpecific gravities of ores, including the metallic ſtones, are uſually found to lie between 7 and 3 times the weight of water. Lead and ſilver ores are the heavieſt, thoſe of copper, tin, and iron being conſiderably lighter. The gold ore we have an account of muſt be ſo poor as hardly to be worth taking any notice of: but we have in general too few of theſe experiments, to draw any certain concluſions from them.

GRANATE, Bohemian. *Boyle.*	4,360
Granate. *J. C.*	3,978

Tab. III. *Of Gems, Chryſtals, Glaſs, and*

Granati

transparent Stones.

Granati minera. *Boyle.*	3,100
A Pseudo-Topazius, being a natural pellucid, brittle, hairy stone, of a yellow colour. *Newton. C.*	4,270
Sapphires. *Davies.*	4,090
A Sapphire very perfect, but rather pale. *Hauksbee.*	4,068
Glass, blue in sticks from Mr Seale. *Hauksbee.*	3,885
D°. whitest, from Mr Seale. *Hauksbee*	3,380
D°. clear crystal. *Cotes.*	3,150
D°. blue plate, old. *Hauksbee.*	3,102
D°. plate. *L.*	2,942
D°. old looking-glass plate of a light colour. *Hauksbee.*	2,888
D°. green. *Freind.*	2,857
D°. green bottle. *Hauksbee.*	2,746
D°. of a bottle. *Oxf. Soc.* It. a blue paste *Hauksbee.*	2,666
D°. common green. *Hauksbee.*	2,620
D°. deep green old. *Hauksbee.*	2,587
D°. vulgar. *Newton. Ward.*	2,580
Vitrum Venetum. *Freind.*	1,791
An oriental cat's-eye, very perfect. *Hauksbee.*	3,703
A diamond, yellow, of a fine water, somewhat paler than the joinquille. *Hauksbee.*	3,666
D°. white of the second water. eau celeste. *Hauksbee.*	3,540
D°. East Indian, the heaviest of many. *Ellicot.*	3,525
D°. the lightest of many. *Ellicot.*	3,512
D°. Brasilian, the heaviest of many. *Ellicot.*	3,521
D°. the lightest of many. *Ellicot.*	3,501
D°. the mean of all his experiments. *Ellicot.*	3,517
D°. *Newton. C.*	3,400
Diamond bort, of a bluish black, with some little adhering foulness. *Hauksbee.*	3,495
A Jacinth of a fine colour, but somewhat foul. *Hauksbee.*	3,637
A Chrysolite. *Hauksbee.*	3,360
Chrystal cubic, supposed to contain lead. *Woodward.*	3,100
Chrystal from Castleton in Derbyshire having the double refraction. *Hauksbee.*	2,724
Chrystal of Island. *Newton. C.*	2,720
Chrystallum disdiaclasticum. *J. C.*	2,704
Chrystallus de rupe. *Fahrenheit.*	2,669
Chrystal rock. *J. C. Boyle* III. 229. *b.*	2,659
D°. a large shoot. *Hauksbee.*	2,658
D°. of the rock. *Newton. C.* It. chrystal in the lead-mines near Worksworth. *Woodward.*	2,650
D°. *Hauksbee.*	2,646
D°. pure pyramidal, supposed to contain tin. *Woodward.* 2 5 or	2.400
Chrystallus. *Petitus.*	2,287

Chrystal

Chryſtal. *Boyle.*	2,210
Talc. Jamaican. *Boyle.*	3,000
D°. Venetian. *Boyle.*	2,730
D°. *J. C.*	2,657
D°. Engliſh. *Woodward.*	2,600
D°. a piece like lapis amianthus. *Boyle.*	2,280
A red paſte. *J. C.*	2,842
A Braſil-pebble, foul and feather'd. *Hauksbee.*	2,755
D°. a fragment uncut. *Hauksbee.*	2,676
D°. cut. *Hauksbee.*	2,591
Jaſper, ſpurious. *J. C.*	2,666
A Corniſh diamond cut. *Hauksbee.*	2,658
A water topaz, very perfect, but ſaid not to be oriental. *Hauksbee.*	2,653
Pebble pellucid. *J. C.*	2,641
Briſtol ſtone. *Davies.*	2,640
Hyacinth, ſpurious. *J. C.*	2,631
Selenites. *J. C.*	2,322
D°. *Newton.*	2,252

As the mean gravity of chryſtal appears, by the foregoing table, to be little more to that of water than as two and a half to one; it may well be ſuſpected, that the granate, pſeudo-topazius, ſapphire, and ſuch other gemms which greatly exceed chryſtal in weight, do contain a conſiderable portion of ſome ſort of metal in their compoſition: as was obſerved of theſe bodies by Dr *Woodward*, in his Method of Foſſils, p. 24.

As to the white ſapphire, which is reputed by Dr *Woodward* to be a ſpecies of gemm intermediate between chryſtals and the diamond in hardneſs, I have not yet obtained any good account of its ſpecific gravity.

The weight of the diamond is aſcertained in N°. 476 of the *Philoſ. Tranſ.* where it appears, that by experiments made with the greateſt care by Mr *John Ellicot*, F. R. S. with moſt exact inſtruments, and upon 14 different diamonds, ſome of them very large, brought from different places, and having the greateſt varieties of colour and ſhape poſſible; they were all found to agree in weight to a ſurpriſing degree of accuracy, being all ſomewhat above 3½ times the weight of common water.

This indeed differs very ſenſibly from what had been found in ſome former experiments, but it is hardly probable that thoſe had been made upon diamonds of ſo large a ſize as theſe: Mr *Boyle* who found their weight leſs than 3 times that of common water, has himſelf told us in the ſame place, V. 83. *b.* that the ſtone he made uſe of, only weighed about 8 grains. And tho' no doubt can be made of the exactneſs of Sir *I. Newton*'s experiment, by which alſo the ſpecific weight of the diamond came out leſs than Mr *Ellicot*'s, yet it may well be queſtion'd whether Sir *Iſaac* had, at the time when he made his trials, either ſo many or ſo perfect and weighty ſtones, as a favourable opportunity offered to this laſt gentleman. I ſhall therefore only obſerve, that, admitting this laſt to be the true ſpecific weight of the diamond, the refractive power of the ſame, in proportion to its denſity, ſhould in Sir *I. Newton*'s table be leſſened from 14556 to 14071; which would ſtill be greater than what is found in any other body; but is upon the whole more conformable to the general law of that table.

Sir *I. Newton* conjectured a diamond to be an unctuous ſubſtance coagulated, and found it to have its refractive power nearly in the ſame proportion to its denſity as thoſe of camphire, oil-olive, linſeed oil, ſpirit of turpentine, and amber, which are fat ſulphureous unctuous bodies: all which have their refractive powers 2 or 3 times greater in reſpect to their denſities, than the refractive powers of other ſubſtances in reſpect of theirs. Yet muſt it be allowed, that a diamond ſuffers no change by heat in any degree, contrary to the known property of ſulphurs; and as it is moſt reaſonable in our philoſophy to treat ſuch bodies as ſimple, in which we are not able to produce any change or ſeparation of parts, we muſt therefore on that account conſider a diamond as a ſimple body and of the chryſtalline kind.

Glaſs, which is a factitious concrete of ſand and alkaline ſalt, is nearly found to aſſume the mean gravity of ſtones and chryſtals.

If there is no miſtake in the gravity of what Dr *Freind* calls *vitrum Venetum*, it differs very remarkably from all other kinds of glaſs.

I do not know whether the jaſper and hyacinth ſpurious of *J. C.* are to be underſtood as natural or artificial gemms.

Tab. IV. *Of Stones and Earths.*

Sardachates. *J. C.*	3,598
Lapis ſciſſilis cæruleus. *Muſſchenbr.* (qu. if not the ſame experiment mentioned before pag. 222. *a blew ſlate with ſhining particles. J. C.*)	3,500
Cornelian. *Boyle.*	3,290
D°. *J. C.*	2,563
A hone. *J. C.*	3,288
D°. to ſet razors on. *Harris.*	2,960
Marmor. *Petitus.* (probably ſome miſtake in the experiment.)	3,937
Marble. *Reynolds.*	3,026
D°. white. *Haukſbee.*	2,765
D°. white Italian, of a cloſe texture viſibly	2,718
D°. white. *Boyle.* fine. *Ward. C.*	2,710
D°. white Italian, tried twice. *Oxford Soc.*	2,707
D°. black Italian. *Oxford Soc.* veined. *L.*	2,704
D°. black. *Haukſbee.*	2,683
D°. Parian. *L.*	2,560
Lapis amianthus. from Wales. *J. C.*	2,913
Turquoiſe, one of the old rock, very perfect. *Haukſbee.*	2,908
Turcoiſe ſtone. *J. C.*	2,508
Lapis nephriticus. *J. C.*	2,894

Corallium

Corallium rubrum. *Freind.*	3,857
Corall. *J. C*	2,689
D°. red. *Boyle* V. *7. a.*	2,680
D°. *Boyle.*	2,630
D°. white, a fine piece. *Boyle.*	2,570
D°. white, another piece. *Boyle.*	2,540
Emeril ſtone, a ſolid piece. *Hauksbee*	2,766
Paving ſtone. *Reynolds.*	2,708
D°, a hard ſort from about Blaiden. *Oxf. Soc.*	2,460
A Whetſtone, not fine, ſuch as Cutlers uſe. *Harris.*	2,740
Pellets, vulgarly called alleys, which boys play withal. *Hauksb.*	2,711
Engliſh pebble. *L.*	2,696
Lapis Judaicus, *Boyle.*	2,690
Id. *Freind.*	2,500
Maidſtone rubble. *L.*	2,666
Marbles, vulgarly ſo called, which boys play withal. *Hauksbee.*	2,658
Morr ſtone. *L.*	2,656
Agate. *Boyle.*	2,640
D°. German, for the lock of a gun, *Hauksbee.*	2,628
D°. Engliſh. *J. C.*	2,512
Lapis, *Petitus.*	2,625
Flint, black, from the Thames. *Hauksbee.*	2,623
Flint ſtone. *L.*	2,621
A round pebble-ſtone within a flint. *Harris.*	2,610
Eaſt Indian blackiſh. Item, an Engliſh one. *Boyle* III. 243. *a.*	2,600
D°. *Oxford Soc.*	2,542
Corallachates. *J. C.*	2,605
Purbeck ſtone. *L.*	2,601
Free-ſtone. *Reynolds.*	2,584
Portland ſtone. *L.*	2,570
D°. white for carving. *L.*	2,312
Grammatias lapis. *J. C.*	2,515
Onyx ſtone. *J. C.*	2,510
Slate Iriſh. *Boyle.* Lapis hibernicus. *Davies.*	2,490
Wood petrified in lough Neagh. *J. C.*	2,341
Oſteocolla. *Boyle.*	2,240
Heddington ſtone. *L.*	2,204
Allom ſtone. *Boyle.*	2,180
Bolus Armena. *Freind.*	2,137
Hatton ſtone. *L.*	2,056
Burford ſtone, an old dry piece. *Oxford Soc.*	2,049
Heddington ſtone, that of the ſoft lax kind. *Oxford Soc.*	2,029
Terra Lemnia. *Freind.*	2,000
Brick. *Cotes.*	2,000
D°. *Oxford Soc.*	1,979
A Gallypot. *J. C.*	1,928

Alabaſter. *Ward.* *C.*	1,874
D°. *Oxford Soc.*	1,872
A ſpotted factitious marble. *J. C.*	1,822
Stone bottle. *Oxford Soc.*	1,777
A piece of a glaſs (perhaps glazed) coffee-diſh of a brown colour. *Harris.*	1,766
Barrel clay. *L.*	1,712
Lapis de Goa. *Davies.*	1,710
Lapis ruffus Bremenſis. *Muſſchenb.*	1,666
An icicle broken from a grotto (I ſuppoſe ſtalactites.) Dr *Slare,* in *Harris.*	1,190
Chalk, as found by Dr *Slare.* *Harris.*	1,079

The mean gravity of ſtone appears to be to that of water as about $2\frac{1}{2}$ to one, and many ſtones of great hardneſs, ſuch as the onyx, turquoiſe, agate, marble, ſlint, *&c.* do not much exceed that weight. It may therefore well be doubted whether ſuch ſtones, whoſe ſpecific gravity comes up to near three times that of water, or even beyond it, owe their denſity to metalline additions; or whether they are really formed of a different ſpecies of matter, as the diamond ſeems to be.

Coral by it's denſity appears to be a ſtone, though in a vegetating ſtate: or it may poſſibly from ſome late obſervations, be of an animal nature.

What is called *Lapis Hibernicus,* is a ſoft ſtone containing vitriol.

We have not many obſervations upon earths: by thoſe we have, it ſeems probable that they contain the ſame kind of matter in a lax form, of which ſtones are a more ſolid and denſer concretion.

Lapis de Goa is but a trifling compoſition perhaps hardly worth retaining in the tables.

What ſpecies of body ſhould *Alabaſter* be accounted? which with a ſtone-like hardneſs, yet falls ſo much below other ſtones, or even earths in gravity.

Tab. V. *Of Sulphurs and Bitumens.*

SULPHUR. *Petitus.*	2,344
D°. a piece of roll. *Hauksbee.*	2,010
D°. vive. *Boyle.*	2,000
D°. German, very fine. *Boyle.*	1,980
D°. tranſparent, Perſian. *Davies.*	1,950
Sulphur mineralis. *Freind.*	1,875
Brimſtone, ſuch as is commonly ſold. *J. C.*	1,811
D°. *Cotes.*	1,800
Aſphaltum. *Boyle.* III. 243. *a.*	1,400
Scotch Coal. *Boyle.* III. 243. *a.*	1,300
Coal, of Newcaſtle. *L.*	1,270
D°. pit, of Staffordſhire. *Oxford Soc.*	1,240
Jet. *J. C.*	1,238

D°.

D°. *Davies.*	1,160
D°. *Davies.*	1,020
Succinum citrinum. *Davies.*	1,110
Id. pingue. *J. C.*	1,087
Id. flavum (by 2 experiments). *Davies.*	1,080
Id. pellucidum. *J. C.*	1,065
Id. album, item pingue. *Davies.*	1,060
Amber. *Boyle. Newton. C.*	1,040
Fine Gunpowder. *Reynolds.*	0,698

Sulphur is in gravity very nearly the ſame as earth, ſo that it's purity can hardly be aſcertained by it's weight, unleſs the matter it is aſſociated with, is of a ſtony denſity.

The ſemidiaphanous *Sulphur* is a beautiful kind which I have but ſeldom ſeen : it is in lumps of the ſize of a ſmall bean.

Coal, the ſorts here taken notice of are conſiderably lighter than *Sulphur :* but there are many other kinds, and of different weights.

I take the *Gagates* or *Jet* to differ very little from the *Channel Coal.*

The different ſorts of *Amber* may be obſerved not to differ conſiderably in their ſeveral gravities.

Sulphurs ſeem to be the ligheſt of all mineral bodies.

GUM Arabic. *Freind.*	1,430	Tab. VI.
D°. *Newton. C.*	1,375	*Of Gums,*
Opium. *Freind.*	1,360	*Reſins, &c.*
Gum Tragacanth. *Freind.*	1,330	
Myrrh. *Freind.*	1,250	
Gum Guaiac. *Freind.*	1,224	
Reſina Scammonii. *Freind.*	1,200	
Aloes. *J. C.* (qu. whether the reſin or the wood.)	1,177	
Aſa fœtida, a very fine ſample. *Hauksbee.*	1,251	
D°. from Dr *John Keill's Introd. ad veram Phyſicam.*	1,143	
Pitch. *Oxford Soc. C.*	1,150	
Thus. *Freind.*	1,071	
Camphire. *Newton. C.*	0,996	
Bees-wax. *Cotes.*	0,955	
Cera. *Ghetaldus.* (ad aquam ut 95 $\frac{5}{11}$ ad 100.)	0,954	
Wax well freed from the honey. *Davies.*	0,938	
Cera. *Petitus.*	0,937	
D°. the ſame lump 2 years after. *Davies.*	0,942	
Balſamus de Tolu. *Muſſchenbr.*	0,896	
Maſtic. *J. C.* (qu. whether the gum or the wood.)	0,849	

The bees wax in my own experiments was well freed from honey, by the boiling it in water, which probably made it lighter than it was ſet down in Mr *Cotes*'s table : and the ſecond experiment which I made

two

two years after the firſt, if the difference was not owing to the difference of heat, is an inſtance of what I take to be a pretty general truth, that bodies become more denſe and compact by reſt, and that they would alſo be found heavier in the ſcale, in thoſe caſes where they do not loſe weight by the evaporation of humidity.

The weights of vegetable gums nearly correſpond with thoſe of the ligneous parts.

Tab. VII. *Of Woods, Barks, &c.*

COCO ſhell. *Boyle.*	1,345
Bois de Gayac. *Muſſchenbr.*	1,337
Lignum Guaiacum. *Freind.*	1,333
Lignum vitæ. *Oxf. Soc.*	1,327
Speckled wood of Virginia. *Oxf. Soc.*	1,313
Cortex Guaiaci. *Freind.*	1,250
Lignum nephriticum. *Freind.*	1,200
Lignum aſphaltum. *J. C.*	1,179
Ebony. *J. C. Item* Aloes. *J. C.*	1,177
Santalum rubrum. *J. C.*	1,128
Id. album. *J. C.*	1,041
Id. citrinum. *J. C.*	0,809
Lignum Rhodium. *J. C.*	1,125
Radix Chinæ. *Freind.*	1,071
Dry Mahogany. *L.*	1,063
Gallæ. *Freind.*	1,034
Red-wood. *Oxf. Soc.* It. Box-wood. *Oxf. Soc. Ward. C.*	1,031
Log-wood. *Oxf. Soc.*	0,913
Oak, dry, but of a very ſound cloſe texture. *Oxf. Soc.*	0,932
D°. tried another time. *Oxf. Soc.*	0,929
D°. ſound dry. *Ward.*	0,927
D°. dry. *Cotes.*	0,925
D°. dry, *Engliſh.* *L.*	0,905
Oak of the outſide ſappy part, fell'd a year ſince. *Oxf. Soc.*	0,870
D°. *Reynolds.*	0,801
D°. very dry, almoſt worm-eaten. *Oxf. Soc.*	0,753
Dry Wainſcot. *L.*	0,747
Beech meanly dry. *Oxf. Soc.*	0,854
Maſtic (qu. if the wood or gum). *J. C.*	0,849
Aſh dry about the heart. *Oxf. Soc.*	0,845
D°. dry. *Cotes.*	0,800
D°. meanly dry, and of the outſide lax part of the tree. *Oxf. Soc.*	0,734
Elm dry. *L.*	0,800
D°. *Reynolds.*	0,768
D°. *Oxf. Soc. C.*	0,600
Rad. Gentianæ. *Freind.*	0,300
Cortex Peruvianus. *Freind.*	0,734

Crabtree

Crabtree meanly dry. *Oxf. Soc.*	0,765
Yew, of a knot or root 16 years old. *Oxf. Soc.*	0,760
Maple dry. *Oxf. Soc. C.*	0,755
Plumtree dry. *J. C.*	0,663
Fir, dry yellow. *L.*	0,657
Dry white Deal. *L.*	0,569
Lignum Abietin. *Freind.*	0,555
Fir dry. *Cotes.*	0,550
D°. *Oxf. Soc.*	0,546
Walnut-tree dry. *Oxf. Soc.*	0,631
Cedar dry. *Oxf. Soc.*	0,613
Juniper-wood dry. *J. C.*	0,556
Saſſafras wood. *J. C.*	0,482
Cork. *Cotes.*	0,240
D°. *J. C.*	0,237

Dr *Jurin* has obſerved in the *Phil. Tranſ.* N°. 369. that the ſubſtance of all wood is ſpecifically heavier than water, ſo as to ſink in it, after the air is extracted from the pores and air-veſſels of the wood, by placing it in warm water under the receiver of an air-pump ; or if an air-pump cannot be had, by letting the wood continue ſome time in boiling water over a fire. The ſeveral weights therefore above given muſt be looked upon as the weights of the concrete bodies, in the condition they were, before the air was either forcibly got out, or the water driven into the ſmall hollows : and both theſe conſiderations may have their uſe as notwithſtanding that the ſpecific weights of the ſolid particles are truly heavier than water, we ſhall from the weights of the bodies as they are now compounded, be enabled to make ſome judgment of their poroſity, ſo far as they may be penetrable by water or other fluids.

MANATI lapis. *Boyle.*	2,860	Tab. VIII.
D°. another *Boyle.*	2,330	*Of Animal*
D°. a fragment of. *Boyle.*	2,290	*parts.*
D°. *J. C.* another from Jamaica. *Boyle.*	2,270	
Pearl, very fine ſeed oriental. *Boyle* V. 12 *a.*	2,750	
D°. a large one, weighing 206 grains. *Boyle* V. 7. *b.*	2,510	
Murex ſhell. *J. C.*	2,590	
Crabs-eyes artificial. *Boyle.*	2,480	
D°. native. *Boyle.*	1,890	
Os ovinum recens. *Freind.*	2,222	
Oyſter ſhell *J. C.*	2,092	
Calculus humanus, juſt voided. *Davies.*	2,000	
D°. *Boyle* V. 7. *b.*	1,760	
D°. *Boyle.*	1,720	
D°. *Cotes.*	1,700	
D°. *Boyle* V. 7. *b.*	1,690	

D°. *J. C.*	1,664
D°. *Davies.*	1 650
D°. *Boyle.*	1 470
D°. *J. C.*	1,433
D°. *Davies.*	1,330
D°. *J. C.*	1,240
Rhinoceros horn. *Boyle.*	1,990
The top part of one. *J. C.*	1,242
Ebur. *Freind*	1,935
Ivory. *Boyle.*	1,917
D°. dry. *Oxford Soc.* C.	1,826
D°. *Ward.*	1,823
Unicorn's horn, a piece. *Boyle.*	1,910
Cornu Cervi. *Freind.*	1,875
Ox's horn, the top part of one. *J. C.*	1,840
Blade bone of an Ox. *J. C.*	1,656
A ſtone of the bezoar kind found with four others in the inteſtines of a mare. *Edw. Bailey*, *M. D.* of *Havant* in *Hampſhire.* See Philoſoph. Tranſact. N°. 481.	1,700
Bezoar ſtone. *Boyle.*	1,640
D°. a large one. *Davies.*	1,570
D°. being the kernel of another. *Boyle* V. 8. *a.*	1,550
D°. a fine oriental one. *Boyle.*	1,530
D°. two weigh'd ſeparately. *Davies.*	1,504
D°. *Cotes.*	1,500
D°. *Boyle.*	1,480
D°. *Boyle.*	1 340
A ſtone from the gall-bladder. *Hales.*	1,220
Blood human, the globules of it *Jurin by calculation.*	1,126
D°. the Craſſamentum of. *Jurin from experiments.*	1,086
D°. *Davies*	1,084
D°. from another experiment. *Jurin*	1,082
Sanguinis humani cuticula alba *Davies.*	1,056
Human blood when grown cold *Jurin*	1,055
The ſame as running immediately from the vein. *Jurin.*	1 053
The ſerum of human blood. *Jurin.*	1,030
D°. *Davies.*	1,026
Ichthyocolla. *Freind.*	1,111
A Hen's egg. *Davies.*	1,090
Milk. *J. C.* C.	1,031
Lac caprinum. *Muſſchenbr.*	1,009
Lac *Freind*	0,960
Urine. *J. C* C.	1.030
Id. *Freind.*	1,012

Mancti

Manati lapis is ſaid to be a ſtone, found in the head of the manatee, or ſea-cow of the *Weſt-Indies.* See *Ray's Syn. Meth. Anim. Quad. &c. Lond.* 1693. 8^vo. Theſe ſtones and pearls are the heavieſt of all the animal productions we are acquainted with.

Dr *Jurin* has obſerved, *Phil. Tranſ.* N°. 369. that, in examining freſh human *calculi* whilſt they were ſtill impregnated with urine, he had met ſuch as exceeded the weight of ſome ſorts of burnt earthen ware and alabaſter, and approached very near to that of brick, and the ſofter ſort of paving ſtone; which I have myſelf alſo found to be true. Whereas thoſe who have made their experiments upon ſuch *calculi*, as had moſt probably been a conſiderable time taken out of the bladder, and had conſequently loſt much of their weight, by the evaporation of the urine, with which they had at firſt been ſaturated, have found thoſe ſtones commonly to have been but about one half part, and ſome of them no more than a fourth part, heavier than an equal bulk of water. From whence it has been too haſtily concluded, that theſe ſtones have very improperly been called by that name, as not at all approaching to the ſpecific gravity of even the lighteſt real ſtones that we have any account of.

The *Calculus Humanus* and *Animal Bezoar* approach nearly to each other in their ſpecific gravity.

Mr *Boyle* has taken notice of the great difference to be found between the gravity of the true and the factitious crabs-eyes. It is ſtrange that the factitious ſhould be made of ſuch materials as can bring them ſo near to the mean gravity of true ſtones: and this conſideration may deſerve the attention of thoſe who may think that any particular dependance is to be had upon the uſe of theſe bodies in medicine.

Dr *Jurin* was the firſt who carefully examined the ſpecific gravities of the different parts which compoſe human blood; and his experiments were performed with the greateſt accuracy. It may be obſerved, that the blood is, by an eaſy *analyſis* divided into *ſerum* and *craſſamentum*; and the *craſſamentum* again into the glutinous and the red globular parts, whoſe ſpecific gravities are the greateſt. It had before theſe experiments been the general received opinion, that the globules of the blood were lighter than the ſerum; and this indeed ſeemed to follow from Mr *Boyle*'s experiments in his natural hiſtory of *human blood*; from which he deduced the ſpecific gravity of the maſs itſelf, to be to that of water as 1040 to 1000, and that of the ſerum alone to be to the ſame as 1190. And theſe numbers 1040 and 1190 had accordingly, till Dr *Jurin* re-examined the the affair, been conſtantly taken to repreſent the true gravities of human blood and it's ſerum reſpectively. See Dr *Jurin*'s *Diſſertation* in *Phil. Tranſ.* N°. 361.

Milk is made by Dr *Freind* to fall more ſhort of the gravity of water, than it is made to exceed the ſame by *J. C.* Poſſibly this difference might ariſe from the milk's being taken in one caſe warm from the cow, and in the other after it had ſtood ſome time.

Tab. IX. *Of Salts.*

MERCURIUS dulcis bis fublim. *Muffch.*	12,353
Mercurius dulcis. *Freind.*	11,715
Id. ter fublim. *Muffchenbr.*	9,882
Id. tertio fublim. Item Panacea rubra. *Freind.*	9,372
Id. quater fublim. *Muffchenbr.* Item turpethum minerale.	8,235
Id. *4to* fublim. Item turpeth mineral. *Freind.*	7,810
Sublimat corrofiv. *Muffchenbr.*	8,000
Id. *Freind*	6,045
Cinis clavellatus, fordibus faleque fuo neutro quodam (quod fere femper magis vel minus in cinere illo reperitur) depurgatus. *Fahrenheit.*	3,112
Sal illud neutrum. *Fahrenheit.*	2,642
Saccharum Saturni. Item fal nitri fix. *Muffchenbr.*	2,745
Eadem. *Freind.*	2,600
Magifterium Coralli. Item pulvis fympatheticus. *Freind.*	2,231
Tartarum vitriolatum. *Muffchenbr.*	2,298
Id. *Freind.*	2,186
Sal mirabile Glauberi. *Muffchenbr.*	2,246
Id. *Freind.*	2,132
Tartarum emeticum. *Muffchenbr.*	2,246
Id. *Freind.*	2,077
Sal Gemmæ. *Newton. C.*	2,143
Nitrum. *Fahrenheit.*	2,150
Nitre. *Newton. C.*	1,900
Id. *Freind.*	1,671
Sal Guaiaci. Item Sal enixum. Item Sal prunellæ. Item S. Polychreft. *Muffchenbr.*	2,148
Eadem omnia. *Freind.*	2,030
Sal maritimum. *Fahrenheit.*	2,125
Cremor Tartari. Item Vitriol. alb. Item Vitriol. rubefact. Item S. Vitriol. *Muffchenbr.*	1,900
Cremor Tar. Item Vitriol. alb. *Freind.*	1,796
Vitriol Englifh, a very fine piece. *Boyle.*	1,880
D°. Dantzick. *Newton. C.*	1,715
Alumen. *Fahrenheit.*	1,738
Alum. *Newton.*	1,714
Sal chalybis. *Freind.*	1,733
Borax. *J. C.*	1,720
D°. *Newton. C.*	1,714
Vitriolum viride. Item Calcanth. rubefact. Item S. Vitriol. alb. *Freind.*	1,671
Saccharum albiff. *Fahrenheit.*	1,606 $\frac{1}{10}$
Mel. *Villalpandus*	1,500
Id. *Ghetaldus* $1\frac{9}{20}$. Honey. *Cotes.*	1,450
Sal volatile Cornu Cervi. *Muffchenbr.*	1,496
Id. *Freind.*	1,421

Sal

Sal Ammoniac. purum. Item Ens Martis ſemel ſublimat. *Muſſchenb.*	1,453
Eadem. *Freind.*	1,374
Ens Martis ter ſublimat. *Muſſchenb.*	1,269
Id. *Freind.*	1,233

Moſt of the experiments in the ninth table are taken from Dr *Freind*, who weighed the ſalts in ſpirits of wine, and regiſtered the proportional gravity of the ſalts to the ſpirits. But the misfortune is, that the gravity of the ſpirits of wine he made uſe of is not regiſtered: ſo that the experiments cannot with certainty be reduced to the common ſtandard of water. He has delivered the gravity of ſpirits of wine to be 0,818, and that of ſpirits of wine rectified to be 0,78. I have ſuppoſed the ſalts to be weighed in the laſt, as being the fitteſt for the purpoſe: but which he really uſed can only be conjectured.

There appears indeed to be a way to diſcover the weight of the ſpirits of wine, in which Dr *Freind* weighed his ſalts: for he weighed 60 grains of mercury, both in water and in ſpirits of wine, and the loſs of it's weight was reſpectively $4\frac{1}{4}$ grains and $2\frac{2}{3}$. Now the gravities of theſe fluids muſt be in the ſame proportion, and this would give for the weight of the ſpirits of wine 0,627, which is much too little for the weight of his own rectified ſpirits, tho' even that is leſs than what is aſſigned by any other author. So that, upon the whole, nothing can really be concluded from this experiment; and it muſt be allowed beſides, that 60 grains of mercury take up too ſmall a bulk in theſe fluids, to have their gravities determined with any exactneſs thereby.

As Prof. *Muſſchenbroke* has given in his table the ſpecific weights of many of the ſame ſalts which are mentioned by Dr *Freind*, but which differ conſiderably from the weights above ſet down, as reſulting from the Doctor's experiments, I have alſo tranſcribed the Profeſſor's numbers from his own table. Theſe do not however appear to me to be derived from new or differing experiments, but from the very ſame related by Dr *Freind*, only computed from the ſuppoſition of a heavier ſort of ſpirits of wine, whoſe ſpecific gravity is ſuppoſed to have been 0,823. The gravity of the *ſublimate corroſive*, ſet down 8,000, I take to be a miſtake, made by the writing down it's comparative weight to that of the ſpirits themſelves, inſtead of the water to which it ſhould have been referred.

It requires great care and attention to take the ſpecific gravities of ſalts with ſufficient accuracy. They diſſolve in water, and in ſome degree in all fluids that partake of the nature of water. If therefore ſpirits of wine are made uſe of for this purpoſe, they ought to be highly rectified, their own gravity accurately aſcertained, and their degree of heat ſhould be preſerved uniform. For as this fluid rarefies much faſter than water does, a ſmall difference of heat would ſenſibly effect the gravities of the ſalts to be determined by it. And perhaps ſpirit of turpentine were a more proper fluid to be employed on these occasions.

It is remarkable, that *Tartar vitriolat. Sal gem. Sal mirabile, Sal maritimum, Nitre*, &c. being salts composed of different acids and an alkaline salt, should so far exceed in gravity the vitriolic salts, composed of the most heavy acid and a metallic earth. Is not this owing to it's forming less solid chrystals, and to it's containing large quantities of air concealed in it's pores?

The great difference in the weight of the *Nitre*, in the several experiments of *Fahrenheit, Newton*, and *Freind*, may possibly be owing to the quantity of it's concealed air.

Tab. X. *Of Fluids.*

MERCURY. *Ward. C.* (see Tab. I. among the metals)	14,000
Oleum Vitrioli. *Fahrenheit.*	1,8775*
Oil of Vitriol. *Newton. C.*	1,700
Spiritus Nitri Hermeticus. *Freind.*	1,760
Id. *Musschenb.*	1,610
Lixivium cineris clavellati, sale quantum fieri potuit impregnatum. *Fahrenheit.*	1,5713*
Id. alio tempore præparatum. *Fahrenheit.*	1,5634*
Oil of tartar. *Cotes.* Ol. tartari per deliquium. *Musschenb.*	1,550
Spiritus Nitri, cum Ol. Vitrioli. *Freind.*	1,440
Id. *Musschenb.*	1,338
Spiritus Nitri communis. Item Bezoardicus. *Freind.*	1,410
Spirit of Nitre. *Cotes.* Item Sp. Nit. Bezoardicus. *Musschenb.*	1,315
Sp. Nitri. *Fahrenheit.*	1,2935*
Sp. Nitri dulcis. *Musschenb.*	1,000
Aqua fortis melioris notæ. *Fahrenheit.*	1,409*
Eadem, duplex. *Freind.*	1,340
Aqua fortis. *Cotes.*	1,300
Eadem, simplex. *Freind.*	1,100
Solutio salis comm. in aqua saturata. *Davies.*	1,244
Eadem, 1 in aquæ 2,7 part. ponderis. *Davies.*	1,240
Eadem, 1 in aquæ 3 part. *Davies.*	1,217
Eadem, 1 in aquæ 3 part. *Freind.*	1,146
Eadem, 1 in aquæ 12 part. *Davies.*	1,060
Soap Lees the strongest. *Jurin.*	1,200
D°. Capital. *Jurin.*	1,167
Spirit of Vitriol. *Freind.*	1,200
Spiritus Salis cum Ol. Vitriol. *Musschenb.*	1,154
Idem, &c.. *Freind.*	1,146
Spirit of salt. *Cotes.* Sp. Salis marini. *Musschenb.*	1,130
Sp. Salis communis. *Freind.*	1,037
Sp. Salis dulcis. *Musschenb.*	0,951
Id. *Freind.*	0,890

Sp.

Sp. Salis Ammoniaci succinat. Item, cum ciner. clavellat. *Freind.*	1,120
Sp. Salis Ammoniac. cum calce. *Musschenb.*	0,952
Idem cum calce viva. *Freind.*	0,890
Sp. Cornu Cervi non rectific. *Freind.*	1,073
Sp. Serici. *Musschenb.*	1,145
Sp. Urinæ. *Cotes.*	1,120
Solutio Salis enixi, 1 in aquæ 5 part. *Freind.*	1,100
Oleum Sassafras. *Musschenb.*	1,094
Decoctio Gentianæ. *Freind.*	1,080
Sp. Tartari. *Freind. Musschenb.*	1,073
Decoctio Bistortæ. *Freind.*	1,073
Decoctio Sarzæ. It. Chinæ. *Freind.*	1,049
Decoctio ari. It. Sp. Salis comm. *Freind.*	1,037
Oleum Cinnamomi. *Musschenb.*	1,035
Ol. Caryophyllorum. *Musschenb.*	1,034
Beer-Vinegar. *Oxf. Soc.*	1,034
Acetum Vini. *Musschenb.*	1,011
Id. distillatum. *Musschenb.*	0,994
Acetum. *Freind.*	0,976
Sack. *Oxf. Soc.*	1,033
Sp. Ambræ. *Musschenb.*	1,031
Sea-Water. *Cotes.*	1,030
D°. settled clear. *Oxf. Soc. Ward.*	1,027
College plain ale. *Oxf. Soc.*	1,028
Solutio Aluminis, 1 in aquæ 5,33 part. Item Solutio Sal. Amm. purif. 1, et vitriol. alb. 1 in aquæ 5 part. *Freind.*	1,024
Laudanum liquidum Sydenhami. It. Panacea Opii. *Freind.*	1,024
Decoctio Cort. Peruv. Item, Granatorum. *Freind.*	1,024
Moil Cyder, not clear. *Oxf. Soc.*	1,017
Aqua fluviatilis. *Musschenb.*	1,009
Tinctura Aloes cum aqua. Item, Decoctio Santali rubri *Freind.*	1,000
Rain water. *Newton, Reynolds.* Common water. *Cotes.* Common clear water. *Ward.* Pump water. *Oxf. Soc. J. C.* Aqua. *Ghetaldus.* Aqua pluviatilis. *Fahrenheit, Musschenb.* &c.	1,000
Aqua vel vinum. *Villalpandus.*	1,000
Aqua putealis. *Musschenb.*	0,999
Oleum Fœniculi. *Musschenb.*	0,997
Oleum Anethi. *Musschenb.*	0,994
Aqua distillata. *Musschenb.*	0,993
Wine, Claret. *Oxf. Soc.*	0,993
D°. red. *Ward.*	0,992
Vinum. *Petitus.*	0,984
Id. *Ghetaldus.* (ad aquam ut 98⅓ ad 100)	0,983

Vinum

Vinum Burgundicum. *Musschenb.*	0,953
Oleum Sabinæ. It. Hysiopi. *Musschenb.*	0,986
Ol. Ambræ. It. Pulegii. *Musschenb.*	0,978
Ol. Menthæ. It. Cumini. *Musschenb.*	0,975
Decoctio Sabinæ. *Freind.*	0,960
Infusio Marrhubii. It. Menthæ. It. Absynth. *Freind.*	0,950
Ol. Nucis Moschatæ. *Musschenb.*	0,948
Ol. Tanaceti. *Musschenb.*	0,946
Ol. Origani. It. Carvi. *Musschenb.*	0,940
Elixir propr. cum Sale volat. It. Infusio Theæ. *Freind.*	0,940
Ol. Spicæ. *Musschenb.*	0,936
Ol. Rorismarini. *Musschenb.*	0,934
Linseed Oil. *Newton. C.*	0,932
D°. *Ward.*	0,931
Spirits of wine proof, or Brandy. *Ward.*	0,927
Sp. of wine well rectified. *Newton. C.*	0,866
Alcohol Vini. *Fahrenheit.*	0,826
Id. magis dephlegmatum. *Fahrenheit.*	0,825
Sp. Vini. *Freind.*	0,818
Id. rectific. *Freind.*	0,781
Esprit de Vin etheré. *Musschenb.*	0,732
Spiritus Croci. *Freind.*	0,925
Lamp Oil. *Reynolds.*	0,924
Oleum. *Ghetaldus.* (ad aquam ut $91\frac{2}{3}$ ad 100.)	0,916
Oil Olive. *Newton. C.*	0,913
D°. *Ward.*	0,912
Sallad Oil. *Reynolds.*	0,904
Oleum. *Villalpandus.*	0,900
Id. *Petitus.*	0,891
Ol. Raparum. *Fahrenheit.*	0,913
Id. It. Tinct. Chalyb. Mynsicht. It. Tinct. Sulphur cum Sp. Terebinth. *Freind.* It. Huile de semences de navets. *Musschenb.*	0,853
Sp. Mellis. *Musschenb.*	0,895
Sp. Salis Ammoniaci cum calce viva.	0,890
Oleum Aurantiorum. *Musschenb.*	0,888
Spirit of turpentine. *Newton. C.*	0,874
Tinct. Castorei. Item Sp. Vini camphorat. *Freind.*	0,870
Oil of turpentine. *Boyle* V. 22. *a.*	0,864
Ol. Terebinth. *Freind.*	0,793
Ol. Ceræ. *Musschenb.*	0,831
Tinctura Corallii. *Freind.*	0,828
Aqua cocta. *Freind.*	0,750
Air. *Newton. C.*	0,00125
Aër *Princip. Edit.* 3. p. 512. Aër juxta superficiem terræ occupat quasi spatium 850 partibus majus quam aqua ejusdem ponderis.	0,00118

The

The same, by an experiment made by the late Mr *Francis Haukſbee*, F. R. S. when the barometer ſtood at 29,7 inches. See *Phyſico Mathem. Exp.* p. 74. 0,00113

As to the abſolute weight of water with which all the other bodies are compared in theſe tables, Mr *Boyle* tells us in his *Medicina Hydroſt.* printed in the new edition of his works, V. 19. *b.* that he had found by his own experiments, that a cubic inch of clear water weighed 256 *Troy* grains. And Mr *Ward* of *Cheſter*, who afterwards purſued this affair with great accuracy, determined that a cubic inch of common clear water did weigh by his tryals 253.18 like troy grains, or 0.527458 decimals of the *Troy Ounce*, or 0. 78697 of the *ounce averdupois*, agreeable to what Mr *Reynolds* had formerly delivered, who found the inch cubic of rain water to weigh by his experiments 0.579036 decimals of the ſame averdupois ounce, differing from the other only 0.000339 parts.

But, as the accuracy of all the experiments in theſe tables depends upon the identity of the weight of common water, it may not be improper to aſcertain that point by a note taken from Mr *Boyle's MedicinaHydroſtatica*, V. 18. *b.* where he expreſſes himſelf in the following manner.

—— " It ſpeciouſly may, and probably will, be objected, that ——
" there may be a great diſparity betwixt the liquors that are called, and
" that deſervedly, *common water*. And ſome travellers tell us from the
" preſs, that the water of a certain eaſtern river, which if I miſtake not is
" *Ganges*, is by a fifth part lighter than our water. But —having had
" upon ſeveral occaſions the opportunity as well as curioſity to examine
" the weight of divers waters, ſome of them taken up in places very di-
" ſtant from one another. I found the difference between their ſpecific
" gravities far leſs than almoſt any body would expect. And if I be not
" much deceived by my memory (which I muſt have recourſe to, becauſe
" I have not by me the notes I took of thoſe trials) the difference be-
" tween waters, where one would expect a notable diſparity, was but
" about the thouſandth part (and ſometimes perchance very far leſs) of
" the weight of either. Nor did I find any difference conſiderable in
" reference to our queſtion, between the weight of divers waters of dif-
" ferent kinds, as ſpring-water, river-water, rain-water, and ſnow-
" water; though this laſt was ſomewhat lighter than any of the reſt.
" And having had the curioſity to procure ſome water brought into *Eng-*
" *land*, if I much miſremember not, from the river *Ganges*, itſelf; I
" found it very little, if at all, lighter than ſome of our common
" waters."

The heavieſt fluid we are acquainted with, next to *Mercury*, is *Oil* of *Vitriol*, or water impregnated with the *vitriolic acid* in the higheſt degree we can obtain it, being almoſt double the weight of water.

The next is probably the *ſaturated ſolution* of the *fix'd ſalt of vegetables*: being a ponderous ſalt, and diſſolving freely in water.

The

The next to this is *spirit of nitre. spirit of salt* is lighter, and inferior in weight to the *saturated solution of salt* itself.

It is observable, that *marine* or *common salt* and *nitre* differ little in gravity, contrary to the nature of their *spirits*.

The several *solutions* of *common salt*, if accurately repeated, would shew in what proportion the gravities of fluids increase, upon the addition of salt: and that *sea-water* does not contain one twenty-fourth part of salt.

I have omitted in this table the three animal fluids, milk, serum of blood, and urine, as the same may be seen before in the 8*th* table, that of *animal parts*; but it may be noted in general, that the specific gravity of all these fluids is nearly the same as that of sea water.

There are in Dr *Freind*'s table several decoctions of plants, which I have inserted, altho' they are not I think of much use, nor greatly to be depended upon. Several of them are lighter than common water, in contradiction to Dr *Jurin*'s observation, that *Vegetable parts* are all heavier that water: But it is probable these experiments were made before the *decoctions* were reduced to the temper of *common water*.

What is meant by the *aqua cocta* of Dr *Freind* in his table, I cannot imagine; not having any idea of such a change by boiling or otherwise, as can deprive common water of a full fourth part of it's weight.

Since the density of the air is as the force by which it is compressed, it follows that the weight of any portion of air must vary in the same proportion with the weight of the whole *atmosphere*: which in our climate is not less than $\frac{1}{10}$ of the whole weight, allowing the *Barometer* to vary from 28 to 31 inches.

Again, by an experiment of the late Mr *Haukshee*'s in his *Phys. Mechan. Exp.* pag. 170. the density of the air varies one eighth part between the greatest degree of heat in summer, and that of cold in the winter season. So that the air, in a hard frost, when the *Mercury* stands at 31 inches, is near a fifth part specifically heavier, than it is in a hot day when the *Mercury* stands 28 inches.

Tab. XI. *From Monf.* Homberg *and* John Caspar Eisenschmid, *of the proportion of the specific weights of certain fluids in the winter, to the weights of the same in the summer season.*

Mercurius	1,00479
Aqua pluvialis	1,00809
Aqua fluviatilis	1,00811
Aqua distillata.	1,00815
Spirit. Vitriol.	1,01272
Lac bubulum	1,01316
Aqua marina	1,01351
Spir. Salis	1,01467
Acetum	1,01600
Ol. Vitrioli	1,02131
Ol. Terebinth.	1.02141
Aqua fortis	1,02637
Ol. Tartari	1,03013

Spir.

Spir. Vini	1,03125
Spir. Nitri	1,04386

The oils of olive and ſweet almonds congealing with the cold, could not be examain'd by the *Aræometer* in the winter ſeaſon.

According to this table, the increaſe of the ſpecific weight of common water in the winter above it's weight in the ſummer, is not more than about the one hundred and twenty-fourth part of the whole; which is little more than half of what Prof. *Muſſchenbroek* has elſwhere accounted the ſame, *deſorte qu' un pied cubique Rhenan d'eau, qui peſe environ* 64 *livres en Eté ſe trouvera être en Hiver de preſque* 65 *livres. Eſſai de Phyſique, p.* 424. but ſure this difference is much too great.

Notwithſtanding that all fluids are condenſed by cold, it is only till ſuch time as they are ready to freeze; for upon the freezing they immediately expand again, ſo as for the ice to be lighter ſpecifically than the fluid of which it is formed, and to ſwim in it: *Muſſchenbroek* gives the ſpecific weight of ice to be to that of water commonly as 8 to 9. *La peſanteur de la glace eſt ordinairment a celle de l'eau, comme* 8 *a* 9. p. 441. I am not acquainted with any other accurate experiments upon this ſubject and it is hard to get ice in which there are not large bubbles of air included.

The *Philoſ. Soc.* at *Oxford*, together with their table of *Specific Gravity* already ſo often mentioned in the foregoing pages, communicated beſides at the ſame time, to the *Royal Society*, another table of a groſſer nature indeed, but which being printed in the ſame Num. 169. of the *Philoſ. Tranſ.* and appearing to be of uſe for many purpoſes: I have thought the ſame not improper to be here alſo tranſcribed.

Of the weight of a cubic foot of divers grains, &c. tried in a veſſel of well-ſeaſon'd Oak, whoſe concave was an exact cubic foot.

The following bodies were poured gently into the veſſel, and thoſe in the firſt 12 experiments were weigh'd in ſcales turning with two ounces; but the laſt 7 were weighed in ſcales turning with one ounce. The pounds and ounces here mentioned are averdupois weight.

	℔	℥
1. A foot of *Wheat* (worth 6 *s.* a buſhel).	47	8
2. *Wheat* of the beſt ſort (worth 6 *s* 4 *d.* a buſhel). Both ſorts were red *Lammas Wheat* of laſt year.	48	4
3. The ſame ſort of *Wheat* meaſured a ſecond time.	48	2
4. White *Oats* of the laſt year.	29	8
The beſt ſort of *Oats* were 2 *d.* in a buſhel better than theſe.		
5. Blue *Peaſe* (of the laſt year) and much worm-eaten.	49	12
6. White *Peaſe* of the laſt year but one.	50	8
7. *Barley* of the laſt year (the beſt ſort ſells for 1 *s.* 6 *d.* in a quarter more than this).	41	2
8. *Malt* of of the laſt year's *Barley*, made 2 months before.	30	4
9. Field *Beans* of the laſt year but one.	50	8
10. Wheaten *Meal* (unſifted).	31	0
11. Rye *Meal* (unſifted).	28	4

	℔	℥
12. Pump-*Water.*	62	8
13. Bay *Salt.*	54	1
14. White Sea-*Salt.*	43	12
15. *Sand.*	85	4
16. Newcaſtle *Coal.*	67	12
17. Pit *Coal*, from *Wedneſbury* 63; but this is very uncertain in the filling the interſtices betwixt the greater pieces.	63	0
18. *Gravel.*	109	5
19. Wood *Aſhes.*	58	5

Of the ſame nature is alſo the following account of *The difference of the weight of ſome liquors upon the tun compared to rain water*, from the experiments made formerly by Mr *Reynolds* in the *Tower* of *London*, and communicated to the *Royal Society*, with his others before-mentioned, by Mr *Smethwick*, *July* 7. 1670.

	℔	℥ *Averd.*
Muſcadine wine was found heavier than rain water	11	2
Milk	8	4
Sherry	5	3
Ale	5	2
Canary Wine	3	3
Small Beer	1	3
White wine was found lighter than rain water	1	2
Rheniſh wine	1	4
Claret	1	6
Sallet Oil	21	6

The proportion given by this author as the true one of the *Averdupois* pound to the *Troy* pound is, that 14 of the former are equal to 17 of the latter.

From whence the *Averdupois* pound would be found equal to 6994.285 and the *ounce* to 437.143 *Troy grains*; which is indeed a little leſs than the ſame have ſince been determined by others; for Mr *Ward* of *Cheſter* gives from a very nice experiment as he calls it, of his own, that one pound averdupois was equal to 14 ounces 11 penyweight and 15½ *Troy grains*, or to 6999½, and conſequently the ounce averdupois to 437.47 of the ſame grains. And ſeveral gentleman of the *Royal Society* who very carefully on 22 *April* 1743. examined the original ſtandards of weights kept in the *Chamberlain's Office* of his MAJESTY's *Exchequer*, found, upon the medium of the ſeveral trials which they made with thoſe ſtandards, that the *Pound Averdupois* was equal to 7000.14, and the *Ounce Averdupois* to 437.51 *Troy grains*. *Phil. Tranſ.* N°. 470.

I ſhall conclude theſe papers with the two tables from *Marinus Ghetaldus* mentioned in the beginning, which I here tranſcribe, with an account of ſome of their uſes, in his own words.

Ad

Ad comparandum inter se duodecim corporum genera, gravitate, & magnitudine Tabella.

	Aurum	Arg. vivum.	Plumb.	Argent.	Æs	Ferrum	Stann.	Mel	Aqua	Vinum	Cera	Oleum
Oleum	$20\frac{8}{11}$	$14\frac{62}{77}$	$12\frac{6}{11}$	$11\frac{3}{11}$	$9\frac{9}{11}$	$8\frac{8}{11}$	$8\frac{4}{55}$	$1\frac{32}{55}$	$1\frac{1}{11}$	$1\frac{4}{55}$	$1\frac{5}{121}$	1
Cera	$19\frac{19}{21}$	$14\frac{32}{147}$	$12\frac{1}{21}$	$10\frac{52}{63}$	$9\frac{9}{21}$	$8\frac{8}{21}$	$7\frac{89}{105}$	$1\frac{109}{210}$	$1\frac{1}{21}$	$1\frac{13}{420}$	1	
Vinum	$19\frac{19}{59}$	$13\frac{331}{413}$	$11\frac{41}{59}$	$10\frac{30}{59}$	$9\frac{9}{59}$	$8\frac{8}{59}$	$7\frac{31}{59}$	$1\frac{28}{59}$	$1\frac{1}{59}$	1		
Aqua	19	$13\frac{4}{7}$	$11\frac{1}{2}$	$10\frac{1}{3}$	9	8	$7\frac{2}{5}$	$1\frac{9}{20}$	1			
Mel	$13\frac{3}{29}$	$9\frac{73}{203}$	$7\frac{27}{29}$	$7\frac{11}{87}$	$6\frac{6}{29}$	$5\frac{15}{29}$	$5\frac{3}{29}$	1				
Stannum	$2\frac{21}{37}$	$1\frac{216}{259}$	$1\frac{41}{74}$	$1\frac{44}{111}$	$1\frac{8}{37}$	$1\frac{3}{37}$	1					
Ferrum	$2\frac{3}{8}$	$1\frac{39}{56}$	$1\frac{7}{16}$	$1\frac{7}{24}$	$1\frac{1}{8}$	1						
Æs	$2\frac{1}{9}$	$1\frac{32}{63}$	$1\frac{5}{18}$	$1\frac{4}{27}$	1							
Argentum	$1\frac{26}{31}$	$1\frac{68}{217}$	$1\frac{7}{62}$	1								
Plumbum	$1\frac{15}{23}$	$1\frac{29}{161}$	1									
Arg. viv.	$1\frac{38}{95}$	1										
Aurum	1											

Quæro, exempli gratia, quam habet rationem in gravitate plumbum ad aurum, Intelligatur plumbum, quoniam levius est auro, gravitatem habere 1, et in linea plumbi, in prima columna nominata, sub titulo auri, quæratur auri gravitas, ea erit $1\frac{15}{23}$. Plumbum igitur ad aurum rationem habebit in gravitate ut 1, ad $1\frac{15}{23}$. Si enim sumantur duo corpora magnitudine æqualia, unum plumbeum alterum aureum, sit autem plumbei corporis gravitas 1, aurei erit $1\frac{15}{23}$; quare corpus plumbeum ad corpus aureum ejusdem magnitudinis rationem habebit in gravitate ut 1,

 ad

ad 1[illegible]. Comparantur autem inter ſe genera diverſa gravitate, in corporibus magnitudine æqualibus.

Rurſus, quæro quam habet rationem in gravitate aqua ad argentum vivum. Intelligatur aqua, ut levior argento vivo gravitatem, habere 1, et in linea aquæ, ſub titulo argenti vivi, quæratur argenti vivi gravitas, ea erit $13\frac{4}{7}$; aqua igitur ad argentum vivum rationem habebit in gravitate ut 1, ad $13\frac{4}{7}$.

Contra, quæro quomodo ſe habent in magnitudine aurum et plumbum. Intelligatur aurum, quoniam gravius eſt plumbo, magnitudinem habere 1, et in linea plumbi, ſub titulo auri, quæratur plumbi magnitudo ea erit 1[illegible]; aurum igitur ad plumbum ſe habebit in magnitudine ut 1, ad 1[illegible]: ſi enim ſumantur duo corpora æque gravia, unum aureum, alterum plumbeum, ſit autem corporis aurei magnitudo 1, plumbei erit 1[illegible]; quare corpus aureum ad corpus plumbeum ejuſdem gravitatis ſe habebit in magnitudine ut 1, ad 1[illegible]. Comparantur autem inter ſe genera diverſa magnitudine, in corporibus æque gravibus.

Quæro denique, quomodo ſe habent in magnitudine ferrum, et aqua, ponatur ferrum, ut gravius aqua magnitudinem habere 1, et in linea aquæ, ſub titulo ferri, quæratur aquæ magnitudo, ea erit 8, ferrum igitur ad aquam ſe habebit in magnitudine ut 1, ad 8.

Altera,

Altera, ad comparandum inter se duodecim corporum genera, gravitate, et magnitudine, Tabella.

	Oleum	Cera	Vinum	Aqua	Mel	Stann.	Ferrum	Æs	Argent.	Plum.	Arg. viv	Aurum.
Aurum	$4\frac{27}{37}$	$5\frac{5}{209}$	$5\frac{10}{37}$	$5\frac{5}{19}$	$7\frac{12}{190}$	$38\frac{18}{89}$	$42\frac{2}{19}$	$47\frac{7}{19}$	$54\frac{22}{57}$	$60\frac{10}{19}$	$71\frac{3}{7}$	100
Arg. viv.	$6\frac{23}{37}$	$7\frac{7}{209}$	$7\frac{14}{37}$	$7\frac{7}{19}$	$10\frac{13}{19}$	$54\frac{16}{19}$	$58\frac{18}{19}$	$66\frac{6}{19}$	$76\frac{8}{57}$	$84\frac{14}{19}$	100	
Plumbum	$7\frac{62}{69}$	$8\frac{76}{253}$	$8\frac{38}{69}$	$8\frac{16}{23}$	$12\frac{19}{23}$	$64\frac{8}{23}$	$69\frac{13}{23}$	$78\frac{6}{23}$	$89\frac{59}{69}$	100		
Argent.	$8\frac{27}{31}$	$9\frac{81}{341}$	$9\frac{16}{31}$	$9\frac{21}{31}$	$14\frac{1}{31}$	$71\frac{19}{31}$	$77\frac{13}{31}$	$87\frac{3}{31}$	100			
Æs.	$10\frac{5}{27}$	$10\frac{20}{33}$	$10\frac{25}{27}$	$11\frac{1}{9}$	$16\frac{1}{9}$	$82\frac{2}{9}$	$88\frac{8}{9}$	100				
Ferrum	$11\frac{11}{23}$	$11\frac{41}{44}$	$12\frac{7}{24}$	$12\frac{1}{2}$	$18\frac{1}{8}$	$92\frac{1}{2}$	100					
Stannum	$12\frac{43}{111}$	$12\frac{166}{407}$	$13\frac{32}{111}$	$13\frac{19}{37}$	$19\frac{27}{37}$	100						
Mel	$63\frac{13}{87}$	$65\frac{265}{319}$	$67\frac{71}{87}$	$68\frac{28}{29}$	100							
Aqua	$91\frac{2}{3}$	$95\frac{5}{11}$	$98\frac{1}{3}$	100								
Vinum	$93\frac{13}{39}$	$97\frac{17}{649}$	100									
Cera	$96\frac{2}{63}$	100										
Oleum	100											

Quæro, exempli gratia, quænam ſit ratio in gravitate, auri ad argentum. Intelligatur aurum quoniam gravius eſt argento, gravitatem habere 100, et in linea auri, ſub titulo argenti, reperietur argenti gravitas $54\frac{22}{57}$, aurum igitur ad argentum rationem habebit in gravitate ut 100, ad $54\frac{22}{57}$. Si enim ſumantur duo corpora, magnitudine æqualia, unum aureum alterum argenteum, ſit autem aurei corporis gravitas 100, erit argentei

argentei $54\frac{22}{27}$; quare corpus aureum, ad corpus argenteum ejusdem magnitudinis, rationem habebit in gravitate, ut 100, ad $54\frac{22}{27}$.

Quæro, quomodo se habet in gravitate aqua ad vinum; quoniam aqua gravior est vino, intelligatur ejus gravitas 100, et quoniam in linea aquæ, sub titulo vini, datur vini gravitas $98\frac{1}{3}$, aqua ad vinum se habebit in gravitate, ut 100, ad $98\frac{1}{3}$.

Contra quæro quomodo se habent in magnitudine argentum, et aurum. Intelligatur argentum ut levius auro, magnitudinem habere 100, et in linea auri, sub titulo argenti, quæratur auri magnitudo, ea erit $54\frac{22}{27}$, argenteum igitur ad aurum se habebit in magnitudine, ut 100, ad $54\frac{22}{27}$. Si enim sumantur duo corpora æqua gravia, unum argenteum, alterum aureum, sit autem argentei corporis magnitudo 100, erit aurei $54\frac{22}{27}$; quare corpus argenteum, ad corpus aureum ejusdem gravitatis, se habebit in magnitudine, ut 100, ad $54\frac{22}{27}$.

Quæro denique, quomodo se habent in magnitudine aqua et argentum vivum. Quoniam aqua levior est argento vivo, intelligatur ejus magnitudo 100, et in linea argenti vivi, sub titulo aquæ, quæratur argenti vivi magnitudo, et reperietur $7\frac{7}{19}$, aqua igitur ad argentum vivum se habebit in magnitudine, ut 100, ad $7\frac{7}{19}$.

A letter from Robert Southwell, *Esq*; *to Mr* Henry Oldenburg, *concerning some extraordinary* Ecchoes, *lately communicated to the* R. S. *by the Rev.* Henry Miles, *D. D. & F.* R. S. No. 480. p. 219. May *and* June 1746, dated Kingsale, Sept. 19. 1661. *Read* June 5. 1746.

IX. I must needs account myself very happy, in that I partake so constant and fresh intelligence of the matters of the world; and that from so active a hand, as that I know no example of greater exactness and industry any where than what is with you.

I am very much rejoiced at the happy advancement of learning in the *Royal Society*; and that the radiant influence of His Majesty is like to smile upon it. And as to your query concerning sounds and ecchoes, I do remember, that the Duke of *Tuscany* * has made rare trials concerning the velocity in the motion of sound; and I gave Mr *Boyle*, in almost a sheet of paper, an Account and a Discourse upon those experiments, and the manner of them.

As for whispering places, the best I ever saw was that at *Gloucester*: but in *Italy*, in the way to *Naples*, two days from *Rome*, I saw, in an inn, a room with a square vault, where whispering, you could easily hear it at the opposite corner, but not in the least manner at the side corner that was nearer to you.

I saw another, in the way from *Paris* to *Lyons*, in the porch of a common inn, which had a round vault; but neither of these were comparable to that of *Gloucester*; only the difference between these two last was, that to this, holding your mouth to the side of the wall, several could hear you on the other side; the voice being more diffused. But, to the former, it being a square room, and you whispering in the corner, it was only audible in the opposite corner; and not to any distance from thence,

* See the Exp. of the Academy *del Cimento*.

as

as to distinction of the words. And this virtue was common to each corner of the room, and not confined to one.

As to ecchoes, there is one at *Brussels* that answers 15 times: but when I was at *Milan*, I took a coach to go two miles from thence to a nobleman's palace, now not in great repair, and only a peasant or contadine living in one end of it. The building is of some length in the front, and has two wings jetting forward; so that it wants only one side of an oblong figure. About 100 paces before the house, there runs a small brook, and that very slowly; over which you pass from the house into the garden. We carried some pistols with us; and, firing one of them, I heard 56 reiterations of the noise. The first 20 were with some distinction; but then, as the noise seemed to fly away, and answer at a great distance, the repetition was so doubled, as that you could hardly count them all; seeming as if the principal sound was saluted in its passage by reports on this and that side at the same time.

There were of our company that reckoned above 60 reiterations when a louder pistol went off; and indeed it was a very grateful divertisement. But on the other side the house, on the opposite wing, it would not sound; and only (to this advantage) in a certain chamber here two stories high from the ground.

CHAP. V.

HYDRAULICKS.

I. MR *Philip Williams*, chief engineer to our water-works at *Norwich*, a man of great ingenuity, who, in his time, has been author of many curious inventions, has contrived lately a machine for the raising of water to supply cities, drain marshy grounds, or other useful purposes, where no head of water can be procured, and the current runs very slowly: circumstances which render most other engines useless.

A Description of a Water-Wheel *for Mills, invented by Mr* Philip Williams. *In a letter from Mr* Wm. Arderon, *F. R. S. to Mr* Baker, *F. R. S.* No. 478. p. 1. Jan. & Feb. 1746. dated *Norwich, May* 30. 1745. *Read* Jan. 9. 1745-6.

With his leave, I now send you a drawing of this machine, which I shall endeavour to explain in a manner to be understood.

The *axis* of the first mover is cut into the form of an hexangular prism, of dimensions suitable to the force requir'd, as is represented by the letter *A*. Into this, several sets of holes are mortised, as *B B B*. These are intended to receive different sets of sails made of iron plates, one whereof is represented in Fig. 54. all which sails are weathered in the same manner as those designed for windmills; only in these the extremity of their ends stands parallel to the planes of each end of the *axis*, those ends I mean which are placed farthest from the centre. Fig. 53.

This hexangular *axis*, when employed, must be placed parallel to the moving stream, and may lie even with its surface: but the engine will act Fig. 54.

most

most vigorously, when it and all the sails employed are entirely under water, as is easy to comprehend. Each set of the sails before described contains six in number, and are so contrived as to be put in and taken out at pleasure; whence it follows, that when a single set of sails is made use of, the engine produceth a single effect, when two sets a double, and so on, till the desired *momentum* is acquired, with the same quantity of running water, provided there be room to fix a sufficient number of sails.

It is farther to be observed, that when this engine is placed with it's sails made and weathered as above directed, they will move with equal velocity, even supposing the current should change it's course, and come upon them in a quite contrary direction, as the case really happens in rivers where the tide ebbs and flows; where most other engines yet invented are of little service.

About six weeks ago I had the pleasure to see a model of this engine tried. It was fixed in our river, in a place where the water moved only 27 feet in 20″ in which time the first mover made six revolutions. It's diameter was no more than two feet and two inches; yet it would have lifted 14 pounds two yards high in the above-mentioned time, had not a misfortune happend to it's case which made it not perform quite so much.

It appeared to me somewhat extraordinary, that the circumference of it's first mover (I mean any determined part thereof) passed through a space of 42 feet in 20″; which is nearly twice as fast as the motion of the water: and as the *momentum* will be in proportion to the number of the sets of sails that are employed, it's force is capable of being greatly augmented with the same quantity of water: a thing not to be admitted without sufficient experiment, but what seems extremely plain in Theory, and what I am apt to think will answer when brought to Practice.

This engine, when once seen, requires little skill for the construction of it, is made at a small expence, and kept in repair with ease.

A description of a Clepsydra, *or Water-Clock; by the Hon.* Charles Hamilton, *Esq;* N°. 479. p. 171. Mar. *and* Apr. 1746. *Read* April 24. 1745-6. Fig. 55. *The Machine in perspective.*

II. An open canal *e e*, is supplied with a constant and equal stream by the siphon *d*; and has at each end *f f*, open pipes, of exactly equal bores, which deliver the water that runs along the canal *e*, alternately into the vessels *g* 1, *g* 2, in such a quantity as to raise the water from the mouth of the *tantalus s*, to the top of the *tantalus t*, exactly in an hour. The canal *e e*, is equally poised by the two pipes *f* 1, *f* 2, upon a centre *r*; the ends of the canal *e*, are raised alternately, as the cups *z z*, are depressed, to which they are connected by lines running over the pulleys *l l*. The cups *z z*, are fixed at each end of the balance *m m*, which moves up and and down upon it's centure *v* *.

* The *letters* of reference answer to Fig. 55, 56, *and* 57. some being seen in one, that do not come in sight in the others.

n 1,

n 1, *n* 2, The edges of two wheels or pulleys, moving different ways alternately, and so fitted to the cylinder *o* (by oblique teeth both in the cavity of the wheel, and upon the cylinder; which, when the wheel *n* moves one way [*i. e.* in the direction of the minute-hand], meet the teeth of the cylinder, and carry the cylinder with it; and, when *n* moves the contrary way, slip over those of the cylinder, the teeth no more meeting, but receding from each other; or it may be done by catches or locks which require a longer description), one or other of these wheels, *n n*, continually moves *o* in the same direction, with an equal and uninterupted motion: for the contrivance is such, that the instant one ceases to act, the other begins, and so on.

A fine chain goes twice round each wheel, having at one end a weight, *x*, always out of water, which equiponderates with *y* at the other end, when kept floating at the surface of the water in the vessel *g*, which *y* must always be. The two cups *z z*, one at each end of the balance *m m*, keep it in *equilibrio*, till one of them is forced down by the weight and impulse of the water, which it receives from the *tantalus s t i*: each of these cups *z z*, has likewise a *tantalus* of it's own *h h*, which empties it after the water has done running form *g*, and leaves the two cups again *in equilibrio*; *q* is a drain to carry off the water.

Fig. 56. represents the dial-plate, with the hour and minute-hands, the weight and float belonging to *n* 2. The front of the *tantalus* in *g* 2, marked *s t i*, of which *s* the mouth is 18 inches above the bottom of the vessel *g*, and 18 inches below the top of the *tantalus t*. *i* is the issuing leg of the *tantalus*, which discharges the water out of the vessel *g* into the cup *z*, as soon as it runs over the top *t*, till the water sinks as low as *s*.

Fig. 56. *The front of the* Clepsydra.

Fig. 57. *The profile of the* Clepsydra.

The case *u u* incloses the whole machine, except the cistern that supplies the siphon *d*, which may be placed at any distance from it, as is most convenient, provided the issuing leg *d*, of the siphon is lengthened out so as to give a constant stream into the canal *e*. This case *u u* supports the *axis* of the cylinder *o* behind, and the dial-plate *p p* before; in the centre of which turns the *axis o*, with the index *k* at it's extremity, being the minute-hand. The hours may be described by two common wheels, as in ordinary clock-work. For cheap work, chains passing round pulleys would do instead of wheels with teeth.

Fig. 58. *The plan of the* Clepsydra *to it's full dimension.*

The short leg of the siphon *d* is placed in a cistern, with it's mouth something below the mouth of the waste-pipe; which cistern is supplied with a constant stream, rather more than runs out at the siphon *d*; which overplus going off at the waste-pipe, the water always remains at the same height in the cistern, and yet always delivers a constant and equal flow into the canal *e e*; consequently, there is not the least intermission. As the end of the canal *e*, fixed to the pipe *f* 1, is in the figure the lowest, the water runs all through the pipe *f* 1, into the vessel *g* 1, till it runs over the top of the *tantalus t*; when it immediately runs out at *i* into the cup *z*, at the end of the balance *m*, and forces it down, the balance *m* moving on it's centre *v*. When one side of *m* is brought down, the string which connects

The motion of the Clepsydra *is effected in the following manner.*

 it

it to f 1, running over the pulley l, raiſes the end f 1, of the canal e, (which turns upon it's center r) higher than f 2; conſequently, all the water which conſtantly runs through the ſiphon d, inſtantly runs through f 2 into g 2, till the ſame operation is performed in that veſſel, and ſo on alternately.

As the height the water riſes in g in an hour, *viz.* from s to t, is equal to the circumference of n, the float y riſing that height along with the water, lets the weight x act upon the pulley n, which carries with it the cylinder o; and, giving a revolution, makes the index k deſcribe an hour upon the dial-plate. This revolution is performed by the pulley n 1; the next is to be by n 2, whilſt n 1 goes back, as the water in g 1 runs out through the *tantalus*; for y muſt follow the water, as it's weight increaſes out of water.

The *axis o* always keeps moving the ſame way; the index k deſcribes the minutes; the *tantalus's* muſt be wider than the ſiphon d, that the veſſels g g may be ſure to be empty as low as s, before the water returns to them.

CHAP. VI.

GEOGRAPHY and *NAVIGATION.*

Obſervations determining the Longitude of Kingſton *in* Jamaica, *by Mr* Ja. Short. *F. R. S.* N°. 496. p. 523. Nov. *&c.* 1750. *Read* Nov. 1. 1750.

I. I Take this opportunity of laying before the *Society*, two obſervations proper for determining the difference of longitude between *London* and *Kingſton*, which came to my hands ſome time ſince. They were made by *Alex. Macfarlane*, Eſq; of *Kingſton*, *F. R. S.* who is provided with a compleate *apparatus* of aſtronomical inſtruments, which he purchaſed of *Colin Campbell*, Eſq; As this gentleman is well verſed both in the Theory and Practice of Aſtronomy; I think the following obſervations may be depended on for fixing the longitude of *Kingſton*; eſpecially as we have the ſame obſervations made at the houſe of Mr *G. Graham* in *Fleetſtreet*, *Lond.* *

Eclipſe *of the* Moon, Oct. 22, 1743.

	h	′	″
Beginning of total darkneſs at - - - - - - - - - -	9	10	58
End of the eclipſe at - - - - - - - - - - - - - -	10	51	33

Tranſit of Mercury *over the* Sun, Oct. 25, 1743.

The ingreſs of *Mercury* upon the ſun could not be ſeen; the ſun being then below the horizon.

Exceſſus e diſco Solis, or the laſt exterior contact, at $7^h\ 56'\ 43''$ *a. m.*

By the firſt obſervation of the eclipſe of the moon, compared with the ſame eclipſe obſerved here, *Kingſton* is found to be $5^h\ 6'\ 2''$ to the weſt of *London*.

* See Vol. VIII. p. 172 and 202.

And

Pla. X. Vol. X. Part I. Page 250.

Fig. 57.

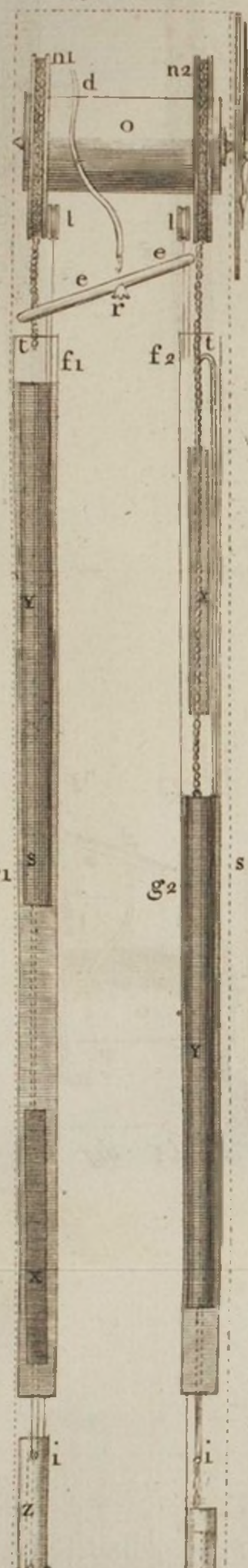


Fig. 55.

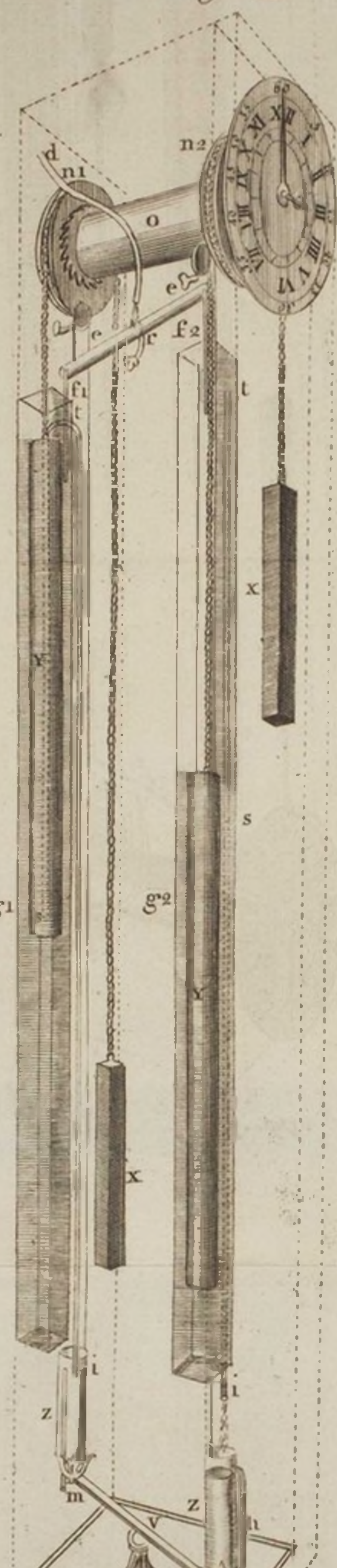


Fig. 56.

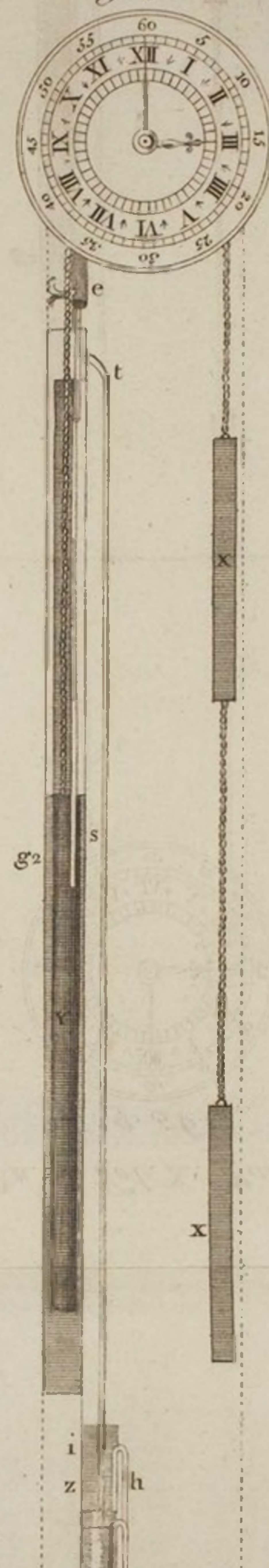


Fig. 53.

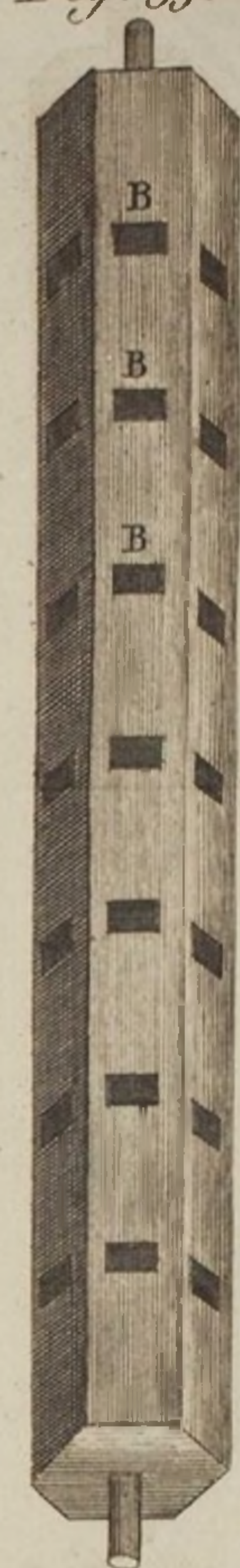


Fig. 54.

3. 6. 9. 12.

Scale of twelve Inches.

And by the *Transit* of *Mercury* neglecting his parallax, *Kingston* is found to be 5° 5′ 33″.

This last is the most to be depended on for settling the longitude of *Kingston*; because in all observations of an eclipse of the moon, an error of a minute or two may be allowed, arising from the indistinctness of the penumbra.

Extract of a letter from Mr Leonard Euler, *Prof. Math. and Member of the* Imp. Soc. *at* Petersburgh, *to the Rev.* Mr Cha. Wetstein, *Chaplain and Sec. to his R. Highness the* Prince *of* Wales, *concerning the* Discoveries *of the* Russians *on the* N. E. Coast *of* Asia. N°. 482. p. 421. Jan. *and* Feb. 1747. *dated* Berlin, Dec. 10. 1746. *Read* Feb. 5. 1746-7.

II. As you are desirous to hear something more particular concerning the *Russian* expeditions to the North and North-East of *Asia*, I will here give you an account of all that has come to my knowledge relating to the same. But as I should, on the one hand, be very glad that these observations might give any light concerning the passage now sought through *Hudson's Bay*, I should, on the other be very sorry, if Mr *Behring's* opinion, who believed that the new land he had discovered was joined to *California*, should rather lead us to doubt of the success of that glorious undertaking. I wish, however, that a happy experiment may soon inform us certainly of the truth. In the mean time you will not be sorry to be acquainted with the reasons upon which Mr *Behring's* suspicions were founded, notwithstanding the objections you have been pleased to make, and to communicate to me upon that head.

First, This new land, which he fell in with at the distance of 50 *German* miles from *Kamschatka* towards the east, was followed by him, and coasted for a great way, though I cannot say how far: from whence alone it will appear, that an abatement must be made in the distance of 30°, or thereabouts, which you suppose to be between the last known head-land of *California* towards the west, and the farthest extremity of this new discovered land towards the east.

Secondly, Capt. *Behring* having had the opportunity of observing an eclipse of the moon at *Kamschatka*, concluded from the same, that that place lay much farther off to the east, than is expressed in any map; and that, to represent it truly, it ought to be transferred into the other hemisphere, as it's longitude is more than 180 degrees [E. from the Isle of *Ferro*]. For this reason Captain *Behring's* new land will be considerably approached to the last known part of *California*, and will not indeed appear to be many degrees from it.

What we have therefore still to hope is only, that in this unknown district there may be found some streight, by which the *pacific sea* may freely communicate with *Hudson's Bay*; but if it shall appear that there is no such passage, it must then be concluded, that whatever further progress may happen to be made through *Hudson's Bay*, the opening at last must only be into the *frozen sea*, from whence there could be no passing into the *pacific ocean*, but by the neighbourhood of *Kamschatka*; and this way would without doubt be too long, and too dangerous, to be master'd in the course of one summer.

I very much doubt whether the *Russians* will ever publish the particulars of their discoveries, either such as have been made from *Kamschatka* towards

America, or ſuch as have been made upon the northern coaſts of *Aſia*. And indeed it is but very much in general that I know the ſucceſs of this laſt expedition. What I do was communicated to me by order of the Court, from the College of Admiralty, for me to make uſe of it in the Geography of *Ruſſia*, which I was at that time charged with.

They paſſed along in ſmall veſſels, coaſting between *Nova Zembla* and the Continent, at divers times, in the middle of ſummer, when thoſe waters are open. The firſt expedition was from the river *Oby*; and at the approach of winter the veſſels ſhelter'd themſelves by going up the *Jeniſka*; from whence the next ſummer they returned to ſea, in order to advance further eaſtward; which they did to the mouth of the *Lena*, into which they again retired for the winter-ſeaſon.

The third expedition was from this river, to the fartheſt North-Eaſt cape of *Aſia*. But here they loſt ſeveral of their boats, and a great part of their crew, ſo as to be diſabled from proceding, and from making the whole tour, ſo as to arrive at *Kamſchatka*.

It was however thought, that a further attempt was then unneceſſary, becauſe Captain *Behring* had already gone round that cape, ſailing northward from *Kamſchatka*.

The *Ruſſians* have not attempted the paſſage round *Nova Zembla*; but as they have paſſed between that land and the coaſt of *Aſia*, and as the *Dutch* did formerly diſcover the northern coaſts of *Nova Zembla*, we may now be well aſſured, that that country is really an iſland.

A letter from Arthur Dobbs, *Eſq;* *of* Caſtle-Dobbs *in* Ireland, *to the Rev Mr* Cha Wetſtein, *Chap. and Se: to the Prince of* Wales, *concerning the diſtances between* Aſia *and* America N° 483. p. 471. Mar. &c. 1747. *Read* April 9. 1747.

III. I am extremely obliged to you for the trouble you have taken, in correſponding with Prof. *Euler* * upon the *Ruſſian* diſcoveries eaſtward from *Kamſchatka*, and communicating to me the accounts he had of *Behring*'s laſt voyage, and of his diſcovery of the lands N. E. of *Japon*; which the Prof. could only have inaccurately, not having ſeen any journal to fix the Lat. and Long. of the countries he then diſcover'd: but ſince Prof. *Euler*, ſway'd by the opinion of Captain *Behring*, ſeems ſtill to believe that the laſt land he diſcover'd is joined to *California*, which country is now known to be part of the continent of *America*, and not an iſland (in which fact of it's being continuous to *California* I differ ſtill in opinion from him) for, if that were a fact to be depended upon, I would candidly own, that there could be no paſſage from the N. W. of *Hudſon's Bay* to the weſtern ocean of *America*, without ſailing near 70° of Long. the diſtance of the N. E. cape of *Aſia* from the N. W. of *Hudſon's Bay*, in a parallel almoſt as far N. as the polar circle, before the paſſage can be made to the *pacific ocean*; which might therefore be very reaſonably call'd an impracticable paſſage, as it could not poſſibly be made in one ſummer, (if at all) and ſince Prof. *Euler* has been ſo kind as to give me Capt. *Behring*'s reaſons for ſupporting his opinion, which are principally from the ſmall diſtance he ſuppoſed it was from the the coaſt he diſco-

* See the preceding article.

vered,

vered, to the weſtern *American* coaſt at *California* (which he imagined was much nearer his N. E. cape of *Aſia* than it is in fact); I muſt therefore, in return to the Profeſſor's goodneſs, in communicating to me all he has known in that diſcovery, beg leave to give you this further trouble of communicating to the Profeſſor my reaſon for ſtill diſſenting from *Behring*'s opinion, that the land he diſcovered laſt was part of the continent of *America*, or continuous with *California*; and if he find the reaſons for ſupporting my opinion make it more probable, that there ſtill may be a large opening betwixt theſe new-diſcovered countries and *California*, I am ſenſible it will give the ingenious and learned Profeſſor great pleaſure, to think we may yet hope for a paſſage by *Hudſon's Bay* to the weſtern *American* ocean, without being obſtructed with ice after paſſing *Hudſon's Streight*.

The Profeſſor imagines I might have been led aſtray, by not conſidering, that the N. E. cape of *Aſia* is much more eaſterly than has been laid down in any former charts; which is now known accurately, by the eclipſe of the moon obſerved by Captain *Behring* at *Kamſchatka*.

I have an abſtract of his Journal by me, upon his firſt diſcovery in 1728, and 1729, when he obſerved that eclipſe, and the calculation of the long. from it; and ſtand by his long. he has fixed; and allow that his N. E. cape is in the other hemiſphere; reckoning eaſtward, either from *Fero*, as the firſt meridian, or from *London*; which laſt I ſhall follow.

Behring fixes his N. E. cape 126° 7′ E. long. from *Tobolſki*; and *Tobolſki* is 86° E. from *Fero*; ſo the cape is 212° 7′ E. of *Fero*, or about 194° E. from *London* —— By Captain *Middleton*'s obſervation of *Jupiter*'s *Satellite* at *Churchill* river in *Hudſon's Bay*, that river is 95° W. from *London*; which, added to 194°, makes 289°; conſequently the N. E. cape of *Aſia* is 71° diſtant from *Churchill*, to complete 360°; which, in the lat. of 65°, computing 8 leagues to a degree of long. of which 20 make a degree of lat. the diſtance betwixt that cape and *Hudſon's Bay* would be 568 ſuch leagues.

From the known long. of the N. cape of *Japon* in 40° lat. which is pretty exactly known, from the obſervations made by the Jeſuits at *Peking*, and is about 150° E. from *London*, and from the beſt computed long. of *California* in 40° N. lat. it lies in 130° long. W. from *London*, making together 280°, leaves 80° for the diſtance of *California* from *Japon*; allowing 17 leagues to a degree of long. in 40° N. lat. the diſtance would be about 1360 leagues: by the ſame calculation *California* muſt be at leaſt 7 or 800 ſuch leagues from the N. E. cape of *Aſia*; ſo that, in ſo great a ſpace there may be very great countries or iſlands *, without ſuppoſing the new diſcovered country continuous to *California*,

* The *Japoneſe*, in their maps of the World printed in *Japon*, have laid down in this very tract two iſlands as large as *Ireland*, with the names to them, as appears in that map bought by Dr *Kempſer* in *Japon* in 1686; now in Sir *Hans Sloane*'s Muſeum. *C. M.*

and

and might well allow of an open channel or sea, from 50 to 100 leagues wide, between the discovered coast and *California*.

By the account given to Prof. *Euler*, *Behring* sailed southwardly to the isles of *Japon*, and from thence sailed eastwardly 50 *German* miles, about 250 *English* miles; which makes about 80 leagues, of 20 to a degree. At that distance from *Japon* he discovered land, which he coasted N. W. still approaching towards the N. E. cape, without going ashore, until he came to the entrance of a great river; where sending his boats and men ashore, they never returned, being either lost, killed, or detained by the natives, which made his discovery incomplete; his ship being stranded, and he afterwards died in an uninhabited island.

As no lat. nor long. are fixed by this account, I must believe he sailed from *Kamschatka* S. E. perhaps more southerly than to 50° lat.; and there found land N. E. from *Japon*; otherwise, by coasting it N. W. he could never approach the N. E. cape, which is, at least 40° long. E. of *Japon*; and if he made land 80 leagues E. of *Japon*, he must have sailed N. E. to make the N. E. cape. I have therefore reason to believe this coast was part of that he saw in his first voyage, where he lost his anchor; and is the coast *Gama* discovered, and the *Dutch* afterwards called the *Company's Land*, E. of the streights of *Uzicez*, which is at least 7 or 800 leagues W. of any known land of *America*, and above 1000 near the lat. of *Japon*: so that, if I should allow 700 leagues for countries or islands E. of his new-discovered coast, there might still be a passage of 100 leagues for the southern or *pacific ocean* to communicate with *Hudson's Bay*, and to cause such great tides and currents, as are found on the N. W. of *Hudson's Bay*; as also a free passage for the whales, which are seen in all the openings N. W. of that bay, and are caught there in numbers by the *Eskemaux* savages: for, as these don't go in by *Hudson's Streight* from our *Atlantic Ocean*, it cannot be presumed that they should go up by *Japon* towards the N. E. cape, and from thence go 70°, or above 560 leagues, to *Hudson's Bay*, and be there in *June*, and, after staying until *Sept.* return again the same way to the southern ocean, to pass the winter. — Now, as *Behring* only coasted at a distance, he could not possibly know whether it was a continent, or great island; the last of which seems the most probable: however, a few months now, if our ships return safe, will give us a certainty on one side or the other; altho' I am sanguine enough to believe they have by this time sailed thro' and discovered this so much wished for passage.

These, Sir, are the reasons I have still to expect success in the attempt I have promoted; and, if you think it may give any satisfaction to Prof. *Euler*, to know the reasons that support my belief of a practicable safe passage, be pleased to communicate it to him, with my compliments for the trouble I have given him by you, and accept of my best acknowledgments for your favours.

IV. It

A letter from F. Anth. Gaubil, *Jeſ. to Dr* Mortimer, *Secr. R. S. containing ſome account of the knowledge of Geography among the* Chineſe. *Tranſlated from the* French *by* T. S. M. D. *and F. R. S.* N°. 494 p. 327. Jan. *&c.* 1750. *dated* Peking *Nov.* 9. 1748. *Read* Feb. 1. 1749.

IV. It is now ſome time ſince I received from M. *de L'iſle* part of a map of the world, found among the papers of the late Dr *Kæmpfer*. In this map were ſeveral *Chineſe* characters, ſome well, ſome ill written, which the late Prof. *Bayer* had attempted to decypher. —— In my anſwer to M. *de L'iſle*, I informed him that it was by no means a *Chineſe* work*; that it could be of no ſervice to a learned *European*, ſuch as he or you were; and that Mr *Bayer*'s explanations were full of faults. I ſuppoſe that M. *de L'iſle* has already writ you my thoughts concerning it from *Peterſbourg*. You have poſſibly ſeen in ſeveral books, what the *Chineſe* know, and have ſet down, concerning foreign countries: and there is no monument extant to prove, that before the arrival of the Jeſuits in this country, they had charts or maps of the world, any way reſembling that, which you found among *Kæmpfer*'s writings.

It is now above 1600 years ſince they tolerably well knew the northern and eaſtern countries of *India*, and thoſe which lie between *China* and the *Caſpian* ſea. On theſe different countries their hiſtory affords ſeveral informations, which are not to be found in the *Greek*, *Latin*, or other hiſtorians. They had ſome, but very confuſed, notions of the regions beyond the *Caſpian* ſea; ſuch as *Syria*, *Greece*, *Egypt*, and ſome parts of *Europe*. I do not ſpeak of the times of *Gentchiſkan* and his ſucceſſors; for then the *Chineſe* were made acquainted with *Ruſſia*, *Poland*, *Germany*, *Hungary*, *Greece*, *&c.* from accounts given by their own contrymen who followed that prince, his ſons, and grandſons: but the monuments that remain of this their knowledge are very confuſed. As to the countries to the eaſt of *China*, there are proofs remaining in books, that, above 1700 years ago, the *Chineſe* were well acquainted with the eaſtern part of *Tartary* as far as the ſea, and the river *Ameur*, *Corea*, and *Japan*. Their books ſpeak alſo in general, and without ſufficiently entering into particulars of many countries to the E. and to the N. of *Japan*. With regard to the monuments of the *Cape of Good Hope*, which have been mentioned by ſome, there are none in *China*; and if there have been any, they are now loſt. It was from the *Europeans*, that the *Chineſe* have learnt the name and the ſituation of the *Cape* [and you will ſoon ſee a Diſſertation, wherein all this affair will be circumſtantially treated].

A letter from Mr John Robertſon *to the* Preſ. *containing an explanation of the late Dr* Halley*'s demonſtration of the Analogy of the* Logarithmic Tangents *to the* meridian line, *or ſum of the* Secants.

V. My curioſity having lately led me to peruſe ſeveral books on the art of Navigation, I was ſomewhat ſuprized not to find in any one of them a clear explanation of that moſt curious paper † written by that excellent Mathematician Dr *Halley*; who, not intending to write for beginners, as himſelf confeſſes, has drawn his concluſions in a manner, that ſeems to ſtand in need of an explanation, for the generality of readers: and as the maritime people are not the beſt acquainted with mathematical knowledge, it

* Doubtleſs it is the work of an *European*, who was giving ſome notion of Geography to a *Chineſe* or *Japaneſe*; or, perhaps, that of a *Chineſe* or *Japaneſe* from memory of what he had heard from *Europeans*, or of the map which he might have ſeen with them.

† See Vol. I. Chap. vii. §. 38.

N°. 496 p. 559 Nov. *&c.* 1750. *Read* Nov. 22. 1750.

it might have been expected, that such of the writers on Navigation within the last 50 years who have undertaken to demonstrate the several parts of their subject, would have removed the difficulties in the Doctor's paper, instead of leaving them in the same state in which they first appeared.

Dr *Halley*, in this tract, seems to have had two chief points in view; first, to prove that *the divisions of the meridian line in a Mercator's chart, were analogous to the logarithmic Tangents of the half-complements of the latitudes.* And secondly, *To find a rule by which the tables of meridional parts might be computed from* Briggs'*s, or the common logarithmic Tangents.* The former of these the Doctor has clearly and elegantly proved: but he has given rather too few steps to shew us clearly the investigation of the latter.

Indeed in many of the treatises on Fluxions, it is shewn how to investigate a rule to find the meridional parts to any latitude: but, to understand those methods, requires some skill in algebraical and fluxionary computations; neither of which are necessary in this business, by keeping to the Doctor's principles, as will be evident from the following articles; some of which are already well known; yet it was thought convenient to annex them to this discourse.

Article I. *If the circumference of a circle be divided into any number of equal parts by as many* radii, *and a line be drawn from the circumference cutting those* radii, *so that their parts intercepted between this line and the centre be in a continued decreasing geometric progression; then will that interfecting line be a curve, called the proportional spiral, and will intersect those* radii *at equal angles.*

This will be evident, by supposing the *radii* so near to one another, that the intercepted parts of the spiral may be taken as right lines; for then there will be a series of similar triangles, each having an equal angle at the centre, and the sides about those angles proportional.

Article II. *The same things still supposed, the parts of the circumference of the circle, reckoned from any one point, may be taken as the logarithms of the* ratio's *between the corresponding rays of the spiral.*

For those rays are a series of terms in a continued geometric progression; and the parts of the circumference from a series of terms in arithmetic progression. Now the terms of the arithmetic series being taken as the exponents of the corresponding terms in the geometric series, there will be the same relation between each geometric term and it's correlative, as between numbers and their logarithms. And hence the proportional spiral is also called the logarithmic spiral.

Article III. *That proportional spiral, which intersects it's* radii *at angles of 45° produces logarithms that are of* Napier'*s kind.*

For,

For, if the difference between the firſt and ſecond terms in the geometric ſeries was indefinitely ſmall, and the firſt diviſion of the circumference was of the ſame magnitude; then may that part of the ſpiral, intercepted between the firſt and ſecond *radii*, be taken as the diagonal of a ſquare, two of whoſe ſides are parts of thoſe *radii*: therefore the ſpiral which cuts it's rays at angles of 45°, has a kind of logarithms belonging to it, ſo related to their correſponding numbers, that the ſmalleſt variation between the firſt and ſecond terms in the geometric ſeries, is equal to the logarithm of the ſecond term, a cypher being taken for the logarithm of the firſt. But of this kind are the hyperbolical logarithms, or thoſe firſt made by their inventor the Lord *Napier*: conſequently the logarithms to that ſpiral which cuts it's rays at angles of 45°, are of the *Napierian* kind.

The Rhumb-lines on the globe are analogous to the logarithmic ſpiral. Article IV.

For every oblique rhumb cuts the meridian at equal angles: and it is a property in ſtereographic projections, that the lines therein interſecting one another, form angles equal to thoſe which they repreſent on the ſphere. Therefore a projection of the ſphere being made on the plane of the equator, the meridians will become the *radii* of the equator, and the rhumbs interſecting them at equal angles, will become the proportional ſpiral.

Hence, the arcs of the equator, or the differences of long. reckoned from the ſame merid. are as the logarithms of thoſe parts of the correſponding meridians, intercepted between the centre and rhumb-line.

A Sea Chart being conſtructed, wherein the meridians are parallel to one another, and the lengths of the degrees of latitude increaſe in the ſame proportion as the meridional diſtances decreaſe on the globes, will conſtitute a Mercator's chart; wherein, beſides the poſitions of places having the ſame proportions to one another as on the globes, the rhumb lines will be repreſented by right lines. Article V.

For none but right lines can cut at equal angles ſeveral parallel right lines.

The diviſions of the meridian line on a Mercator's chart, are the ſame as a table of the differences of long. anſwering to each minute, or ſmall difference of lat. on the rhumb line making angles of 45° with the meridians. Article VI.

For, in ſuch a chart, the parallels of lat. are equal to the equator, and are at right angles to the meridians: and therefore a rhumb of 45° cuts the meridians and parallels of latitudes at equal angles; conſequently between the interſection of any meridian and parallel, and a rhumb cutting them at 45°, there muſt be equal parts of the meridian and parallel intercepted:

tercepted: now, on the equator, or parallels of lat. are reckoned all the successive differences of longitudes, and on the meridians the successive meridional differences of latitudes, or the divisions of the nautical merid.: therefore on the rhumb of 45°, the successive differences of long. are equal to the corresponding divisions of the nautical merid.

Article VII. *The tangents of the angles which different rhumbs make with the meridians, are directly proportional to the differences of longitudes made on those rhumbs, when the meridional differences of latitudes are equal; or, are reciprocally proportional to unequal meridional differences of latitudes on those rhumbs, when the differences of longitudes are equal.*

For the meridional difference of lat. is to the diff. of long.; as *radius* is to the tangent of the angle of the course, or of the angle which the rhumb makes with the merid. therefore, when the meridional differences of latitudes are equal, the differences of longitudes are as the tangents of the courses: but, when the differences of longitudes are equal, the meridional differences of latitudes are reciprocally as the tangents of the courses.

Article VIII. *The logarithmic tangents of the half-complements of the latitudes, are analogous to the lengthened degrees in the nautical merid. line, in a Mercators's chart.*

For, in the stereographic projection of the sphere on the plane of the equator, the latitudes of places are projected by the half-tangents of the complements of those latitudes, which half-tangents are the rays of a proportional spiral: now, if a series of successive latitudes be taken on any rhumb, the corresponding differences of longitudes will be logarithms to the rays of the spiral, or to the tangents of the half-complements of those latitudes: therefore the differences of longitudes are as the logarithmic tangents of the half-complements of the latitudes: but (*Art.* VI.) the lengthened degrees on the nautical merid. are as the differences of longitudes on the rhumb of 45°; consequently the logarithmic tangents of the half-complements of latitudes are as the lengthened degrees on the nautical merid.

Corol. 1. When the angle between the rhumb line and the merid. is equal to 45°, then the longitudes of places on that rhumb are expressed by logarithms of *Napier*'s kind; whose corresponding numbers are natural tangents of the half-complements of the latitudes to arcs expressed in parts of the *radius*.

Corol. 2. Hence, to any two places on a rhumb of 45°, the difference of long. or the meridional diff. of lat. is equal to the diff. of the *Napierian* logarithmic tangents of the half-complements of the latitudes of those places, estimated in parts of the *radius*.

As

As there may be an indefinite variety of rhumbs, and therefore as many different kinds of logarithms, consequently every species of logarithms has it's peculiar rhumb, distinguishable by the angle it makes with the merid.: therefore, among these there are two kinds, whereto the differences of longitudes are the differences of the logarithmic tangents of the half-complements of latitudes, estimated in minutes of a degree; one of them belonging to *Napier*'s form of logarithmic tangents, and the other to *Briggs*'s, or the common logarithmic tangents. *Corol.* 5.

The common logarithmic tangents are a table of the differences of longitudes, to every minute of lat. on the rhumb line making angles with the meridians of 51° 38′ 9″. Article IX.

For, let z represent the merid. diff. of lat. between two places on the rhumb of 45°; or it's equal, the difference between the logarithmic tangents of the half-complements of the latitudes of those places, estimated either in parts of the *radius*, or in minutes of a degree. Then,

As the circumference in parts of the *radius* = 62831,853, *&c.*

To the circumference in minutes of a degree = 21600.

So is a meridional diff. of lat. in parts of the *radius* = z.

To a merid. diff. of lat. in minutes of a degree, = 0,34377468, *&c.* × z.

Whose corresponding rhumb is different from that which z belonged to; and the angle which this rhumb makes with the merid. will be found by the following analogy from *art.* 7.

As the meridional diff. of lat. on one rhumb = 0,34377468, *&c.* z.

To the merid. diff. of lat. on a rhumb of 45°, = z.

So is the natural tangent of the rhumb of 45°, = 10000.

To the natural tangent of the other rhumb, = 29088,821, *&c.*

Which tangent answers to 71°. 1′ 42″; and this is the angle that the rhumb line makes with the meridians, on which the differences of the logarithmic tangents of the half-complements of the latitudes, in *Napier*'s form, are the true differences of longitudes estimated in sexagesimal parts of a degree.

Now *Napier*'s logarithms being to *Briggs*'s as 2,30258, *&c.* is to 1.

Therefore, 2,30258, *&c.* : 1 :: 29088,821, *&c.* : 12633,114, *&c.*; which is the tangent of 51° 38′ 9″; and in this angle are the meridians intersected by that rhumb, on which the differences of *Briggs*'s logarithmic tangents of the half-complements of the lat. are the true differences of longitudes corresponding to those latitudes.

The diff. between Briggs's *logarithmic tangents of the half-complements of the latitudes of any two places, to the merid. diff. of lat. in minutes between those places, is in the constant ratio of* 1263,3, *&c.* to 1; or of 1 to 0,0007915704, *&c.* Article X.

For *Briggs*'s logarithmic tangents are as the differences of longitudes on the rhumb (A) of $51° 38' 9''$; whose natural tangent is 1263,3, &c.

The nautical meridian is a scale of longitudes on the rhumb (B) of 45° by *Art.* VI. whose tangent being equal to the *radius*, may be expressed by unity. And the differences of long. to equal differences of latitudes on different rhumbs, being to each other as the tangents of the angles those rhumbs make with the meridians. Therefore,

As the tangent of A ($51° 38' 9''$) = 1,2633, &c.

To the tangent of B (45°) = 1,0000;

So is the difference of long. on A, or the diff. between the logarithmic tangents of the half co-latitudes of two places

To the diff. of longitudes on B, or the merid. difference of latitudes of those places.

And hence arise the rules which are given in nautical works, for finding the meridional parts by a table of common logarithmic tangents.

This curious discovery of Dr *Halley*'s, joined to that excellent thought of his, of delineating the lines, shewing the variation of the compass on the nautical chart, are some of the very few useful additions made to the art of navigation within the last 150 years: for if beside these, we except the labours of that ingenions artist Mr *Richard Norwood*, who improved the art by adding to it the manner of sailing in a current, and by finding the measure of a degree on a great cicle, the Theory of Navigation will be found nearly in the same state in which it was left by that eminent mathematician Mr *Edward Wright*; who, about the year 1600, published the principles on which the true nautical art is founded; and shewed, what does not appear to have been known before, how to estimate a ship's true place at sea, as well in long. as in lat. by the use of a table of meridional parts, first made by himself, and constructed by the constant addition of the secants, and which differs almost insensibly from such a table made on Dr *Halley*'s principles, contained in the preceding articles.

I shall conclude this discourse with an article, which altho' it be somewhat foreign to the preceding subject, yet, as it was discover'd while I was contemplating some part thereof, and perhaps is not exhibited in the same view by others, it may not be improper to annex it in this place: which is to demonstrate this common logarithmic property, that *the fluxion of a number divided by that number, is equal to the fluxion of the* Napierian *logarithm of that number*.

Fig. 59. Let BEG be a logarithmic spiral, cutting it's rays at angles of 45°: then, if AE be taken as a number, BC will be it's *Napierian* or hyperbolic logarithm.

Also, let CD express the fluxion of the logarithm BC; and the corresponding fluxion of the number AE, will be represented by FG, or it's equal FE; as the angles FEG and FGE are equal.

Now, $AC : CD :: AE : (EF =) FG$.

Therefore $CD = \frac{FG}{AE} \times AB$.

And

Fig. 58.

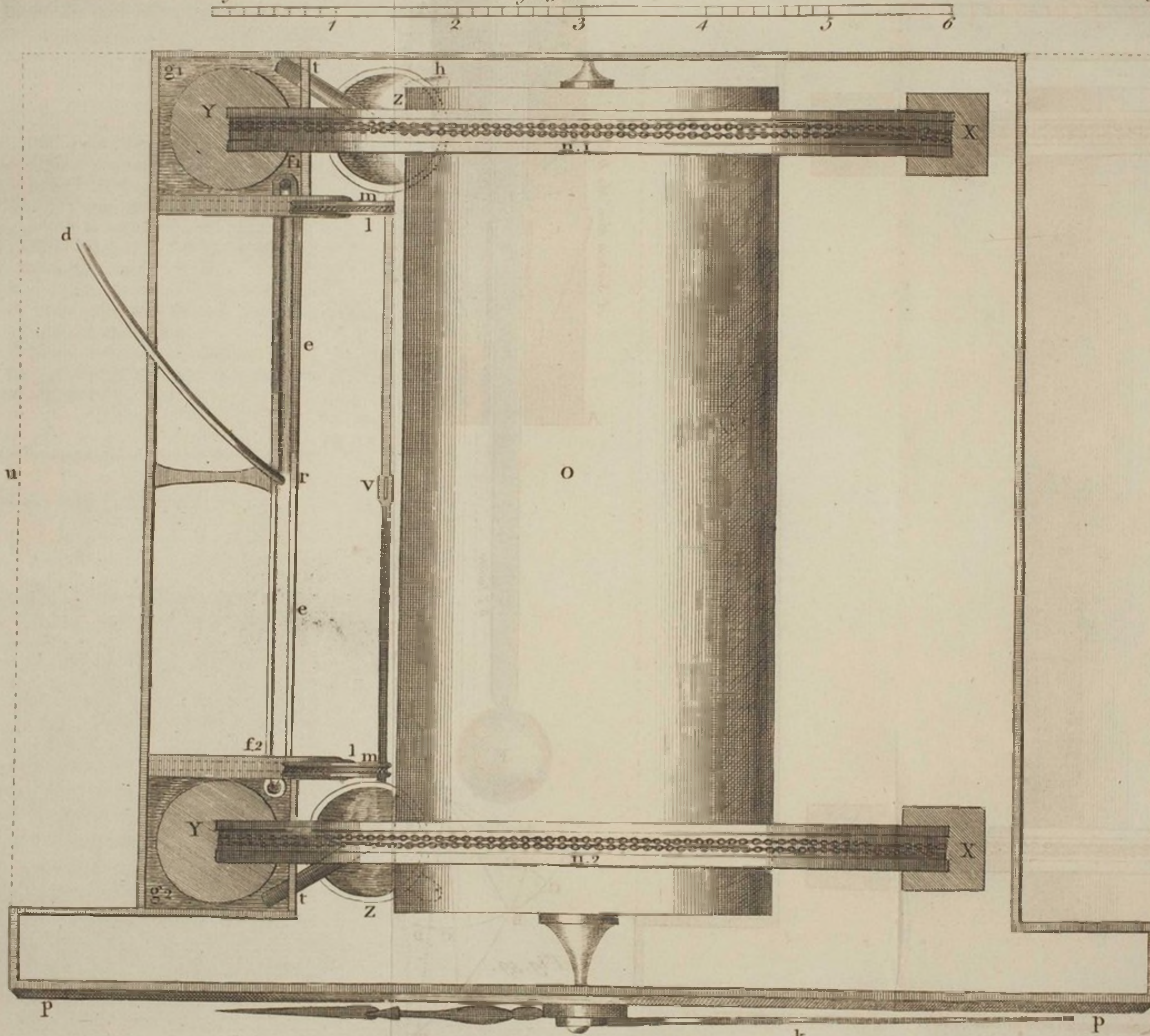
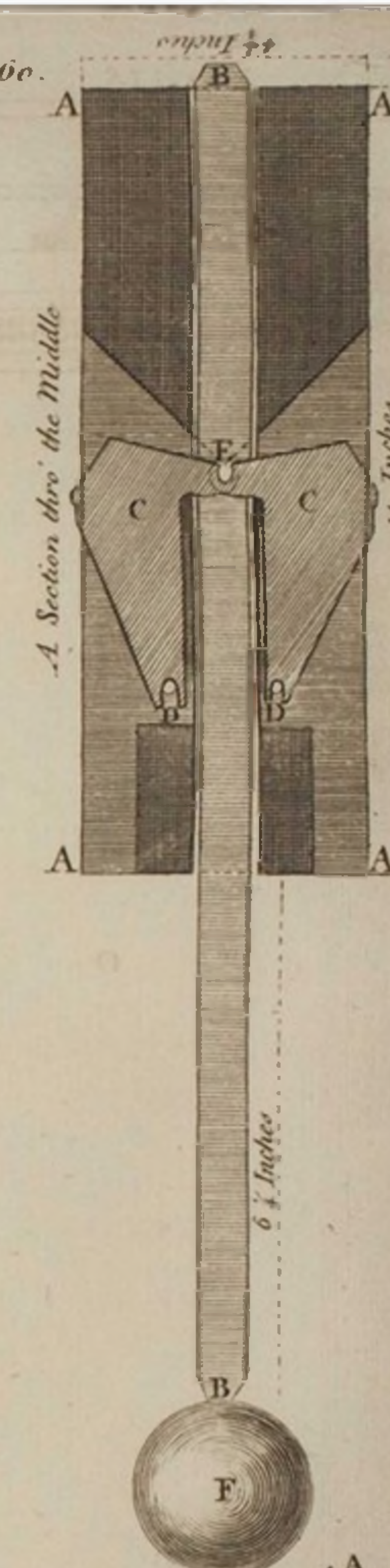


Fig. 60.



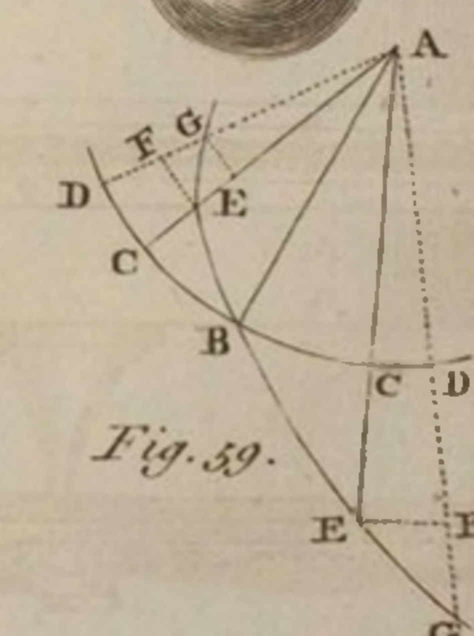


Fig. 59.

And if AB be taken as the unit or term from whence the numbers begin:

Then $CD = \frac{FG}{AE}$. *Q. E. D.*

VI. $AAAA$ represent a trunk of timber, with a square hollow, through the centre of which passes the square piece of timber BB. A groove on each side, in which are placed the two pieces of iron CC; the foot of each resting on the pins DD, that pass through the trunk; the upper part of the irons are hooked to an iron pin at E, which passes through the square piece BB; which piece is hollowed between H and H, for the hooks of the irons CC to pass up and down. *A* machine *for* sounding *the* Sea *at any depth, or in any part, invented by Major* Wm. Cook *in the year* 1738, *in a voyage to* Georgia. N°. 479. p. 146. Mar. *and* April, 1746. *Presented* April 10. 1746. Fig. 60.

When the weight F touches the ground, the two irons CC sink the trunk to G, which unhooks them at E; whereupon they fall off, and leave the trunk at liberty to float or rise up again to the surface.

A machine of these dimensions, loaded with an iron ball, F, of 12 pounds weight, being let down in water 100 fathoms deep, will go down to the bottom, and the trunk will return in 1′ 3″.

CHAP. VII.

MUSIC.

I. IN compliance with your request, I here send you some of my thoughts on the various *genera* and *species* of the *Greek* Music. What they were, and how far the doctrine of the Ancients in this respect is reconcileable with the true nature of musical sounds, are, you know, questions which have not a little perplexed the learned. *Of the various* Genera *and* Species *of* Music *among the* Ancients, *with some* observations *concerning their* scale; *in a letter from* John Christopher Pepusch, *Mus. D. &* F. R. S. *to Mr* Abr. de Moivre, *F. R. S.* N°. 481. p. 266. Oct. *&c.* 1746. *Read* Nov. 13. 1746 *here printed with alterations.*

That musical intervals are founded on certain *ratio's* or proportions expressible in numbers, is an old discovery. Nobody is better acquainted with these proportions than yourself; and I am not a little obliged to you for the light you have herein given me. It is well known, that all musical *ratio's* may be analysed into the prime numbers 2, 3, and 5; and that all intervals may be found from the octave, fifth and third *major*; which respectively correspond to those numbers. These are the Musicians elements, from the various combinations of which all the agreeable variety of relations of sounds result. This system is so well founded on experience, that we may look upon it as the standard of truth. Every interval that occurs in Music is good or bad, as it approaches to, or deviates from, what it ought to be on these principles. The doctrine of some of the Ancients seems different. Whoever looks into the numbers given us by *Ptolemy*, will not only find the primes 2, 3, and 5, but 7, 11, *&c.* introduced. Nay he he feems to think all fourths good, provided their component intervals may be expreffed by fuperparticular *ratio's*. But thefe are juftly exploded conceits; and it feems not improbable, that the contradictions of different numerical hypothefes, even in the age of *Ariftoxenus*, and their inconfiftency with experience, might lead him to reject numbers altogether. It is pity he did: had he made a proper ufe of them, we fhould have had a clearer infight into the Mufic of his times. However, what remains of the writings of this great Mufician, joined to my own obfervation and experience, has enabled me, I hope, to throw fome light upon the obfcure fubject of the ancient fpecies of Mufic.

By the manner in which *Euclid* and others find the notes of their fcale, it muft have been compofed of tones *major*, and *limma's*. Hence the feven intervals of one octave would be thus expreffed in numbers, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$.

Some modern authors have from this inferred the imperfection of the *Greek* Mufic. They alledge we here find the *ditonus*, or an interval equal to two tones *major*, expreffed by $\frac{81}{64}$, inftead of the true third *major* expreffed by $\frac{5}{4}$. As there can be no queftion of the beauty and elegance of the latter, the former therefore muft be out of tune, and out of tune by a whole comma, which is very fhocking to the ear. In like manner the trihemitone of the Ancients falls fhort of the third *minor* by a comma; which is alfo the deficiency of their hemitone or *limma*, from the true femitone *major*, fo effential to good melody. Thefe errors would make their fcale appear much out of tune to us. This I readily grant; and add, that it appeared out of tune to them; fince they exprefsly tell us, that the intervals lefs than the *diateffaron* or fourth, as alfo the intervals between the fifth and octave, were diffonant and difagreeable to the ear. Their fcale, which has been called by fome the *fcala maxima*, was not intended to form the voice to fing accurately, but was defigned to reprefent the fyftem of their modes and tones, and to give the true fourths and fifths of every key a compofer might choofe. Now if, inftead of tones *major* and *limma's*, we take the tones *major* and *minor*, with the femitone *major*, as the moderns contend we fhould, we fhall have a good fcale indeed, but a fcale adapted only to the concinnous conftitution of one key; and whenever we proceed from that into another, we find fome fourth or fifth erroneous by a comma. This the Ancients did not admit of. If, to diminifh fuch errors, we introduce a temperature, we fhall have nothing in tune but the octave. We fee then the fcale of the Ancients was not deftitute of reafon; and that no good argument againft the accuracy of their practice can from thence be formed.

It was ufual among the *Greeks* to confider a defcending as well as an afcending fcale; the former proceding from acute to grave, precifely by the fame intervals as the latter did from grave to acute. The firft found in each was the *Proflambanomenos*. The not diftinguifhing thefe two fcales has led feveral learned Moderns to fuppofe, that the *Greeks*, in fome centuries, took the *Proflambanomenos* to be the loweft note in their fyftem;

and,

and, in other centuries, to be the higheſt. But the truth of the matter is, that the *Proſlambanomenos* was the loweſt, or higheſt note, according as they conſidered the aſcending, or deſcending ſcale. The diſtinction of theſe is conducive to the variety and perfection of melody; but I never yet met with above one piece of Muſic, where the compoſer appeared to have any intelligence of this kind. The compoſition is about 150, or more, years old, for four voices; and the words are, *Vobis datum eſt noſcere myſterium regni Dei, cæteris autem in parabolis; ut videntes non videant, & audientes non intelligant.* By the choice of the words, the author ſeems to allude to his having performed ſomething not commonly underſtood.

I ſhall here give you an octave only of the aſcending and deſcending ſcales of the diatonic *genus* of the Ancients, with the names for their ſeveral ſounds, as alſo the correſponding modern letters.

Aſcending.				Deſcending.
A		Proſlambanomenos		*g*
	$\frac{9}{8}$		$\frac{8}{9}$	
B		Hypate Hypaton		*f*
	$\frac{256}{243}$		$\frac{243}{256}$	
C		Parhypate Hypaton		*e*
	$\frac{9}{8}$		$\frac{8}{9}$	
D		Lychanos Hypaton		*d*
	$\frac{9}{8}$		$\frac{8}{9}$	
E		Hypate Meſon		*c*
	$\frac{256}{243}$		$\frac{243}{256}$	
F		Parhypate Meſon		*b*
	$\frac{9}{8}$		$\frac{8}{9}$	
G		Lychanos Meſon		*a*
	$\frac{9}{8}$		$\frac{8}{9}$	
a		Meſe		*G*

Where you ſee the ſame *Greek* names ſerve for the ſounds in the aſcending and deſcending ſcales.

In the octave here given, four ſounds, *viz.* the *Proſlambanomenos*, *Hypate Hypaton*, *Hypate Meſon*, and *Meſe*, were called *ſtabiles*, from their remaining fixed throughout all the *Genera* and *Species*.

The other four ſounds being the *Parhypate Hypaton*, *Lychanos Hypaton Parhypate Meſon*, and the *Lychanos Meſon*, were called *mobiles*, becauſe they varied according to the different ſpecies and varieties of Muſic.

I come now to determine the queſtion, what theſe different *genera* and *ſpecies* were. You know, that by *genus* and *ſpecies* was underſtood a diviſion of the *diateſſaron*, containing 4 ſounds, into 3 intervals. The *Greeks* conſtituted 3 *genera*, known by the names of *enharmonic*, *chromatic*, and *diatonic*. The *chromatic* was ſubdivided into 3 ſpecies, and the *diatonic* into 2. The 3 *chromatic* ſpecies were the *chromaticum molle*, the *ſeſquialterum*,

sesquialterum, and the *toniæum*. The 2 *diatonic* species were the *diatonicum molle*, and the *intensum*; so that they had six species in all. Some of these are in use among the Moderns, but others are as yet unknown in theory or practice.

I now proceed to define all these species, by determining the intervals, of which they severally consisted; begining by the *diatonicum intensum*, as the most easy and familiar.

The *diatonicum intensum* was composed of two tones, and a semitone: but, to speak exactly, it consists of a semitone *major*, a tone *minor*, and a tone *major*. This is in daily practice; and we find it accurately defined by *Didymus* in *Ptolemy*'s *Harmonics* published by Dr *Wallis*.

The next species is the *diatonicum molle*, as yet undiscovered, as far as appears to me, by any modern author. It's component intervals are, the semitone *major*, an interval composed of two semitones *minor*, and the complement of these two to the fourth, being an interval equal to a tone *major*, and an *enharmonic diesis*.

The third species is the *chromaticum toniæum*, it's component intervals are, a semitone *major*, succeeded by another semitone *major*; and, lastly, the complement of these two to the fourth, commonly called a superfluous tone.

The fourth species is the *chromaticum sesquialterum*, which is constituted by the progression of a semitone *major*, a semitone *minor*, and a third *minor*. This is mentioned by *Ptolemy*, as the *chromatic* of *Didymus*. Examples among the Moderns are frequent.

The fifth species is the *chromaticum molle*. It's intervals are two subsequent semitones *minor*, and the complements of these two to the fourth; that is, an interval compounded of a third *minor*, and an *enharmonic diesis*. This species I never met with among the Moderns.

The sixth and last species is the *enharmonic*. *Salinas* and others have determined this accurately. It's intervals are, the semitone *minor*, the *enharmonica*, *diesis* and the third *major*.

Examples of four of these species may be found in modern practice. But I do not know of any Theorist who ever yet determined what the *chromaticum toniæum* of the Ancients was: nor have any of them perceived the analogy between the *chromaticum sesquialterum* and our modern *chromatic*. The *enharmonic*, so much admired by the Ancients, has been little in use among our Musicians as yet. As to the *diatonicum intensum* it is too obvious to be mistaken.

Aristoxenus and others often mention the tone as divided into 4 parts, and the semitone into 2; thereby making 10 divisions or *dieses* in the fourth. And this is true, if we consider these sounds in one tension; that is, either ascending or descending: but, accurately speaking, when we consider all the *dieses* or divisions of the fourth, both ascending, and descending, we shall find 13; 5 to each tone, and 3 to the semitone *major*. But then it is to be observed, that some of these divisions will be less than the *enharmonic diesis*: for, if we divide the semitone *major* into the semitone

tone *minor*, and *enharmonic dieſis*: aſcending, for inſtance, *E*, ✕*E*, *F*, and then divide in like manner deſcending, *F*, ♭*F*, *E*, we ſhall have the ſemitone *major* divided into three parts thus, *E*, ♭*F*, ✕✕*E*, *F*; where the interval between ♭*F* and ✕✕*E*, is leſs than the *enharmonic dieſis* between *E* and ♭*F* and between ✕✕*E* and *F*, is as eaſily proved.

Now, if we ſuppoſe theſe ſmall intervals equal, by increaſing the leaſt diviſion, and diminiſhing the true *enharmonic dieſis*, we ſhall then have a fourth divided into 13 equal parts; and, conſequently, the octave divided into 31 ſuch equal parts; which gives us the celebrated temperature of *Huygens*, the moſt perfect of all.

From this it appears, that the diviſion of the octave into 31 parts, was neceſſarily implied in the doctrine of the Ancients. The firſt of the Moderns who mentioned ſuch a diviſion was Don *Vincentino*, in his book intitled *L'Antica Muſica ridotta alla moderna prattica*, printed at *Rome*, 1555, *folio*. An inſtrument had been made according to his notion; which was condemned by *Zarlino* and *Salinas*, without ſufficient reaſon. But Mr *Huygens*, having more accurately examined the matter, found it to be the beſt temperature that could be contrived. Though neither this great Mathematican, nor *Zarlino*, *Salinas*, nor even Don *Vincentino*, ſeem to have had a diſtinct notion all theſe 31 intervals, nor of their names, nor of their neceſſity to the perfection of Muſic.

I muſt obſerve to you, that I received, ſome time ago, a manuſcript from *Florence*, where a Muſician of that city had rightly named theſe intervals of the octave. I found their names, you know, many years ago.

In *Huygens*'s temperature the tones are all equal: but, in a true and accurate practice of ſinging, they are not ſo. And I muſt add, that the tone divided in every ſpecies muſt be the tone *minor*; for the diviſion of the tone *major* is harſh and inelegant. So that, in the diviſion of the fourth, it is to be obſerved, that in every ſpecies, the tone *major* muſt either be an undivided interval, or make part of one.

You may perhaps wonder how the foregoing doctrine can be found in the writings of the Ancients, ſince the diſtinction of tones into *major* and *minor* is no where mentioned in their writings. But it is to be obſerved, that tho' the terms do not occur, yet the thing itſelf was not unknown to them. I own, they have not expreſſed themſelves fully; yet, from the whole of their writings come to our hands, I think the doctrine before laid down may be well ſupported. But, as it would require ſome time to put this in a juſt light, I muſt defer it to another opportunity.

II. I think the incloſed paper is the effect of great ingenuity and much thought; and as the ſubject-matter of it may tend to give great improvement and pleaſure to many, not only in our own country, but every-where, I hope my preſenting it may not be thought improper, that it may thereby be printed and publiſhed to the world.

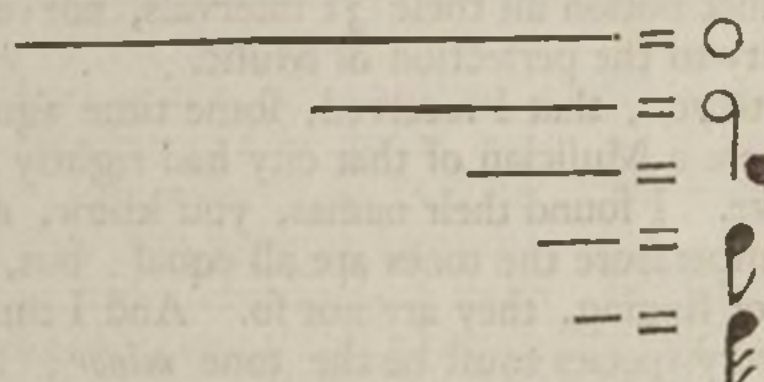
A letter from Mr John Freke, *F. R. S. Surgeon to St* Bartholomew's *Hoſpital, to the* Preſ. *incloſing* *a paper of the late Rev Mr* Creed, *concerning a* machine *to write down* extempore *Voluntaries, or other pieces of* Music N°. 483. p. 445. Mar. *&c.* 1747. *Read* Mar. 12. 1746-7.

It was invented and written by Mr *Creed*, a Clergyman, who was esteemed, by those who knew him, to be a man well acquainted with all kinds of mathematical knowledge. And was sent me by a gentleman of very distinguished merit and worth.

A demonstration of the possibility of making a machine *that shall write* extempore Voluntaries, *or other pieces of* Music, *as fast as any master shall be able to play them upon an* organ, harpsicord, *&c. and that in a character more natural and intelligible, and more expressive of all the varieties those instruments are capable of exhibiting, than the character now in use.*

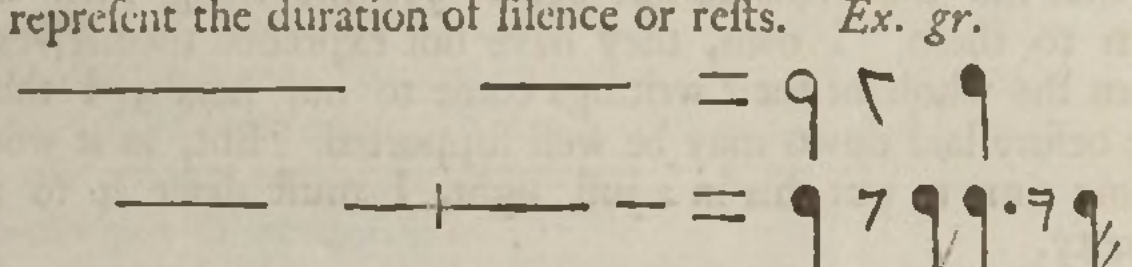
Maxim I. All the varieties those instruments afford fall under these 3 heads: *First*, The various durations of sounds, commonly called *minims*, *crotchets*, &c. *Secondly*, The various durations of silence, commonly called *rests*. *Thirdly*, The various degrees of acuteness or gravity in musical sounds, as *A re*, *B mi*, &c.

Maxim II. Strait lines, whose lengths are geometrically proportion'd to the various durations of musical sounds, will naturally and intelligibly reqresent those durations. *Ex. gr.*



The first (being 2 inches) represents a *semibreve*.
The second is 1 inch, and denotes a *minim*.
The third is half an inch, and signifies a *crotchet*.
The fourth is a quarter, and answers to a *quaver*.
The fifth is an eighth, and stands for a *semiquaver*.

Maxim III. The quantity of the blank intervals, or discontinuity of the lines, will exactly represent the duration of silence or rests. *Ex. gr.*



Maxim IV. The different degrees of Musical sounds, as *Gamut*, *A re*, *B mi*, &c. may be represented by the different situations of those black lines upon the red ones or faint ones.

Fig. 61.

Problem. *To make a machine to write Music in the aforesaid character, as fast as it can be play'd upon the organ or harpsicord, to which the machine is fixed.*

That

That a cylinder may be made by the application of a circulating, not *Poſtulatum.*
a vibrating, *pendulum*, to move equally upon it's *axis* the quantity of 1 inch in a ſecond of time, which is about the duration of a *minim* in *allegro's*;

Suppoſe the cylinder *a* to be ſuch, and to move under the keys of an Fig 62.
organ, as *b*, *c*, *d*, and nail points under the heads of the keys, it is manifeſt, that if an organiſt play a *minim* upon *c*, that is, if he preſs down *c* for the ſpace of a ſecond, the nail will make a ſcratch upon the cylinder of 1 inch in length, which is my mark for a *minim*.

Again, if he reſt a *crotchet*, that is, if he ceaſe playing for the ſpace of half a ſecond, the cylinder will have moved under the nails half an inch without any ſcratch; but if the organiſt next preſſeth down *d* for the ſpace of half a ſecond, the nail under *d* will make a ſcratch upon the cylinder half an inch long, which is my mark for a *crotchet*. It will likewiſe be differently ſituated from the ſcratch that was made by *c*, and conſequently diſtinguiſhed from it as much as the notes now in uſe are from one another by their different ſituation in the lines. (*Vide Fig.* 61.)

Theſe three inſtances include all that can be performed upon an organ, *&c.* (Maxim I.)

Therefore it is already demonſtrated, that whatever is play'd upon the organ during one revolution of the cylinder *a* (*Fig.* 62.) will be inſcribed upon it in intelligible characters. —— I proceed to ſhew how this operation may be continued for a long time.

In *Fig.* 63. *aa*, *b*, *c*, *d*, are the ſame as in *Fig.* 62. Let *x* be a long Fig 63.
ſcroll of paper wound upon ſuch a cylinder as *z*. Let *e e e e* be the ſame ſcroll brought over the cylinder *a a*, to be wound upon the cylinder *y y*, as faſt as the motion of *a a* (which is determined by a *pendulum*) will permit.

It is manifeſt, that whatever is play'd upon the organ during the winding up of *y y* will be written on the ſcroll by the pencils *b*, *c*, *d*, *&c.*

All the graces in Muſic being only a ſwift ſucceſſion of ſounds of minute duration, will be expreſſed by the pencils by ſmall hatches geometrically proportion'd to thoſe durations. *Ex. gr.*

A ſingle *Beat*

A double *Beat*

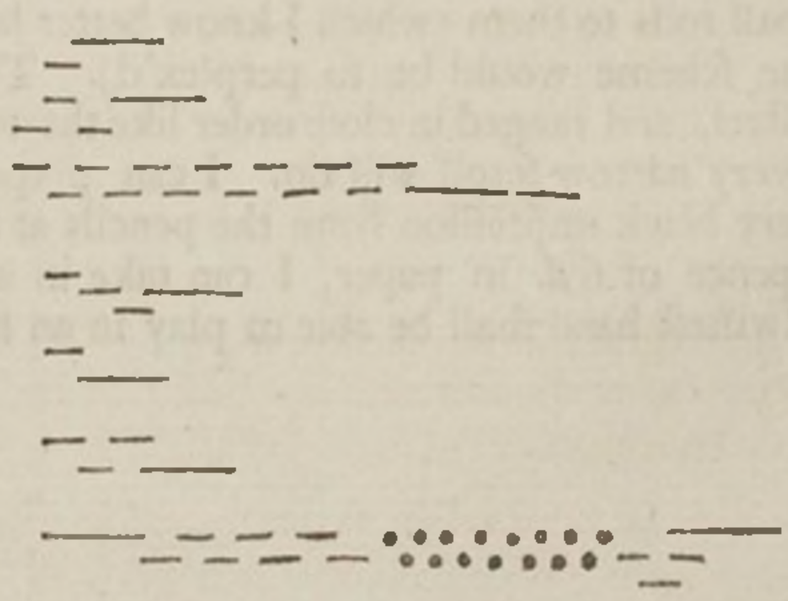
A *Shake*

A *Turn*

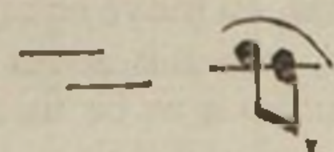
A ſingle *Backfall*

A double *Backfall*

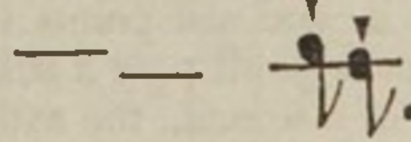
A *Shake* and *Turn*



If a line commence exactly over or under the termination of another, it is an indication of a *ſlur*; as



So a ſmall interval indicates the contrary; as



Flat or ſharp notes are implied by their ſituation on the red lines; the natural notes being always drawn between them, *viz.* in the ſpaces. (*Vide* Fig. 61.)

The ſcroll may be prepared before-hand with red lines to fall under their reſpective pencils. It is the ſureſt way to rule them after; though it is feaſible or poſſible to contrive that they may be ruled the ſame inſtant the Muſic is writing.

The places of the bars may be noted by two ſupernumerary pencils, with a communication to the hand or foot of a perſon beating time.

Grave Muſic from briſk, ſlow from faſt, *&c.* will be better diſtinguiſhed by this machine, than in the ordinary way by the words *Adagio*, *Allegro*, *Grave*, *Preſto*, &c. for, by theſe words, we only know in general this muſt be ſlow or faſt, but not to what degree, that being left to the imagination of the performer; but here I know exactly how many notes muſt be play'd in a ſecond of time; *viz.* as many as are contain'd in 1 inch of the ſcroll *per poſtulatum.*

Laſtly, whereas, in the ordinary way of writing Muſic, you have either no character for graces, or ſuch as do not denote the time and manner of their performance, here you have the the minuteſt particles of ſound that compoſe the moſt tranſient graces mathematically delineated.

N. B. Though, to facilitate the demonſtration, I ſuppoſe the pencils to be fixed under the heads of the keys, and conſequently to require a very broad ſcroll to paſs under them; yet I intend the pencils a more commodious ſituation, *viz.* the motion of the keys to be communicated by ſmall rods to them (which I know better how to do than to deſcribe, the ſcheme would be ſo perplex'd). The pencils are to be made of ſteel, and ranged in cloſe order like the teeth of a ſmall comb, ſo that a very narrow ſcroll will do. I can prepare the paper to receive a very black impreſſion from the pencils at ſo cheap a rate, that, at the expence of 6 *d.* in paper, I can take in writing all the Muſic that the ſwifteſt hand ſhall be able to play in an hour.

THE

Fig. 61.

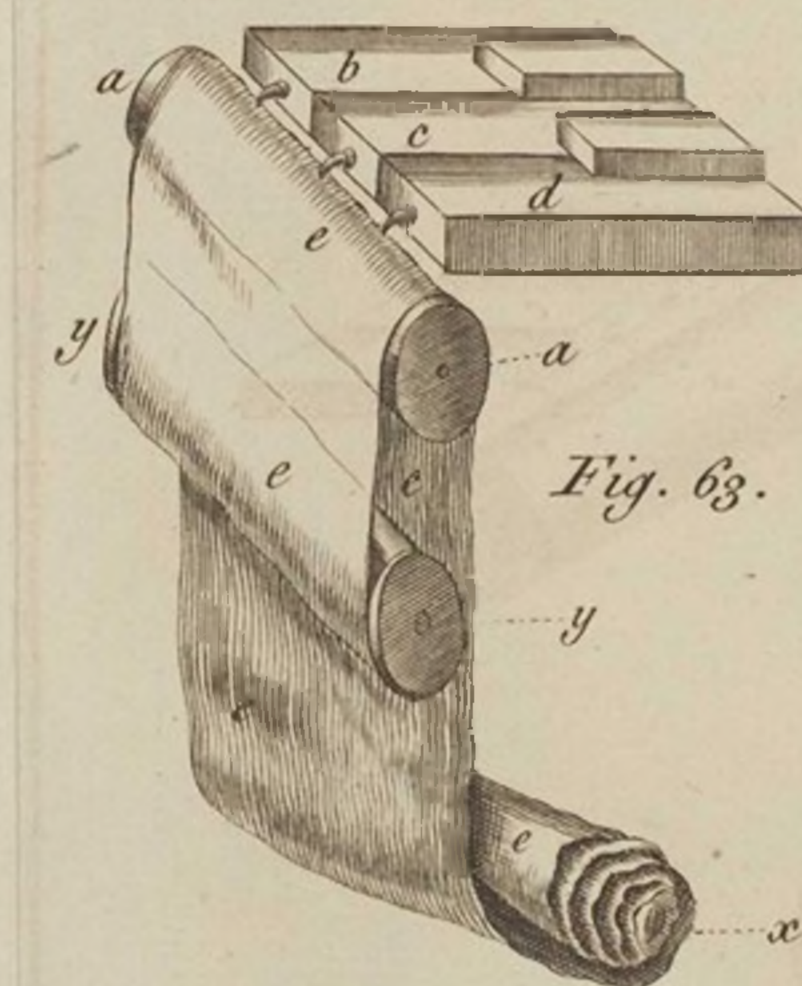
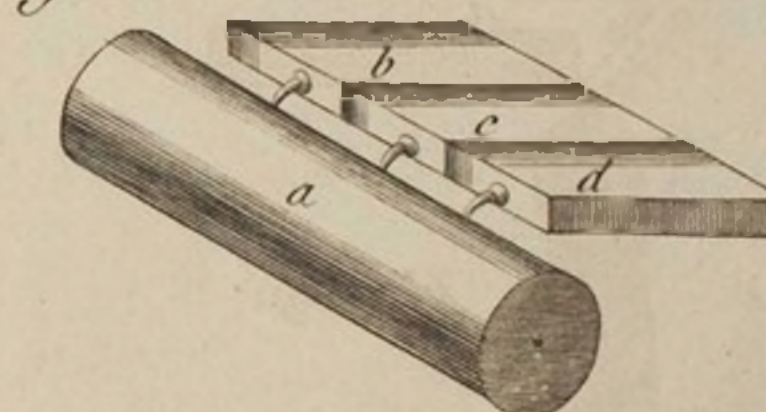
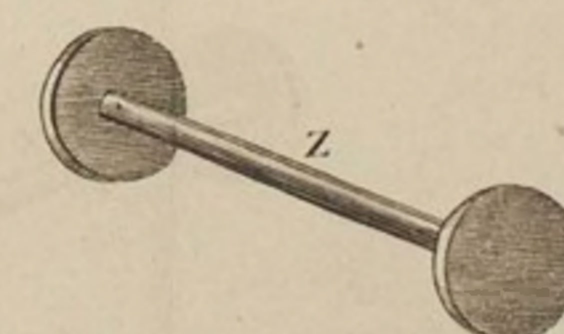


Fig. 63.

Fig. 62.





THE
Philosophical Transactions
ABRIDGED.

PART II.
CONTAINING THE
Physiological PAPERS.

CHAP. I.
PHYSIOLOGY, METEOROLOGY, PNEUMATICKS.

I. 1. THE electrical sparks from metals, such as iron and silver, are capable of kindling all such fluids as may be otherwise kindled by actual flame. And this experiment succeeds best, when the *Quinta essentia vegetabilis* is held in a spoon under the cross of a sword, whose point is turned towards the electrifying glass (TAB. II. *Fig.* 4.) * In like manner, the same spirits may easily be set on fire, by the sparks proceeding from an electrified tube of tin.

Abstract of what is contained in a book concerning Electricity, *just published at* Leipsic, 1744. *by* John Henry Winkler, Greek *and* Latin Professor *there*;

This experiment with the sparks coming from metals when made electric, was first made by Dr *Ludolph*, of *Berlin*; who, toward the

* In the Author's original book.

beginning

from Art. 75. *to* Art. 79. N°. 474 p. 166. June, &c. 1744. *Read* Nov. 22. 1744.

beginning of the present year 1744, kindled, with the sparks excited by the friction of a glass tube, the *ethereal spirits of* Frobenius. This was done at the opening of the *Royal Academy*, and in the presence of some hundreds of persons. This account was not only related in the *Berlin Gazette*, of the 30th of *May* last; but has been since confirmed by several letters, sent from *Berlin* to *Leipsic*, to Count *Manteuffel*, immediately after the experiment.

Mr *Marscall*, who now studies here, also communicated to me a letter he had received from *Berlin* concerning the same; and I have since been also certified of it, by the account of several men of learning, that had seen the experiment at *Berlin*, and that have since visited me at this place. Lastly, Mr *Reinhart*, who came hither about last *Easter*, with Count *Zaluski*, Great Chancellor of *Poland*, told me, that the experiment was not difficult to be made; and that the liquor, called *Quinta essentia vegetabilis* *, might very readily be kindled by the electrical sparks. I immediately sent for some of that essence, and found the experiment succeed to my wish.

Red-hot iron sets no spirits on fire, tho' held very near to those spirits; but if that iron is made electric, its electric sparks very readily kindle all well-rectified spirits.

The sparks that proceed from the body of a man, made electrical, kindle spirits as quick as those from electrified metal, whether the body of the man is rendered electric immediately by the glass tube, or by the intermediate tube of tin.

I made this experiment with success upon myself, before his Excellency Count *Manteuffel*, at his house, about the middle of last *May*, in the presence of Professor *Christian Wolf*, of *Hall*, and many others. Neither myself, nor any of the company, knew, at that time, that the electric sparks, from the body of a man, were capable of kindling spirits; but, upon seeing the *Quinta essentia vegetabilis* kindled with extraordinary quickness, by the sparks proceeding from an iron tube that was rusty, one of the company started the question, whether the sparks, from the body of a man, might not possibly do the same? Upon which I immediately stept on to a frame, over which blue silken lines were extended: I took hold with one hand of the rusty iron tube, and held the fingers of the other over some of the *Quinta essentia*; and the sparks from my fingers immediately struck with such violence into the silver spoon that held it, that the essence was in a moment set all in a flame.

This experiment, so unexpected, gave the greatest satisfaction to all the company; and an account of it was published in the *Leipsic Gazette* of the 21st of *May*; where it was also mentioned, that divers other experiments, with the sparks of electrified metal, had already been made both at *Dantzic*, and at *Berlin*.

Dead fowls, pork, and veal, both raw and drest, may be made electric by a tin tube, or by the hand of a man; insomuch that the sparks, pro-

* *i. e. Spirit of Wine* so highly rectified, as, being pour'd upon *gunpowder*, and then being set on fire, will at last flash the gunpowder. *C. M.*

ceeding

ceeding from thoſe ſeveral bodies, will alſo kindle the ſame eſſence. If ſuch fluid bodies, as are uſually kindled by flame, are not fine enough, they need only be warm'd a little in the ſpoon: or the ſpirits may be lighted a little before, and blown out again, before they are brought to the electrical body.

In this manner I have kindled, with the electrical ſparks, camphorated ſpirits of wine, coloured with ſaffron, the common *Eſſentia vegetabilis*; and even *French* brandy, and corn-ſpirits, only taking the precaution of warming theſe liquors a little before.

Even oil, pitch, and ſealing-wax, may be lighted by the electric ſparks, provided they are before heated to a degree that is next to kindling.

Of Electrical fire by Sam. Chriſtian Hollman, *Prof. Pub. Ord.* Gotting. *In a letter to Dr* Mortimer, *dated* Gottingen, Oct. 15. 1744. N°. 475. p. 239. Jan. *&c.* 1745. *Read* Jan. 10. 1744-5.

2. After Mr *Du Fay* had diſcovered by accident, that an electrified human body, if touched by another not electrified, would emit ſparks that pricked pretty ſharply, theſe experiments were repeated in the univerſity of *Leipſic*; and inſtead of the glaſs tube which Mr *Gray* and Mr *Du Fay* uſed, they applied a glaſs ball, ſuch as Mr *Hawkſbee* formerly uſed in his electrical experiments. On this occaſion it was obſerved, that electrified bodies, eſpecially thoſe of animals and metals, emitted a fire ſo ſtrong, that not only ſpirit of wine moderately warmed, which ſucceeds very eaſily, but alſo other inflammable bodies, ſuch as gunpowder, pitch, brimſtone, and ſealing-wax, being firſt well heated, may be ſet on fire. I relate theſe laſt experiments on the credit of another; but the former I can affirm on my own experience whilſt the glaſs ball, thro' which an iron axle paſſes, is turned ſwiftly round, there is put upon it as near as poſſible an iron tube, made of iron plates tinned over, near an inch in diameter, and 3 or 4 feet long; and laid horizontally on lines of blue ſilk: and to keep the tube from doing any hurt to the glaſs ball as it turns round, I put into it's hollow extremity ſome bundles of various ſorts of thread, ſome plain, and others covered with gold or ſilver, the extremities of which whilſt they touch the ball, amongſt other pleaſant *phænomena*, make the force in the iron tube much ſtronger. The other extremity of this tube is held by a man, who ſtands upon a cake of pitch 2 or 3 inches thick, poured into a wooden veſſel: and then the electrical force is ſo diffuſed thro' his whole body; that any part of it will attract and repel alternately leaf-gold and other light bodies, and if any part, either of the iron tube, or of the electrified perſon, is touched by another not electrified, it will emit ſparks, that are extremely pungent. It will often happen alſo, that if the electrified perſon ſtanding on the pitch has a ſword on, ſparks will be emitted from the extremity of the ſheath, even of their own accord.

Let the perſon who ſtands on the pitch hold a gold or ſilver laced hat under his arm, and let another not electrified touch the edging, and he will feel a ſmart ſtroke and pain in his arm. If a perſon not electrified holds highly rectified ſpirit of wine, moderately warmed, in a

ſpoon,

ſpoon, and an electrified perſon brings his finger, an iron key, or the point of a ſword near the ſurface of the ſpirit, it will immediately be inflamed. If an electrified perſon holds a ſpoon with ſpirit of wine in his hand, and one of the company puts his finger near it, the ſame effect will follow. If 2, 3, or 4, ſtand upon pitch, and join their hands, or unite by the mediation of a cord, iron tube, &c. the laſt will perform the ſame with the firſt and ſecond.

I do not mention other newly diſcovered *phænomena*, relating to the attraction and repulſion of the electrified body. I ſhall only add, that when the glaſs ball is turned round, a hand muſt be uſed, that is dry, and not too hot; for nothing has yet been found equal to a human hand.

A letter from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. of firing* Phoſphorus *by* Electricity. *Ibid* p. 290 *Read* March 7. 1744-5.

3. It came into my head laſt night, to try whether the *effluvia* of an excited glaſs tube would not kindle *Phoſphorus*; and having been uſing my tube for the ſake of a little exerciſe, I took a ſmall bit of about a $\frac{1}{4}$ of an inch long, which has lain by me theſe ten years; and having nothing at hand convenient for holding it, I roll'd it up in a ſmall piece of white paper; and applying it to the excited tube, it immediately took fire, emitting a conſiderable quantity of flame and ſmoke: after ſome time I quenched it, by dipping it into water, which was ready for that purpoſe; and taking it out again without ſtaying any longer than to be ſatisfied it was not on fire, I applied it as before, when it ſuddenly took fire, as at firſt: this I repeated in the ſame manner for 6 or 7 times with the like effect; tho' the *Phoſphorus* could not be drained of the water, eſpecially as the paper about it was wet.

The room in which I made the trial was not abſolutely dark, having a dull fire (tho' without any candle): the tube I uſe is about 2 feet $\frac{1}{2}$ long, the diameter of the bore nearly one inch, the thickneſs about $\frac{1}{8}$ of an inch, hermetically ſealed at one end (which ſort are, by the way, moſt convenient for rubbing): the *Phoſphorus* was held generally about 5 inches from the tube; but once or twice bringing it nearer, I could perceive a continued ray of light from the tube to the *Phoſphorus*. Some occaſions calling me away in the midſt, I could not be more accurate; but I would not omit to tell you one obſervation I made, upon pretty ſmartly exciting the tube, that the corruſcations of light were larger, more ſubſtantial, and of a more regular form than I had ever obſerved them before, this happened, not when the *Phoſphorus* was applied, but in the intervals. Whether any of the fumes of the *Phoſphorus*, which remained in the room, might contribute hereto, I cannot tell, tho' it is not very likely. Tho' I never made many trials with *Phoſphorus*, yet as I am not inſenſible, that ſome ſolid kinds of it will be inflamed by the mere action of the air upon it, when it is taken out of the water in which it is uſually kept; I was therefore minded to try whether the air would have that effect upon mine, and accordingly took it out of the water, with a *forceps*, and laid it down on a ſhelf, ſo as nothing touch'd it but the inſtrument which held it, but I could not perceive

perceive the leaſt glimmering of light, tho' the place was ſufficiently dark, after it had lain there for the ſpace of half an hour, which I thought long enough to ſatisfy me, that it was not kindled by the action of the air upon it in the above-mentioned experiment.

A repreſents the tube which I held in my right-hand, and excited with my left, having on a glove, which I find more convenient for me in rubbing it. I ſhould obſerve, that my method then was to rub it ſmartly for about half a ſcore times up and down; and then giving it one briſk ſtroke, beginning at the end from me, upon diſcharging my hand quick from the tube, the corruſcations of light appear'd as mark'd α and β, both in ſize and form: ſome allowance may be thought reaſonable to be made for one's judgment in ſuch a caſe, the motion being ſo very ſudden, and the *phænomenon* ſo ſoon diſappearing. But I intend to repeat the experiment whenever the temperature of the air ſhall be favourable, which I don't find it to be this morning. I forgot to mention, that, during this trial, I found the *effluvia* troubleſome to my eyes to a great degree, occaſioning a very ſenſible ſmarting pain, which did not go off for ſome time; tho' I never deſignedly brought the tube near my face. This was the firſt time of uſing this tube. Fig. 2.

4. § 1. Hollow glaſs balls, and veſſels of glaſs, which are rubbed by rotation and application of the hand, excite ſuch an Electricity in metals and perſons near them, that the electrical ſparks, which are emitted on the approach of a body void of Electricity, burſt out in a continual ſtream. *New obſervations on Electricity; by* Jo. Hen. Winkler, Gr. *and* Lat. Prof.

Pub. Ord. *and Rector of the Univerſity of* Leipſic. *Ibid.* p. 307. *Pref.* Mar. 21. 1744-5.

§ 2. But if the glaſs tubes and veſſels are rubbed up and down, the ſparks are emitted by intervals.

§ 3. For the more convenient rubbing of the tubes, I have cauſed a machine to be made after the following manner. Four columns are inſerted into a plank *a b c d*. On the tops *g h* of the 2 middle ones *e* and *f* are ſcrewed little planks, the middle part of which is hollowed, ſo as to fit the convexity of the glaſs tube. To theſe little planks others of the ſame kind hollowed in like manner are ſcrewed. One of theſe columns with its little planks is repreſented in *Fig.* 3. where *i k* ſhews the lower plank, *l m* the upper one, and *n o* the ſcrews that faſten them. The cavities of the upper and lower plank are ſo lined with buck-ſkin and hair, as cloſely to embrace the glaſs tube, which is to be drawn backwards and forwards. The extremities of the tube *q q* are armed with braſs caſes, which are cemented to them. *The kinds of Electricity excited by attrition.* Fig. 2. Fig. 3. Fig. 2.

To the caſes are annexed rings, to which are faſtened hempen cords, one of which *q r* is drawn thro' a hole of the column *t u*, and the other *q ſ* over a pully *x* faſtened to the column *y z*. Then the glaſs tube, being drawn backwards and forwards by two perſons, abundantly communicates the Electricity excited therein to an iron tube $\alpha\,\beta$, placed in nets of ſilk. To the extremity of the iron tube *a* are tied ſilver threads, which touch the glaſs tube between the two columns *e g* and *f h*.

§ 4. And tho' the ſparks excited by the rotation of a glaſs ball flow continually on the ſurfaces of metals; yet thoſe which ariſe from glaſs veſſels drawn to and fro are more vehemently pungent, provided that theſe veſſels are of the ſame magnitude with the balls, and the glaſs is equally good.

§ 5. The electrical ſparks alſo, which are raiſed on the ſurfaces of metals by the drawing of glaſs tubes, exceed the ſparks excited by the turning round of glaſs veſſels.

§ 6. Glaſs balls rubbed by the hand as they are turned round ſhew more Electricity, than by the application of a leathern cuſhion.

§ 7. In experiments made either by turning the ball, or drawing the tube, there is need of three perſons. But in uſing the Turners wheel there wants only one.

The method of increaſing Electricity.

§ 8. I call that Electricity *ſimple*, which is raiſed by one glaſs veſſel, ball, or tube; *double*, which is raiſed by 2, *triple* by 3, *quadruple* by 4, and ſo on.

§ 9. The Electricity, which I raiſed by the attrition of 2 glaſs balls, of the diameter of $\frac{1}{2}$ a *Paris* foot, was ſo great in water, ſnow, and ice, that the electrical ſparks flying from theſe bodies have ſet fire to pure ſpirit of wine warmed.

In water the experiment is made 2 ways. For either the ſpirit is applied in a ſmall ſpoon, and hanging from an electrified iron tube: or elſe a finger dipped in warm ſpirit of wine is extended over water in a tin veſſel, but at a certain diſtance from the ſurface of the water. To the veſſel, covered with a ſilken net, is added an iron wire, which reaches to the glaſs ball, tube, or veſſel, in the electrical machine. Snow and ice alſo are laid upon the ſilken net in the tin veſſel.

§ 10. To make the Electricity ſtill greater, 2 machines are ſo placed as to have each of them 2 balls, which communicate the Electricity to the ſame iron tube. Over each machine is laid a ſilken net *a b*, to which the iron tube *c d* is joined, which extends near the machine 2 iron arms, *b c*, *e f* and *b d*, *g h*, to which ſilver threads are joined touching the balls in *i k l m*. (Fig. 1.)

If inſtead of balls I make uſe of glaſs cups, which as they are turned round are rubbed by cuſhions; I add no ſilver threads to the iron arms, to touch the veſſels. For I have found, that by adding theſe the Electricity is diminiſhed.

The Electricity, when it returns into the body, from which it firſt proceeded, is diminiſhed.

§ 11. The machine with the glaſs veſſel, and a man that turns the glaſs veſſel with his foot after the manner of the Turners, reſt upon ſilken nets large enough for the machine and the man to be at a conſiderable diſtance from the wooden ſides, to which the nets are faſtened.

Fig 5.

§ 12. When the glaſs veſſel as it turns is rubbed by the cuſhion, not only the iron tube placed on the tube and neareſt the veſſel, but alſo the man manand the machine discover a certain Electricity, by which light bodies under a glass ball, which another man holds in his hand, are variously moved.

§ 13. The same happens, if a ball is made use of instead of the vessel; and the person, who applies his hand as it turns, stands with one foot on the machine, and the other on the silken net.

§ 14. But when things are thus constituted, and the iron tube *a b* is placed on the silken net near the glass vessel or ball, another tube *c d* is added, and extended in such a manner as to touch the machine in *e*, the sparks, which before were excited, cease, and the attracting force is greatly diminished. Fig. 10.

§ 15. The machine, by which Electricity may conveniently be excited *in vacuo*, and propagated thro' a glass ball into the air, and communicated to all sorts of bodies, is represented in *Fig.* 6. Electricity *in vacuo.* Fig. 6.

It consists of a glass vessel *a b c d*, to the bases of which *a c* and *b d* are cemented brazen plates, to one of which *a c* is annexed a wooden arm *e f*. In this wooden arm and another plate *b d* are conical cavities, into which little axles may be put, which being in form of a screw are fastened into the sides of the metallic support *g h i k l m*, which being furnished with a male screw *m n*, may be inserted into a female screw in the orb of the pneumatical machine. The male screw passes thro' a hole of a bent elastic plate. To the foot of the support *l m* a plate *n o* is screwed, the upper part of which *p q* being lined with buckskin and hair approaches the glass vessel. Fig 8. Fig 9. Fig 7.

Into the ball *a b c d* is infixed a perforated metalline cylinder *g*, thro' the cavity of which a piece of catgut is passed. This catgut is wound about the wooden arm *e f* within the ball, and has a button which fastens it to a bent elastic plate perforated at the end *r f t*. The catgut is let out of the bell thro' a hog's bladder open on both sides. One part of the bladder is bound about the metalline tube *g*, and tied with a piece of packthread; and the other *u* is strongly fastened between 2 knots made in the catgut. The bladder is wetted, so that after it has been wiped on the inside with a linen cloth, it may easily be extended or contracted. On the outside of the bladder appears a certain part of the catgut *u x*, by the drawing of which the glass vessel may be agitated and rubbed under the bell. Fig 6.

§ 16. In a square iron vessel α β γ δ, which is placed either in a silken net extended over a hollow glass vessel *a b c d*, Fig. 8. or upon resin, or sealing wax, and has an iron stile γ ι, annexed to it and extended toward the cushion, are placed small bits of leaf-gold. To a moveable metalline cylinder ζ π, which may be thrust thro' the middle of the neck of the bell, is annexed transversly an iron wire π ϑ, 2 or 3 lines distant from the pieces of leaf-gold, which leap towards it, as soon as the glass vessel, on the air being drawn out of the bell, is agitated, and rubbed by the cushion. Fig. 6, 7, 8.

§ 17. In the other perforated ſide of the bell λ, a ſmall glaſs tube is fixed, thro' which an iron wire, κ λ δ, reaches to the middle of the glaſs veſſel, an exceeding ſmall ſpace being between the veſſel and the wire. The tube and the wire are ſo ſtrongly cemented with ſealing-wax, that no air can penetrate. And that it may be all driven out, the moveable cylinder ζ π, is covered with ſuet, where it touches the neck of the bell. On drawing the catgut *x u g*, the wire not only conceives Electricity from the agitation and attrition of the veſſel, but alſo propagates it thro' the glaſs tube ſtopt with ſealing-wax, and communicates it to bodies laid on ſilk, which touch the wire on the outſide in κ, ſo that the metals emit electrical ſparks in the dark, on the approach of bodies void of Electricity.

§ 18. Thus alſo Electricity excited without is communicated to the wire, and pervades thro' the ſealed tube, and emits fire in the dark at the end of the wire within the bell, and attracts the leaf-gold on the iron veſſel.

Uſe of the machine deſcribed in Fig. 4.

§ 19. Between the 2 anterior columns *a b* and *c d*, are ſuſpended glaſs veſſels or balls *e* and *f*, and an elaſtic plate *i k* is put into the upper hole of the third poſterior column, and a wheel is added to the ſide. A catgut faſtened to the elaſtic plate in *k* is wound round the longer arms of the veſſels, and faſtened to the moveable plank *b l m n*. Thus the glaſs veſſels may be turned round.

§ 20. In order to turn a glaſs veſſel or ball, a cord *o p q r* is brought round the wheel, and the wooden pullies of the veſſels or balls, and may be ſtraitened or looſened by means of a ſcrew applied to the hinder part of the machine.

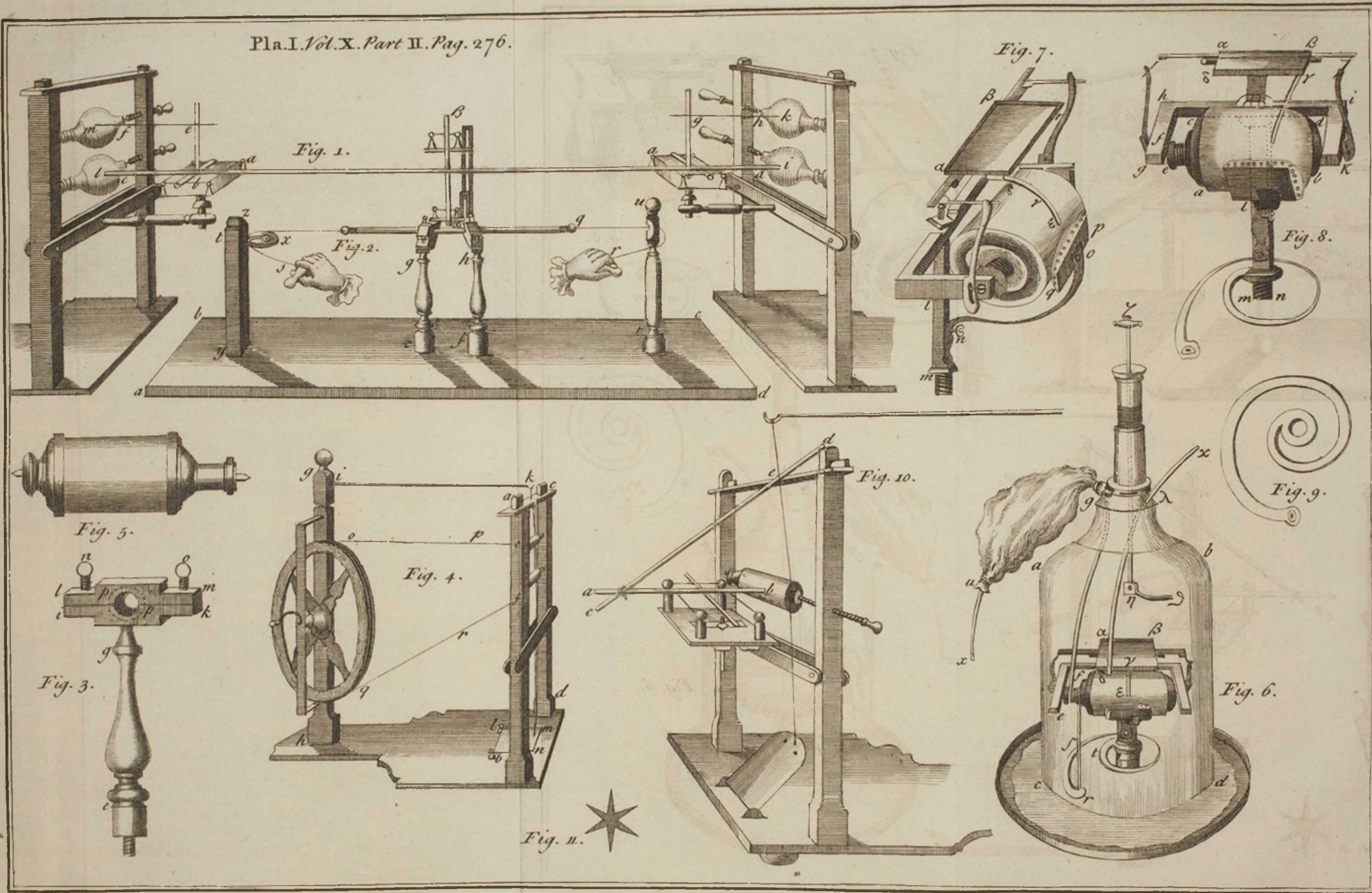
§ 21. The anterior columns are faſtened by 2 braces, from which 2 perforated cylinders ſtand out, in the hinder part of which a very ſmall column is fixed, in which again two little cylinders covered with buckſkin with hair underneath is faſtened: but in the fore part an inſtrument in which ſilken threads are extended, to which an iron tube with 2 arms is faſtened. This tube is held by perſons ſtanding on ſilken nets to be electrified. Into this tube if a ſword is put, which hangs by the hilt with a ſilken thread, the electrical ſparks will be emitted from it's ſhell, and kindle ſpirit of wine in a ſmall ſpoon. So what I call an electrical ſtar * is laid on a large ſilken net, and connected by means of an iron wire with the brachiated glaſs tube, annexed to a ſmaller net near the veſſels or balls. As ſoon as the glaſs veſſels in turning receive the friction of the cuſhions, or hand, the rays of the ſtar emit ſhining ſtreaks in the dark, and when the ſtar is turned round, deſcribe a lucid circle.

Fig. 11.

§ 22. When the veſſels are turned round, ſilver threads, touching the veſſels are joined to the arms of the iron tube. Thus a continued ſtream of Electricity is obtained. But on the contrary the Electricity is diminiſhed, if the extremities of the veſſels, as they turn, have ſilver

* See the *Acta Germanica*, or *Literary Memoirs of* Germany, Vol. II. p. 123.

threads


Pla. I. Vol. X. Part II. Pag. 276.
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.

threads added, which touch the veſſels. In like manner if cuſhions are applied inſtead of the hand, the Electricity decreaſes.

Abſtract of a letter from M. De Bozes, *Prof. Exper. Philoſ. at the Acad of* Wirtemberg, *to M.* De Maizeaux. *Communicated by Mr* Baker *from Mr* Ellis, *and tranſlated out of the* Latin *by Mr*. Baker. N°. 476. p. 419. Apr. &c. 1745. *Read* May 23. 1745.

5. A hollow globe of glaſs, of 6 or 8 inches diameter, being ſwiftly turned round upon it's *axis*, by means of a large wheel, in the manner Mr *Haukſbee* formerly adviſed; and being rendered as electrical as poſſible by the application of a dry woollen cloth, or rather of a very dry hand; if, whilſt in this ſwift rotation, it be brought near the end of an iron bar, ſuſpended by ſtrings of ſilk that are exceedingly well dried, ſuch an electric power will be communicated to the iron, that upon touching the other end of it with one's finger, not only ſparks of fire, in the uſual manner, will be emitted very briſkly, but even blood will be drawn from the finger; the ſkin of which will be burſt, and a wound appear as if made by a cauſtic.

2. If highly rectified ſpirit of wine heated in a ſpoon, the ethereal ſpirit of *Frobenius*, oil of turpentine, ſulphur, pitch, or reſin melted, be applied to the iron bar, inſtead of one's finger, the ſparks proceeding therefrom will ſet it on fire inſtantly.

3. A chair being ſuſpended by ropes of ſilk, made perfectly dry, a man placed therein is rendered ſo much electrical by the motion of the above-mentioned globe, that, in the dark, a continual radiance, or *corona* of light, appears incircling his head, in the manner ſaints are painted.

4. If ſeveral ſuch-like globes, or electric tubes, are brought near the man ſuſpended in the chair, the motions of the heart and arteries are very ſenſibly increaſed; and if a vein be opened under the operation, the blood that comes from it appears lucid like *phoſphorus*, and runs out faſter than when the man is not electrify'd.

5. Water, in like manner, ſpouting from an artificial fountain ſuſpended by ſilk lines, ſcatters itſelf in luminous little drops; and a larger quantity of water is thrown out, in any given time, than when the fountain is not made electric.

N. B. If 3, 4, or 5 globes be employed, the effect will be proportionably better: and M. *L'Abbé Nollet* has found, that globes or tubes made of glaſs, coloured blue with *zaffer*, are preferable to others; for when the glaſs is blue, the experiments ſucceed in all weathers; whereas, in damp weather, the white glaſs loſes much of it's electric power.

A letter from the Rev Henry Miles, *D. D. F. R. S. to the* Pres. *containing Obſervations of* luminous Emanations

6. In the late edition of the works of the Hon. Mr *Boyle*, * is a letter from Mr *Clayton*, dated *June* 23. 1684. at *James* city in *Virginia*; in which he gives Mr *Boyle* an account of a ſtrange accident (as he calls it); and adds, that he had incloſed the very paper Colonel *Digges* gave him of it, under his own hand and name, to atteſt the truth; and that the ſame was alſo aſſerted to him by Madam *Digges*, his lady, ſiſter to the

* Vol. V. Page 646.

wife

from human Bodies, *and from* Brutes; *with some Remarks on* Electricity. *Ibid* p. 441. *dated* May 9. 1745. *Read* June 13. 1745.

wife of Major *Sewall*, and daughter of the Lord *Baltimore*, to whom this accident happened.

This paper, very unhappily, came not to hand till after Mr *Boyle*'s works were printed; and therefore could not be inserted with Mr *Clayton*'s letter: but, having since met with it, I present the following exact copy of it.

" *Maryland*, *Anno* 1683.

" There happened, about the month of *November*, to one Mrs *Su-*" *sannah Sewall*, wife to Major *Nic. Sewall* of the province abovesaid, " a strange flashing of sparks (seem'd to be of fire) in all the wearing " apparel she put on, and so continued till *Candlemas*: and, in the " company of several, *viz.* Captain *John Harris*, Mr *Edward Branes*, " Captain *Edward Poulson*, &c. the said *Susannah* did send several of " her wearing apparel; and, when they were shaken, it would fly out " in sparks, and make a noise much like unto bay-leaves when flung " into the fire; and one spark litt on Major *Sewall*'s thumb-nail, and " there continued at least a minute before it went out, without any " heat: all which happened in the company of

Wm. Digges,

* " My Lady *Baltimore*, her mother-in-law, for some time before " the death of her son *Cæcilius Calvert*, had the like happened to her; " which has made Madam *Sewall* much troubled at what has happened " to her."

" They caused Mrs *Susanna Sewall* one day to put on her sister " *Digges*'s petticoat, which they had tried beforehand, and would not " sparkle; but at night when Madam *Sewall* put it off, it would sparkle " as the rest of her own garments did."

The celebrated *Bartholin* of *Copenhagen*, in his collection of anatomical histories that are unusual, † which he intitles *Mulier splendens*, gives us a parallel instance in a noble lady of *Verona* in *Italy*, which, he says, he had from an account of the *phænomenon* published by *Petrus à Castro*, a learned Physician of the same place, in a small treatise intituled *De Igne Lambente*. There is this circumstance not mentioned in Mrs *Sewall*'s case (tho' perhaps it would have happened, if trial had been made, as well as in the case of the *Italian* lady); which I think not improper to mention, in *Bartholin*'s own words. ——— " *ut quotiens leviter linteo corpus tetigerit, scintillæ ex artubus copiose prosiliant,* " *cunctis domesticis conspicuæ, non secus ac si è silice excuterentur.*" At the conclusion of this relation he refers us to a book of his, intituled, *De Luce Animalium*, for more instances of these lucid *effluvia*; and

* The additional lines are not in Colonel *Digges*'s hand, but seem to be in Mr *Clayton*'s.

† Cent. III. Hist. LXX. ‖ Hist XII.

says

ſays, he has there ſhown the cauſe of them at large; but, as I have not yet got a ſight of that book, I can ſay nothing further —— only, that in the ſecond Cent. of the hiſtories above-mentioned, * he aſſerts, that he has prov'd, in his book *de Luce*, &c. that light is connatural or innate to all, as well vegetables as animals.

There is another author, Dr *Simpſon*, who publiſhed a Philoſophical Diſcourſe of Fermentation, dedicated to the *R. Soc.* 1675. who takes notice of light proceeding from animals, on the frication or pectation (as he calls it) of them; and inſtances in the combing a woman's head, the currying of a horſe, and the frication of a cat's back; the two laſt of which are known to moſt. I cannot tell whether it be material to add, that, according to this gentleman's hypotheſis, he would aſſign the principles of fermentation, which he ſuppoſes to be *Acidum & Sulphur*, as the cauſe of theſe lucid *effluvia* in animals. His hypotheſis I may not take upon me to judge of; but I humbly apprehend, the properties of the *effluvia* in animal bodies are many of them common with thoſe produced from glaſs, *&c.*; ſuch as their being lucid, their ſnapping, and their not being excited without ſome degree of friction, and, I preſume, I may add, Electricity; for I have, by repeated trials, found a cat's back to be ſtrongly electrical when ſtroak'd.

P. S. In the account of ſome of the earlier electrical experiments made by Mr *Gray* †, we are informed, that he electrified ſeveral other bodies, beſides animal ſubſtances, by drawing them between his thumb and fingers; in particular, linen of divers ſorts, paper, and fir-ſhavings, which would not only be attracted to his hand, but attract all ſmall bodies to them, as other electric bodies do. Now, notwithſtanding this laſt circumſtance of their attracting, as well as being attracted, may it not be queſtioned, whether, in this way of trial, it appears that they are electrical bodies, or Electrics *per ſe?* Is it not doubtful (ſince his fingers muſt be excited conſiderably in this experiment) whether he did not communicate Electricity to them from his hand, rather than excite it in them? I have no doubt but that the principle is inherent in many other bodies beſides animal, poſſibly, in all bodies whatever; but as it is allow'd, I ſuppoſe generally, that animals have a greater quantity of it reſiding in them, than other ſubſtances, there ſeems room to admit the doubt I have mention'd, which I ſubmit to the conſideration of ſuch as are curious in experiments of this kind.

7. The Society having heard, from ſome of their correſpondents in *Germany* ‖, that what they call a vegetable quinteſſence had been fired by Electricity, I take this opportunity to acquaint you, that, on *Friday* *Experiments and obſervations tending to illuſtrate the nature*

* Hiſt. XII.
† See Vol. V. Part ii. Chap. I. Sect. III. 1.
‖ See the preceding Articles.

evening

and properties of Electricity; *by* Will. Watson, *Apothecary, F. R. S.* N°. 477. p. 481. Aug &c. 1745. *Read at several meetings of the* R. S. *between* Mar. 28, *and* Oct. 24, 1745. *here printed with alterations.*

A letter to Martin Folkes, *Esq*; *Pr. R. S. dated* Mar. 27, 1745.

evening last, I succeeded, after having been disappointed in many attempts, in setting spirits of wine on fire by that power.

The preceding part of the week had been remarkably warm, and the air very dry; than which nothing is more necessary towards the success of electrical trials: to these I may add, that the wind was then easterly, and inclining to freeze. I that evening used a glass sphere, as well as a tube; but I always find myself capable of sending forth much more fire from the tube than from the sphere, probably from not being sufficiently used to the last.

I had before observ'd, that, altho' * non-electric bodies made electrical, lose almost all that Electricity, by coming either within or near the contact of *non-electrics* not made electrical. It happens otherwise with regard to *Electrics per se*, when excited by rubbing, patting, *&c.*; because from the rubbed tube I can sometimes procure five or six flashes from different parts; as though the tube of 2 feet long, instead of being one continued cylinder, consisted of five or six separate segments of cylinders, each of which gave out it's Electricity at a different explosion.

The knowledge of this theorem is of the utmost consequence towards the success of electrical experiments; inasmuch as you must endeavour, by all possible means, to collect the whole of this fire at the same time. Prof. *Hollman* seems to have endeavour'd at this, and succeeded, by having a tin tube; in one end of which he put a great many threads, whose extremities touch'd the sphere when in motion, and each thread collected a quantity of electrical fire, the whole of which center'd in the tin tube, and went off at the other extremity. Another thing to be observed is to endeavour to make the flashes follow each other so fast, as that a second may be visible before the first is extinguish'd. When you transmit the electrical fire along a sword, or other instrument, whose point is sharp, it often appears as a number of disseminated sparks, like wet gunpowder or wild-fire: but if the instrument has no point, you generally perceive a pure bright flame, like what is vulgarly call'd the *blue ball*, which gives the appearance of stars to fired rockets.

The following is the method I made use of, and was happy enough to succeed in. I suspended a poker in silk lines; at the handle of which I hung several little bundles of white thread, the extremities of which were about a foot at right angles from the poker. Among these threads, which were all attracted by the rubbed tube, I excited the greatest electrical fire I was capable, whilst an assistant, near the end of the poker, held in his hand a spoon, in which were the warm spirits. Thus the

* I call *Electrics per se*, or originally *Electrics*, those bodies, in which an attractive power towards light substances is easily excited by friction; such as glass, amber, sulphur, sealing-wax, and most dry parts of animals, as silk, hair, and such like. I call *Non-Electrics*, or conductors of Electricity, those bodies in which the above property is not at all, or very slightly, perceptible: such as wood, animals living or dead, metals, and vegetable substances. See *Gray, Du Fay, Desaguliers, Wheler*, in the *Philos. Trans.*

thread

thread communicated the Electricity to the poker, and the ſpirit was fired at the other end. It muſt be obſerv'd in this experiment, that the ſpoon with the ſpirit muſt not touch the poker; if it does, the Electricity, without any flaſhing, is communicated to the ſpoon, and to the aſſiſtant in whoſe hand it is held, and ſo is loſt in the floor.

By theſe means I fired ſeveral times not only the ethereal liquor or *Phlogiſton* of *Frobenius*, and rectified ſpirit of wine, but even common proof ſpirit. Theſe experiments, as I before obſerved, were made laſt *Friday* night, the air being perfectly dry. *Sunday* proved wet, and *Monday* ſomewhat warm; ſo that the air was full of vapour, wind S. W. and cloudy. Under theſe diſadvantages, on *Monday* night I attempted again my experiments; they ſucceeded, but with infinitely more labour than the preceding, becauſe of the unfitneſs of the evening for ſuch trials.

I lately acquainted you, that I had been able to fire ſpirit of wine, the *Phlogiſton* of *Frobenius*, and common proof ſpirit, by the power of Electricity. Since which (till yeſterday) we have had but one very dry fine day; *viz. Monday Apr.* 15. wind E. N. E.; when, about 4 in the afternoon, I got my *apparatus* ready, and fired the ſpirit of wine four times from the poker as before, 3 times from the finger of a perſon electrified, ſtanding upon a cake of wax, and once from the finger of a ſecond perſon ſtanding upon wax, communicating with the firſt by means of a walking-cane held between their arms extended. The horizontal diſtance in this caſe between the glaſs tube and the ſpirit was at leaſt ten feet.

A letter to the Royal Society, *dated* Apr. 25, 1745. *Read* April 25, 1745.

You all know, that there is the repulſive power of Electricity, as well as the attractive; inaſmuch as you are able, when a feather, or ſuch-like light ſubſtance, is replete with Electricity, to drive it about a room, which way you pleaſe. This repulſive power continues, until either the tube loſes it's excited force, or the feather attracts the moiſture from the air, or comes near to ſome non-electric ſubſtance; if ſo, the feather is attracted by, and it's Electricity loſt in, whatever non-electric it comes near. In electrified bodies, you ſee a perpetual endeavour to get rid of their Electricity. This induced me to make the following experiment.

I placed a man upon a cake of wax, who held in one of his hands a ſpoon with the warm ſpirits, and in the other a poker with the thread. I rubbed the tube amongſt the thread, and electrified him as before. I then ordered a perſon not electrified to bring his finger near the middle of the ſpoon; upon which, the flaſh from the ſpoon and ſpirit was violent enough to fire the ſpirit. This experiment I then repeated three times.

In this method, the perſon by whoſe finger the ſpirit of wine is fired, feels the ſtroke much more violent, than when the electrical fire goes from him to the ſpoon. This way, for the ſake of diſtinction, we will call the repulſive power of Electricity.

The late Dr *Deſaguliers* has obſerved, in his excellent Diſſertation concerning Electricity. 'That there is a ſort of capricioufneſs attending

'these experiments, or something unaccountable in their *phæno-*
'*mena*, not to be reduced to any rule. For sometimes an experi-
'ment, which has been made several times successively, will all at
'once fail.' Now I imagine, that the greatest part, if not the whole of this matter, depends upon the moisture or dryness of the air; a sudden though slight alteration in which, perhaps not sufficient to be obvious to our faculties, may be perceived by the very subtle fire of Electricity. For,

1st, I conceive, that the air itself (as has been observed by Dr *Desaguliers*) is an electric *per se*, and of the vitreous kind; therefore it repels the Electricity arising from the glass tube, and disposes it to electrify whatever non-electrical bodies receive the *effluvia* from the tube.

2dly, That water is a non-electric, and, of consequence, a conductor of Electricity. This is exemplified by a jett of water being attracted by the tube, from either electrics *per se* conducting Electricity, and non-electrics more readily when wetted; but what is more to my present purpose, is, that if you only blow through a dry glass tube, the moisture from your breath will cause that tube to be a conductor of Electricity.

These being premised, in proportion as the air is replete with watery vapours, the Electricity arising from the tube, instead of being conducted, as proposed, is, by means of these vapours, communicated to the circumambient atmosphere, and dissipated as fast as excited.

This theory has been confirmed to me by divers experiments, but by none more remarkably than on the evening of the day I made those before-mention'd; when the vapours, which in the afternoon, by the sun's heat, and a brisk gale, were dissipated, and the air perfectly dry, descended again in great plenty, upon the absence of both, and in the evening was very damp. For between seven and eight o'clock, I attempted again the same experiments in the same manner, without being able to make any of them succeed; though all those mentioned in this paper, with others of less note, were made in less than half an hour's time.

I am the more particular in this, being willing to save the labour of those, who are desirous of making this kind of trials. For, although some of the lesser experiments may succeed almost at any time, yet I never could find, that the more remarkable ones would succeed but in dry weather.

A letter to the Royal Society. *Read* Octob. 24. 1745.

In some papers I lately did myself the honour to lay before you, I acquainted you of some experiments in Electricity; particularly I took notice of having been able to fire spirit of wine by what I called the repulsive power thereof; which I have not heard had been thought of by any of those *German* gentlemen, to whom the world is obliged for many surprising discoveries in this part of Natural Philosophy.

How far, strictly speaking, the spirit, in this operation, may be said to be fired by the repulsive power of Electricity, or how far that power, which

which repels light ſubſtances when fully impregnated with Electricity, fires the ſpirit, may probably be the ſubject of a future inquiry; but, as I am unwilling to introduce more terms into any demonſtration than what are abſolutely neceſſary for the more ready conception thereof, and as inflammable ſubſtances may be fired by Electricity two different ways, let the following definitions at preſent ſuffice of each of theſe methods.

But firſt give me leave to premiſe, that no inflammable ſubſtances will take fire, when brought into or near the contact of electrics *per ſe* excited to Electricity. This effect muſt be produced by non-electrical ſubſtances impregnated with Electricity received from the exciting electrics *per ſe*. But to return:

1ſt, I ſuppoſe that inflammable ſubſtances are fired by the attractive power of Electricity, when this effect ariſes from their being brought near excited non-electrics.

2dly, That inflammable ſubſtances are fired by the repulſive power of Electricity; when it happens, that the inflammable ſubſtances, being firſt electrified themſelves, are fired by being brought near non-electrics not excited.

This matter will be better illuſtrated by an example. Suppoſe that either a man ſtanding upon a cake of wax, or a ſword ſuſpended in ſilk lines, are electrified, and the ſpirit, being brought near them, is fired, this is ſaid to be performed by the attractive power of Electricity. But if the man electrified, as before, holds a ſpoon in his hand containing the ſpirit, or the ſame ſpoon and ſpirit are placed upon the ſword, and a perſon not electrified applies his finger near the ſpoon, and the ſpirit is fired from the flame ariſing from the ſpoon and ſpirit upon ſuch application, this I call being fired by the repulſive power. Of the two mention'd kinds I generally find the repulſive power ſtrongeſt.

Since my laſt communication, the ſpirit has been fired both by the attractive and repulſive power thro' four perſons ſtanding upon electrical cakes, each communicating with the other, either by the means of a walking-cane, a ſword, or any other non-electric ſubſtance. It has likewiſe been fired from the handle of a ſword held in the hand of a third perſon.

I have not only fired *Frobenius*'s *Phlogiſton*, rectified ſpirit, and common proof ſpirit, but alſo *Sal volatile oleoſum*, ſpirit of lavender, dulcified ſpirit of Nitre, Peony-water, *Daffy*'s elixir, *Helvetius*'s ſtyptic, and ſome other mixtures where the ſpirit has been very conſiderably diluted; likewiſe diſtilled vegetable oils, ſuch as that of turpentine, lemon, orange-peels, and juniper; and even thoſe of them which are ſpecifically heavier than water, as oil of ſaſſafras; alſo reſinous ſubſtances, ſuch as balſam *Capivi*, and turpentine; all which ſend forth, when warmed, an inflammable vapour. But expreſſed vegetable oils, as thoſe of olives, linſeed, and almonds, as well as tallow, all whoſe vapours are uninflammable, I have not been able yet to fire; but theſe indeed will not fire on the application of lighted paper. Besides, if these last would fire with lighted paper, unless their vapours were inflammable, I can scarce conceive they would fire by Electricity; because, in firing spirits, *&c.* I always perceive, that the Electricity snaps, before it comes in contact with their surfaces, and therefore only fires their inflammable vapours.

As an excited non-electric emits almost all its fire, if once touch'd by a non-electric not excited, I was desirous of being satisfy'd, whether or no the fire emitted would not be greater or less in proportion to the volume of the electrified body. In order to this, I procured an iron bar about 5 feet long, and near 170 pounds in weight; this I electrified lying on cakes of wax and resin, but observed the flashes arising therefrom not more violent than those from a common poker. In making this experiment, being willing to try the repulsive force, it once happen'd, that whilst the bar was at one end electrifying, a spoon lay upon the other; and, upon an assistant's pouring some warm spirit into the spoon, the electrical flash from the spoon snapped, and fired the first drop of the spirit; which unexpectedly fired not only the whole jett as it was pouring, but kindled likewise the whole quantity in the pot, in which I usually have it warm'd.

I find, in firing inflammable substances from the finger of a man standing upon wax, that, *cæteris paribus*, the success is more constant, if the man, instead of holding the thread (the use of which I communicated in a former paper) in his hand, the thread is suspended at the end of an iron rod held in one hand, and he touches the spirit with one of the fingers of the other.

If a man, standing upon the electrical cake with a dish or deep plate of water in one hand, and the iron rod with the thread in the other, is made electrical, and a person not electrified touches any part either of the plate or water, the flashes of fire come out plentifully; and wherever you bring your finger very near, the water rises up in a little cone, from the point of which the fire is produced, and your finger, though not in actual contact, is made wet. The same experiment succeeds through three or more people.

In firing inflammable substances, the Person who holds the spoon in his hand to receive the electrical flashes, when the finger of the electrified person is brought near thereto, not only feels a tingling in his hand, but even a slight pain up to his elbow. This is most perceptible in dry weather, when the Electricity is very powerful.

There is considerable difficulty in firing electrics *per se*, such as turpentine and balsam *Capivi*, by the repulsive power of Electricity; because, in this case, these substances will not permit the Electricity to pass through them: therefore, when you would have this experiment succeed, the finger of the person who is to fire them, is to be applied as near to the edge as possible of these substances when warmed in a spoon, that the flashes from the spoon (for these substances will emit none) may

snap,

ſnap, where they are ſpread the thinneſt, and then fire their *effluvia.* This experiment, as well as ſeveral others, ſerves to confute that opinion, which has prevailed with many, that the Electricity floats only upon the ſurfaces of bodies.

If an electrical cake is dipp'd in water, it is thereby made a conductor of Electricity; the water hanging about it tranſmitting the electrical *effluvia* in ſuch a manner, that a perſon ſtanding thereon can by no means be electrified enough to attract the leaf-gold at the ſmalleſt diſtance; though the perſon ſtanding upon the ſame cake when dry, attracted a piece of fine thread hanging at the diſtance of two feet from his finger. We muſt here obſerve, that the cake being of an unctuous ſubſtance, the water will no-where lie uniformly thereon, but adhere in ſeparate *moleculæ*; ſo that, in this inſtance, the Electricity jumps from one particle of water to another, till the whole is diſſipated.

From the appearance of the threads, amongſt which I rub the tube, I can frequently judge, though the ſpirit may be many feet diſtant from them, whether or no it will fire; becauſe, when the perſons ſtanding upon the wax are made electrical enough to fire the ſpirit, the threads repel each other at their lower parts, where they are not confin'd to a conſiderable diſtance; and this diſtance is in proportion as the threads are made electrical.

If two perſons ſtand upon electrical cakes at about a yard's diſtance from each other, one of which perſons, for the ſake of diſtinction, we will call *A*, the other *B*; if *A*, when electrified, touches *B*, *A* loſes almoſt all his Electricity at that touch only, which is received by *B*, and ſtopped by the electrical cake: if *A* is immediately electrified again to the ſame degree as before, and touches *B*, the ſnapping is leſs upon the touch; and this ſnapping, upon electrifying *A*, grows leſs and leſs, till *B*, being impregnated with Electricity, though received at intervals, the ſnapping will no longer be ſenſible.

That glaſs will repel and not conduct the Electricity of glaſs, has been mention'd by others, who have treated of this ſubject; but the experiments to determime this matter muſt be conducted with a great deal of caution; for, unleſs the glaſs tube, intended to conduct the Electricity, be as warm as the external air, it will ſeem to prove the contrary, unleſs in very dry places and ſeaſons. Thus I ſometimes have brought a cold though dry glaſs tube near three foot long into a room where there has been a number of people; when, upon placing the tube upon ſilk lines, and laying ſome leaf-ſilver upon a card at one end, and rubbing another glaſs tube at the other, the ſilver has, contrary to expectation, been thrown off as readily as from an iron rod. At firſt I was ſurprized at this appearance; but then conjectur'd, that it muſt ariſe from the coldneſs of the glaſs, condenſing the floating vapour of the room. In order then to obviate this, I warm'd the tube ſufficiently, and this effect was no longer produc'd, but the ſilver lay perfectly ſtill.

If

If a number of pieces of finely ſpun glaſs, cut to about an inch in length, little bits of fine wire of the ſame length, of what metal you pleaſe, and ſmall cork-balls, are either put all together, or each by themſelves, into a dry pewter plate, or upon a piece of poliſhed metal, they make, in the following manner, a very odd and ſurpriſing appearance. Let a man, ſtanding upon electrical cakes, hold this plate in his hand, with the bits of glaſs, wire, &c. detached from each other, as much as conveniently may be; when he is electrified, let him cauſe a perſon ſtanding upon the ground to bring another plate, his hand, or any other non-electric, exactly over the plate, containing theſe bodies. When his hand, &c. is about 8 inches over them, let him bring it down gently: as it comes near, in proportion to the ſtrength of the Electricity, he will obſerve the bits of glaſs firſt raiſe themſelves upright; and then, if he brings his hand nearer, dart directly up, and ſtick to it without ſnapping. The bits of wire will fly up likewiſe, and as they come near the hand ſnap aloud; you feel a ſmart ſtroke, and ſee the fire ariſing from them to the hand at every ſtroke: each of theſe, as ſoon as they have diſcharged their fire, falls down again upon the plate. The cork-balls alſo fly up and ſtrike your hand, but fall again directly. You have a conſtant ſucceſſion of theſe appearances, as long as you continue to electrify the man in whoſe hand the plate is held; but if you touch any part either of the man or plate, the pieces of glaſs, which before were upon their ends, immediately fall down.

Some few years ago, Sir *James Lowther* brought ſome bladders fill'd with inflammable air, collected from his coal-mines, to the *Royal Society*. This air flamed, upon a lighted candle being brought near it. This inflammability has occaſion'd many terrible accidents. Mr *Maud*, a worthy member of this *Society*, made at that time, by art, and ſhew'd the *Society*, air exactly of the ſame quality. I was deſirous of knowing if this air would be kindled by electrical flaſhes. I accordingly made ſuch air, by putting an ounce of filings of iron, an ounce of oil of vitriol, and four ounces of water, into a *Florence* flaſk; upon which an ebullition enſued, and the air, which aroſe from theſe materials, not only fill'd three bladders, but alſo, upon the application of the finger of an electrified perſon, took flame, and burnt near the top and out of the neck of the flaſk a conſiderable time. When the flame is almoſt out, ſhake the flaſk, and the flame revives. You muſt, with your finger dipped in water, moiſten the mouth of the flaſk as faſt as it is dried by the heat within, or the Electricity will not fire it: becauſe the flaſk, being an electric *per ſe*, will not ſnap at the application of the finger, without the glaſs being firſt made non-electric by wetting. It has ſometimes happen'd, if the finger has been applied before the inflammable air has found a ready *exit* from the mouth of the flaſk, that the flaſh has filled the flaſk, and gone off with an exploſion equal to the firing of a large piſtol; and ſometimes indeed it has burſt the flaſk. The ſame effect is produced from ſpirit of ſea ſalt, as from oil of vitriol; but as the acid of ſea-ſalt is much lighter

lighter than that of vitriol, there is no neceſſity to add the water in this experiment.

Thoſe who are not much acquainted with Chemical Philoſophy, may think it very extraordinary, that, from a mixture of cold ſubſtances, which, both conjunctly and ſeparately, are uninflammable, this very inflammable vapour ſhould be produced. In order to ſolve this, it may not be improper to premiſe, that iron is compounded of a ſulphureous as well as a metallic part. This ſulphur is ſo fixed, that, after heating the iron red hot, and even melting it ever ſo often, the ſulphur will not be diſengaged therefrom: but, upon the mixture of the vitriolic acid, and by the heat and ebullition which are almoſt inſtantly produced, the metallic part is diſſolved, and the ſulphur, which before was intimately connected therewith, being diſengaged, becomes volatile. This heat and ebullition continue, till the vitriolic acid is perfectly ſaturated with the metallic part of the iron; and the vapour, once fired, continues to flame, until, this ſaturation being perfected, no more of the ſulphur flies off.

I have heretofore mentioned, how conſiderably perfectly dry air conduces to the ſucceſs of theſe experiments; but we have been lately informed, by an extract of a letter, that Abbé *Nolet* was of opinion, that they would ſucceed in wet weather, provided the tubes were made of glaſs tinged blue with zaffer. I have procured tubes of this ſort, but, after giving them many candid trials, I cannot think them equal to their recommendation. I firſt tried one of them in a ſmart ſhower of rain after a dry day, when the drops were large, and the ſpirit fired 3 times in about 4 minutes: the ſame effect ſucceeded, under the ſame circumſtances, from the white one; but, after 3 or 4 hours raining, when the air was perfectly wet, I never could make it ſucceed. And, to illuſtrate this matter further, I have been able, when the weather has been very dry, with once rubbing my hand down this blue tube, and applying it to the end of an iron rod 6 feet long, to throw off ſeveral pieces of leaf-ſilver lying upon a card at the other end of this rod; whereas I never have been able to throw it off by any means in very wet weather. Beſides, I am of opinion, that, after the electrical fire is gone from the tube, the tube has no ſhare in the conducting of it: my ſentiments on that head I laid before you in a former paper: for if the ſilk lines are wetted, they diffuſe all the Electricity; and the ſame effects happen, when the air is wet, be your glaſs of what colour it will.

It may not be improper here to obſerve, that zaffer, which is uſed by the Glaſs-makers and Enamellers, is made of cobalt or mundick calcined after the ſubliming the flowers. This being reduced to a very fine powder, and mixt with twice or thrice it's own weight of finely powder'd flints, is moiſtened with water, and put up in barrels, in which it ſoon runs into an hard maſs, and is called zaffer.

A dry ſponge hanging by a packthread at the end of an electrified ſword, or from the hand of an electrified man, gives no ſigns of being made

made electrical: if it is well ſoak'd in water, wherever it is touch'd, you both ſee and feel the electrical ſparks. Not only ſo, but, if it is ſo full of water that it falls from the ſponge, thoſe drops in a dark room, receiv'd upon your hand, not only flaſh and ſnap, but you perceive a pricking pain. If you hold your hand, or any non-electrical ſubſtances, very near the water, which had ceaſed dropping when the ſponge was not electrified, drops again upon it's being electrified, and the drops fall in proportion to the receiv'd Electricity, as though the ſponge were gently ſqueez'd between your fingers. I was deſirous to know if I was able to electrify a drop of cold water, dropping from the ſponge, enough to fire the ſpirit; but, after many unſucceſsful trials, I was forced to deſiſt; becauſe the cold water dropping from the ſponge not only cool'd the ſpirit too much, but alſo render'd it too weak: likewiſe every drop carried with it great part of the Electricity from the ſponge.

I then conſider'd, in what manner I could give a tenacity to the water ſufficient to make the drops hang a conſiderable time; and this I brought about by making a mucilage of the ſeeds of fleawort. A wet ſponge then, ſqueez'd hard, and fill'd with this cold mucilage, was held in the hand of an electrified man, when the drops, forced out by the Electricity, aſſiſted by the tenacity of the liquor, hung ſome inches from the ſponge; and by a drop of this, I fired not only the ſpirit of wine, but likewiſe the inflammable air before-mentioned, both with and without the exploſion. What an extraordinary effect is this, that a drop of cold water (for the ſeeds contribute nothing, but add conſiſtence to the water) ſhould be the *medium* of fire and flame?

Camphire is a vegetable reſin, and, of conſequence, an electric *per ſe*. This ſubſtance, notwithſtanding it's great inflammability, will not take fire from the finger of a man, or any other body electrified, tho' made very warm, and the vapours ariſe therefrom in great abundance; becauſe, neither electrics *per ſe* excited, or electrified bodies, exert their force by ſnapping upon electrics *per ſe*, though not excited. If you break camphire ſmall, and warm it in a ſpoon, it is not melted by heat like other reſins; but, if that heat were continued, it would all prove volatile. To camphire thus warm'd, the finger of an electrified man, a ſword, or ſuch-like, will, in ſnapping, exert it's force upon the ſpoon, and the circumambient vapour of the camphire will be fired thereby, and light up the whole quantity expoſed. The ſame experiment ſucceeds by the repulſive power of Electricity.

A poker, thoroughly ignited, put into ſpirit of wine, or into the diſtilled oil of vegetables, produces no flame in either. It indeed occaſions the vapors to ariſe from the oil in great abundance; but if you electrify this heated poker, the electrical flaſhes preſently kindle flame in either. The experiment is the ſame with camphire. Theſe experiments, as well as the following, ſufficiently evince, that the electrical fire is truly flame, and that extremely ſubtil.

I have

I have made feveral trials in order to fire gunpowder alone, which I tried both warm and cold, whole and powder'd, but never could fucceed: and this arifes, in part, from it's vapours not being inflammable, and in part from it's not being capable of being fir'd by flame; unlefs the fulphur in the compofition is nearly in the ftate of accenfion. This we fee, by putting gunpowder into a fpoon with rectified fpirit, which, when lighted, will not fire the powder, till, by the heat of the fpoon from the burning fpirit, the fulphur is almoft melted. Likewife, if you hold gunpowder ground very fine in a fpoon over a lighted candle, or any other flame, as foon as the fpoon is hot enough to melt the fulphur, you fee a blue flame, and inftantly the powder flafhes off. The fame effects are obferved in the *Pulvis fulminans*, compofed of nitre, fulphur, and fixed alkaline falt. Befides, when the gunpowder is very dry, and ground very fine, it (as you pleafe to make the experiment) is either attracted or repell'd; fo that, in the firft cafe the end of your finger, when electrified, fhall be cover'd over with the powder, though held at fome diftance; and in the other, if you electrify the powder, it will fly off at the approach of any non-electrified fubftance, and fometimes even without it. But I can, at pleafure, fire gunpowder, and even difcharge a mufket, by the power of Electricity, when the gunpowder has been ground with a little camphire, or with a few drops of fome inflammable chemical oil. This oil fomewhat moiftens the powder, and prevents it's flying away: the gunpowder then being warm'd in a fpoon, the electrical flafhes fire the inflammable vapour, which fires the gunpowder: but the time between the vapour firing the powder is fo fhort, that frequently they appear as the fame, and not fucceffive operations, wherein the gunpowder itfelf feems fired by the Electricity: and, indeed, the firft time this experiment fucceeded, the flafh was fo fudden and unexpected, that the hand of my affiftant, who touch'd the fpoon with his finger, was confiderably fcorch'd. So that there feems a fourth ingredient neceffary to make gunpowder readily take fire by flame; and that fuch a one as will heighten the inflammability of the fulphur.

In common cafes, the lighted match, or the little portion of red-hot glafs, which falls among the powder, and is the refult of the collifion from the flint and fteel, fires the charcoal and fulphur, and thefe the nitre. But if to thefe three ingredients you add a fourth, *viz.* a vegetable chemical oil, and gently warm this mixture, the oil, by the warmth, mixes intimately with the fulphur, lowers it's confiftence, and makes it readily take fire by flame.

In thefe operations, notwithftanding I always made ufe of the fineft-fcented oils of Orange-peel, Lemons, and fuch-like, yet, upon the leaft warming the mixture, the rank fmell of balfam (*i. e.* of the ready folution of fulphur) was very obvious.

Further Experiments and Observations, by the same N°. 478. p. 41. Jan. *and* Feb. 1746. *Read* Feb. 6. 1745-6.

8. * As water is a non-electric, and of consequence a conductor of Electricity, I had reason to believe, that ice was endow'd with the same properties. Upon making the experiment I found my conjectures not without foundation; for, upon electrifying a piece of ice, where-ever the ice was touch'd by a non-electric, it flashed and snapped. A piece of ice also, held in the hand of an electrify'd man, as in the beforementioned processes, fired warm spirit, chemical vegetable oils, camphire, and gunpowder prepared as before. But here great care must be taken, that by the warmth of the hand, or of the air in the room, the ice does not melt; if so, every drop of water therefrom considerably diminishes the received Electricity. In order to obviate this, I caused my assistant, while he was electrifying, to be continually wiping the ice dry upon a napkin hung to the buttons of his coat; and this being electrified as well as the ice, prevented any loss of the force of the Electricity. The experiment will succeed likewise, if, instead of the ice, you electrify the spirit, *&c.* and bring the ice not electrified near them. I must observe, that ice is not so ready a conductor of Electricity as water; so that I very frequently have been disappointed in endeavouring with it to fire inflammable substances, when it has been readily done by a sword, or the finger of a man.

In my first paper I took notice of my having observed two different appearances of the fire from electrified substances; *viz.* those large bright flashes, which may be procured from any part of electrified bodies, by bringing a non-electric unexcited near them, and with which we have fired all the inflammable substances mention'd in the course of these observations; and those, like the firing of wet gunpowder, which are only perceptible at the points or edges of excited non-electrics. These last also appear different in colour and form, according to the substances from which they proceed: for, from polish'd bodies, as the point of a sword, a silver probe, the points of scissors, and the edges of the steel bar made magnetical by the ingenious Dr *Knight*, the electrical fire appears like a pencil of rays, agreeing in colour with the fire from *Boyle*'s *Phosphorus*; but from unpolished bodies, as the end of a poker, a rusty nail, or such-like, the rays are much more red. The difference of colour here, I am of opinion, is owing rather to the different reflection of the electrical fire from the surface of the body, from which it is emitted, than to any difference in the fire itself. These pencils of rays issue successively as long as the bodies, from which they proceed, are exciting; but they are longer and more brilliant, if you bring any non-electric not excited near them, though it must not be close enough to make them snap. If you hold your hand at about two or three inches distance from these points, you not only feel successive blasts of wind from them, but hear also a crackling noise. Where there are several points, you observe at the same time several pencils of rays.

* This Paper is reprinted, with some mistakes, in N°. 484. p. 695, *& seq.*

It

It appears, from experiments, that besides the several properties that Electricity is possess'd of peculiar to itself, it has some in common with magnetism and light.

In common with Magnetism, Electricity counteracts, and in light substances overcomes the force of gravity. Like that extraordinary power likewise, it exerts it's force *in vacuo* as powerfully as in open air, and this force is extended to a considerable distance through various substances of different textures and densities. PROP. I.

Gravity is the general endeavour and tendency of bodies towards the centre of the earth: this is over-come by the magnet, with regard to iron, and by Electricity, with regard to light substances, both in it's attraction and repulsion; but I have never been able to discern that vortical motion, by which this effect was said to be brought about by the late Dr *Desaguliers*, and others, having no other conception of it's manner of acting than as rays from a centre, which indeed is confirmed by several experiments: one of which, very easy to be tried, is, that if a single downy seed of cotton-grass is dropped from a man's hand, and in it's fall comes within the attraction of the rubbed tube; the down of this seed, which before seemed to stick together, separates, and forms rays round the centre of the seed: or if you fasten many of these seeds, with mucilage of gum-arabic, round a bit of stick, the down of them when electrified, which otherwise hangs from the stick, is raised up, and forms a circular appearance round the stick. As these light bodies are directed in their motions only by the force impressed upon them, and as their appearance is constantly *radiatim*, such appearance by no means squares with our idea of a *vortex*. COROL.

Some have imagined a polarity also, when they have observed one end of an excited glass tube repel light substances, and the other attract them; but this is a deception, arising from the whole length of the tube not being excited, but only such part of it as has been rubb'd; so that as much of the tube as is held in the hand remains in an unexcited state, and permits light substances to lie still thereon, though forcibly repell'd at the other end. This attractive power of Electricity acts not only upon non-electrics, as leaf-gold, silver, thread, and such-like, but also upon originally-electrics, as silk, dry feathers, little pieces of glass, and resin: it attract all bodies, that are not of the same standard of Electrity (if I may be allowed the expression), as the excited body from which it proceeds. I found no body, however dense, whose pores are not pervious to Electricity, by a proper management, not even gold itself.

In common with light, Electricity pervades glass, but suffers no refraction therefrom; I having, from the most exact observations, found it's direction to be in right lines, and that through glasses of different forms, included one within the other, and large spaces left between each glass. PROP. II.

Corol. This rectilineal direction is obſervable only as far as the Electricity can penetrate through unexcited originally-electrics, and thoſe perfectly dry; nor is it at all material, whether theſe ſubſtances are tranſparent, as glaſs; ſemidiaphanous, as porcelain, or thin cakes of white wax; or quite opaque, as thick woollen cloth, as well as woven ſilk of various colours; it is only neceſſary that they be originally-electrics. But the caſe is widely different with regard to non-electrics; wherein the direction, given to the Electricity by the excited originally-electric, is alter'd, as ſoon as it touches the ſurface of a non-electric, and is propagated with a degree of ſwiftneſs ſcarcely to be meaſured in all poſſible directions to impregnate the whole non-electric maſs in contact with it, or nearly ſo, however different in itſelf; and which muſt of neceſſity be terminated by an originally-electric, before the Electricity exerts the leaſt attraction; and then this power is obſerved firſt at that part of the non-electric the moſt remote from the originally-electric. Thus, for example, by an excited tube held over it, leaf-gold will be attracted through glaſs, cloth, *&c.* held horizontally in the hand of a man ſtanding upon the floor, and this attraction is exerted to a conſiderable diſtance. On the contrary, the rubbed tube will not attract leaf-gold, or other light bodies, however near, through ſilver, tin, the thinneſt board, paper, or any other non-electric, held in the manner beforementioned. But if you rub the paper over with wax melted, and by that means introduce the originally-electric therein, you obſerve the Electricity acts in right lines, and attracts powerfully. And here I muſt beg leave to remind you, not only of the former corollary, but of ſome of the former experiments alſo; by which it appears, that although, to make a non-electric exert any power, we muſt excite the whole maſs thereof, yet we can excite what part, and what only, of an originally-electric we pleaſe. Thus we obſerve, that leaf-gold, and the ſeed of cotton-graſs (which grows upon boggs, and is a very proper ſubject for theſe inquiries), are attracted under a glaſs jar made warm *, and turned bottom upwards, upon which are placed books, and ſeveral other non-electrics; and that the motions of the light bodies underneath correſpond with the motions of the glaſs tube held over them, the Electricity ſeeming inſtantaneouſly to paſs through the books and the glaſs. But this does not happen, till the Electricity has fully impregnated the non-electrics, which lie upon the glaſs; which received Electricity is ſtopped by the glaſs; and then theſe non-electrics dart their power directly through the upper part of the glaſs, after the manner of origi-

* I have conſtantly obſerved, that the electrical attraction through glaſs is much more powerful when the glaſs is made warm, than when cold. This effect may proceed from a twofold cauſe: firſt, warm glaſs does not condenſe the water from the air, which makes the glaſs, as has been before demonſtrated, a conductor of Electricity: ſecondly, as heat enlarges the dimenſions of all known bodies, and, conſequently cauſes their conſtituent parts to recede from each other, the electrical *effluvia*, paſſing in ſtrait lines, find, probably, a more ready paſſage through their pores.

nally-

nally-electrics. But if the thinnest non-electric, even the finest paper, as I before mentioned, is held in the hand of a man at the smallest distance over the leaf-gold, and the Electricity is not stopped, not the least power will be exerted, and the gold will lie still. I must here remark likewise, that this law of Electricity is so constant and regular, that I have not found one deviation from it; so that even the quicksilver, spread thin, as it usually is at the back of a plate of a looking-glass, will prevent the passing thro' of the electrical attraction, unless stopped by an originally-electric This penetration of the electrical power through originally-electrics is much greater than has hitherto been imagined, and has caused the want of success to great numbers of experiments. I have been at no small pains to determine, how far this power can penetrate through a dry originally-electric; and have found, by repeated trials, that either in a cake of wax alone, or of wax and resin mixed, when the Electricity is very powerful, it has passed, I say, in strait lines through these cakes of the thickness of 2 inches and $\frac{4}{10}$; but I never could make it act through one of 2 inches and $\frac{6}{10}$; for in this it was perfectly stopped. So that the cakes commonly made use of to stop the Electricity, by being too thin, suffer a considerable quantity of the electrical power to pervade them, and be lost in the floor. I make no doubt if the electrical power could be more increased, it would penetrate much further through these originally-electric bodies.

Electricity in common with light likewise, when it's forces are collected, and a proper direction given thereto, upon a proper object, produces fire and flame. Prop. III.

The fire of Electricity (as I have before observed) is extremely delicate; and sets on fire, as far as I have yet experienced, only inflammable vapours. Nor is this flame at all heightened, by being superinduced upon an iron rod, red-hot with coarser culinary fire, as in a preceding experiment; nor diminished by being directed upon cold water However I was desirous of knowing, if this flame would be affected by a still greater degree of cold; and in order to determine this, I made an artificial cold; by which the mercury, in a very nice Thermometer adjusted to *Fahrenheit*'s scale, was depressed in about 4', from 15° above the freezing point to 30° below it; that is, the mercury fell 45°. From this cold mixture, when electrified, the flashes were as powerful, and the stroke as smart, as from the red-hot iron. I could have made the cold more intense, but the above was sufficient for my purpose. This experiment seems to indicate, that the fire of Electricity is affected neither by the presence or absence of other fire. For as red-hot iron, by Sir *I Newton*'s scale of heat, is fixed at 192°, and as the *ratio* between Sir *Isaac*'s degrees and *Fahrenheit*'s is as 34 to 180, it necessarily follows, that the difference of heat between the hot iron and the cold mixture is 1040°; and nevertheless this vast difference makes no alteration in the appearance of the electrical flame. We find likewise, that as the fire, arising from the refraction of the rays of light Corol.

by

by a *Lens*, and brought to a *Focus*, is obſerved, firſt, at ſome ſmall diſtance from their ſurfaces, to ſet on fire combuſtible ſubſtances; the ſame effect, as I have before obſerv'd, is produced in like manner by electrical flame.

I may perhaps be thought too minute in ſome of the before-mentioned particulars; but, in inquiries abſtruſe as theſe are, where we have ſo little *à priori* to direct us, the greateſt attention muſt be had to every circumſtance, if we are truly deſirous of inveſtigating the laws of this ſurpriſing power. For, as has been ſaid upon another occaſion, by my ever honoured friend *Martin Folkes*, Eſq; our moſt worthy *Preſident*, " That Electricity ſeems to furniſh an inexhauſtible fund for " inquiry: and ſure *phænomena* ſo various, and ſo wonderful, can ariſe " only from cauſes very general and extenſive; and ſuch as muſt have " been deſigned by the Almighty AUTHOR of nature for the produc- " tion of very great effects, and ſuch as are of great moment to the " ſyſtem of the univerſe."

§ 1. 9. The favourable reception wherewith you honour'd ſome papers I laid before you ſome time ſince, relating to Electricity, emboldens me to trouble you again upon the ſame ſubject: and I am the more encouraged ſo to do, as the progreſs of our diſcoveries therein, both here and abroad, has been ſo rapid; that what, little more than a year ago, we conceived to be the *ne plus ultra* of our inquiries, is now regarded as mere rudiments.

A Sequel to the Experiments and Obſervations in a letter to the Royal Society *from the ſame* N°. 484 p. 704. Oct. &c. 1747. *Read* Oct. 30. 1746.

§ 2. It were treſpaſſing too much upon you, to recount the great number of experiments I have made; for which reaſon I ſhall only take notice of ſuch as are either in themſelves ſtriking, or tend to illuſtrate ſome propoſition.

§ 3. At the beginning of laſt ſummer I cauſed a machine to be made for electrical purpoſes; the wheel whereof was four feet in diameter. In the periphery of this wheel were cut four grooves, correſponding with four globes of ten inches diameter, which were diſpoſed vertically at about 3 inches diſtance from each other. One, two, or the whole number of theſe globes might be uſed at pleaſure. They were mounted upon ſpindles of two inches diameter, and their mean motion round their axis was about 1100 times in a minute. As it is next to impoſſible to have theſe globes blown and mounted perfectly true, I order'd the leather cuſhions, with which they were rubb'd, to be ſtuffed with an elaſtic ſubſtance (curled hair) that the globes in their rotations might be as equally rubb'd as poſſible. You might likewiſe cauſe the globes to be rubb'd by the hands of your aſſiſtants; but under a certain treatment (of which hereafter) the cuſhions excite equally ſtrong. The leather cuſhions were now and then rubb'd over with whiting. As a minute detail of the parts of this machine would take up too much of your time, I have herewith laid before you a draught thereof.

I lined

I lined one of these globes to a considerable thickness, with a mixture of wax and resin, in order to observe whether or no the Electricity would be the sooner or more strongely excited; but I found no difference in the power of this globe from the others, which were without this treatment. § 4.

The power of Electricity is increased by the number and size of the globes to a certain degree; but by no means in proportion to their number and size: therefore, as the bodies to be electrified, will contain only a certain quantity of Electricity, of which more largely hereafter; when that quantity is acquired, which is soonest done by a number of globes, the surcharge is dissipated as fast as it is excited. § 5.

After the globes had been a few times used, I found myself master of a much greater quantity of electrical power, with much less labour to myself, than when I used only tubes. I could attract and repel light substances at a much greater distance than before; fire spirits of wine, camphire, and all other substances whose vapours were inflammable, with great ease, and at any distance, with non-electrics placed upon originally-electrics: I could fire them, I say, at all times; though not equally easy, when the weather was moist. § 6.

I discover'd with this machine, and communicated to several Members of this *Society*, several of the experiments said to be first made by M. *le Monnier* at *Paris*, before the letter communicating them was received by our *President* from thence. § 7.

I order'd another machine to be made for a friend of mine, which carried a globe of 16 inches diameter. I united the power of this large globe with that of 3 of the others before-mention'd, and found the strokes from the excited non-electrics not increased according to my expectation. In two experiments indeed, where the dissipation of the whole power of these globes was visible as fast as it was excited, the effect of this additional globe was very considerable. The first was, when two pewter plates were held, one in the hand of an electrified man, and the other by one standing upon the floor: when these plates were brought near each other, the flashes of perfectly pure and bright flame were so large, and succeeded each other so fast, that, when the room was darken'd, I could distinctly see the faces of 13 people who stood round the room. The other was from a piece of large blunt wire hanging to the gun-barrel; from the end of which, when electrified, and any black * non-electric unexcited was brought near, though not near § 8.

* In the course of these observations, whenever I mention either originally-electrics or non-electrics, I always understand the whole *genus* of each. Thus when I mention a man placed upon originally-electrics, I am indifferent whether he is suspended either in lines of dry silk, hair, or wool; or (which is much more convenient) if he stands upon glass, wax, resin, pitch, sulphur, *&c.* or upon different mixtures of these, if of a sufficient thickness. As we are now masters of a greater electrical power than heretofore, I have found the Electricity pervade, tho' in very small quantity, originally-electrics of above four inches diameter.

enough

enough to cause a snap, a brush of blue lambent flame, totally different from the former, was very conspicuous when the room was dark, of more than an inch long and an inch thick. I mention that what is held near the bottom of the wire should be black, because then you see this flame more sharp. Here the phosphoreal smell might be perceived at a considerable distance. If the back of your hand was brought so near this wire as to occasion a snap, and these snaps were received for some time, you would feel them like so many punctures upon your skin, occasioning red spots, which have lasted 24 hours.

§ 9. If, when a person is electrified, he brings his hand upon the cloaths of one that is not, they both have a sensation exactly resembling that of many pins running into the skin, which continues as long as the globes are in motion. This is most perceptible when the cloaths are of thin woollen cloth or silk, animal substances; less so, when of linen or cotton, which are vegetable.

§ 10. If some oil of turpentine is set on fire in any vessel held in the hand of an electrified man, the thick smoke that arises therefrom receiv'd against any non-electric of a large surface, held in the hand of a second man standing upon an electrical cake; this smoke, I say, at a foot distance from the flame, will carry with it a sufficient quantity of Electricity for the second man to fire any inflammable vapour. The electrical strokes have been likewise perceptible upon the touching the second man, when the non-electric held in his hand has been in the smoke of the oil of turpentine between 7 and 8 feet above the flame. Here we find the smoke of an originally-electric a conductor of Electricity.

§ 11. Likewise if burning spirit of wine be substituted in the place of oil of turpentine, and if the end of an iron rod in the hand of the second man be held at the top of the flame, this second man will kindle other warm spirits held near his finger. Here we find that flame conducts the Electricity, and does not perceptibly diminish it's force.

§ 12. These two experiments demonstrate, that the opinion of those is erroneous, who suppose the electrical *effluvia* to be of a sulphureous nature; and that these themselves are set on fire at the snapping observ'd, when you bring non-electrics unexcited to those that are. If their opinions were true, the electrical *effluvia* should be destroyed by the flame in both the preceding experiments; the contrary of which is observed.

§ 13. I now proceed to take notice of that surprising effect, that extraordinary accumulation of the electrical power in a phial of water, first discover'd by Professor *Musschenbroek*, a man born to penetrate into the deepest mysteries of Philosophy: and I hope I shall stand excused, if I enter into a minute detail of the circumstances relating thereto. The experiment is, that a phial of water is suspended to a gun-barrel by a wire let down a few inches into the water through the cork; and this gun-barrel, suspended in silk lines, is applied so near an excited glass globe, that some metallic fringes inserted into the gun-barrel touch the globe in motion. Under these circumstances a man grasps the phial with one hand, and touches

the

the gun-barrel with a finger of the other. Upon which he receives a violent ſhock through both his arms, eſpecially at his elbows and wriſts, and acroſs his breaſt. This experiment ſucceeds beſt, *cæteris paribus*,

1. When the air is dry.
2. When the phial containing the water is of the thinneſt glaſs.
3. When the outſide of the phial is perfectly dry.
4. In proportion to the number of points of non-electric contact. Thus if you hold the phial only with your thumb and finger the ſnap is ſmall; larger when you apply another finger, and increaſes in proportion to the graſp of your whole hand.
5. When the water in the phial is heated; which being then warmer than the circumambient air, may not occaſion the condenſing the floating vapour therein upon the ſurface of the glaſs.

From theſe conſiderations it is to be obſerv'd, that this effect ariſes from electrifying the non-electric water, included in the originally-electric glaſs; ſo that whatever tends to make the outſide of the glaſs non-electric by wetting it, as, a moiſt hand, damp air, or the water from the inſide of the phial, defeats the experiment, by preventing the requiſite accumulation of the electrical power. § 14.

That a gun-barrel is abſolutely neceſſary to make this experiment ſucceed, is imaginary; a ſolid piece of metal of any form is equally uſeful. Nor have I yet found, that the ſtroke is in proportion to the quantity of electrified matter; having obſerved the ſtroke from a ſword as violent as that from a gun-barrel with ſeveral excited iron bars * in contact with it. § 15.

I have tried the effect of increaſing the quantity of water in glaſſes of different ſizes, as high as four gallons, without in the leaſt increaſing the ſtroke. If † filings of iron are ſubſtituted in the room of water, the effect is conſiderably leſſen'd. If mercury, much the ſame as water; the ſtroke is by no means increaſed in proportion to their ſpecific gravities, as might have been imagined ‖. § 16.

The phial ſhould not be leſs than can conveniently be graſped. I generally make uſe of thoſe, which hold ſeven or eight ounces, and fill them about four fifths with water; and the ſtroke from one of theſe, under the ſame circumſtances, is equally ſtrong with that of a *Florence* flaſk held in the hand, which I have ſometimes made uſe of; though § 17.

* If of ſix men touching each other, and ſtanding upon originally-electrics, one touches the gun barrel, the whole are electrified; all theſe then muſt be conſider'd, as ſo much excited non-electric matter. From the aggregate of all theſe, not more fire is viſible upon the touch than from either of them ſingly.

† For a further account of the filings of iron, made uſe of in this experiment, ſee Art. 28. § 14. *

‖ In this experiment, and in others, wherein we aſſert, that the ſtroke is not increaſed in proportion to the quantity of electrified matter; it muſt always be underſtood, that the excited non electrics themſelves are touched, without being contained in originally-electrics, as water in the glaſs; for otherwiſe (as will hereafter be ſpecified) the effects of different quantities of matter will be very different.

 the

the glafs of this laft is equally thin with that of the phial, and the quantity of water four times as much. That the ftroke therefore is not as the quantity of water electrified, is evident from this experiment. This fact does not depend upon my judgment alone, but likewife upon the opinions of feveral learned Members of this *Society*, who have experienced the greater and lefs quantity of water.

§ 18. If a dry twig of birch, or any other wood, be run through the cork inftead of the metallic wire, the ftroke is not greater than is ufually felt from the gun-barrel without the application of the water. The ftroke is likewife leffen'd, if the phial is held in the hand with a glove on.

§ 19. After the gun-barrel and phial have been fufficiently excited, which is done in a few feconds, the furcharge is diffipated; fo that the continuing the motion of the machine ever fo long after the faturation is complete, does not increafe the electrical force.

§ 20. The force of the ftroke from the electrified phial does not increafe in proportion to the dimenfions of the glafs, or the number of globes employed. I have been ftruck as forcibly with one phial from a globe of 7 inches diameter, as when I made ufe of, at the fame time, one of 16 inches, and 3 of ten. I have been lately informed, that at *Hamburgh* a fphere was employed for this purpofe a *Flemifh* ell in diameter, without the expected increafe of power.

§ 21. When the phial is well electrified, and you apply your hand thereto, you fee the fire flafhes from the outfide of the glafs wherever you touch it, and crackles in your hand.

§ 22. The phial may be electrified by applying the wire therein to the globe in motion; after which, if it is grafped in one hand, and the wire touched with a finger of the other, the ftroke is as great as from the gun-barrel. If you only bring your finger near the end of the wire without touching it, you obferve the fame brufh of blue flame, as from the wire hanging to the gun-barrel, before taken notice of. This inftantly difappears upon touching the wire, though you do not receive a fhock, unlefs at the fame time you grafp the phial.

§ 23. If you grafp the phial with your hand, and do not at the fame time touch the wire, the acquired Electricity of the water is not diminifhed. So that, unlefs by accident or otherwife the wire is touched, the electrified water will contain it's force many hours, may be conveyed feveral miles, and afterwards exert it's force upon touching the wire.

§ 24. If, when the machine is in motion, the phial is hung upon the gun-barrel, no increafe of the ftroke is perceived upon touching the gun-barrel with your finger, unlefs at the fame time the phial is taken in the hand.

§ 25. If, when the gun-barrel and phial are excited, you grafp the phial with one hand, and touch the gun-barrel with a piece of any metal held in the other, the fhock is as great in your arms as though you touched the gun-barrel with your finger; but not the leaft fhock is felt, if, inftead of metal, you touch the gun-barrel with a piece of dry wood.

I have

I have felt a very great ſtroke, when I hung two phials to the gun-barrel, and, graſping them both, brought my forehead near it. The ſhock then was ſo violent, that I ſeem'd ſtunn'd, as though ſtruck on the head with a great ſtick, and I have never ſince choſen to repeat this experiment. This increaſe of the electrical force was owing to the additional phial, whereby the points of non-electric contact were augmented. § 26.

Likewiſe if a perſon placed upon originally-electrics, graſps two phials, as before-mentioned, and a ſecond perſon, ſtanding upon the floor, touches any part of his body, a very ſlight ſtroke only is perceived. But if the ſecond perſon, while the globes are in motion, places one of his fingers upon the hand, or any part of the naked body of the firſt, and at the ſame time touches the gun-barrel with his other hand; both feel a ſhock equal to that juſt now mention'd, but more tolerable, becauſe not felt in the head, in the arms only, and acroſs the breaſt. In this experiment, it is not neceſſary that the outſide of the glaſſes held in the hands ſhould be dry, as in the former experiments; becauſe whatever by the moiſture is communicated to the man, is ſtopped by the originally-electrics upon which he is placed. If, inſtead of his hand, you gently touch the firſt perſon's cloaths, you only perceive a ſmall ſtroke upon your finger; but if you preſs his cloaths cloſe to his body, you frequently perceive a double ſtroke; the one, ſlight from his cloaths; the ſecond, a violent ſhock from his body. § 27.

Upon ſhewing ſome experiments to Dr *Bevis*, to prove my aſſertion that the ſtroke was, *cæteris paribus*, as the points of contact of non-electrics to the glaſs, that ingenious Gentleman has very clearly demonſtrated it likewiſe by the following experiment: he wrapped up two large round-bellied phials in very thin lead ſo cloſe as to touch the glaſſes every-where, except their necks. Theſe were filled with water, and cork'd, with a ſtaple of ſmall wire running through each cork into the water. A piece of ſtrong wire about 5 inches long, with an eye at each end, was provided, and at each end of this hung one of the phials of water by the ſmall ſtaple running through the cork. A ſmall wire loop then was faſten'd into the lead at the bottom of each phial, and into theſe loops was inſerted a piece of ſtrong wire like the former. If then theſe phials were hung acroſs the gun-barrel and electrified, and a perſon ſtanding upon the floor touched the bottom wire with one hand, and the gun-barrel with the other, he received a moſt violent ſhock through both his arms, and acroſs his breaſt. § 28.

Theſe phials may be concealed, and the ſhock be more univerſal, in the following manner: the phials may be placed in a corner of the room, and any thing laid over them, ſo as not to touch the upper wires; then a very fine wire muſt be ſuſpended to the gun-barrel, and faſten'd to the upper ſtrong wire. A ſecond piece of ſmall wire, of a ſufficient length to reach from the phials almoſt under the gun-barrel, muſt be faſten'd to the lower ſtrong wire, and this may be conceal'd under a floor- § 29.

cloth. The phials then are electrified; and if a perſon, placing his foot upon the floor-cloth over the wire which comes from the bottom of the phials, touches the gun-barrel, he receives a moſt terrible ſhock. The firſt time I experienced it, was when the phials were fully electrified, and both my feet were placed upon the wire. Upon receiving the ſtroke from the gun-barrel upon my finger, it ſeemed to me, uſed as I am to theſe trials, as though my arm were ſtruck off at my ſhoulder, elbow, and wriſt; and both my legs, at the knees, and behind near the ankles. So that, to try the effects of this experiment, you muſt be careful of not electrifying the phials too much. If a dozen or more of theſe phials, or one very large bottle, were cover'd over with thin lead in the above manner, and ſtrongly electrified, and this Electricity were diſcharged by a man at once in the manner here mention'd, I ſhould dread the conſequences.

§ 30. We muſt obſerve, that this ſhock is not felt, unleſs the wire, coming from the bottoms of the bottles, is touched; and then not, if the ſhoes are dry, and of conſequence originally-electric. In this experiment we ſee the effects of the increaſe of the points of contact; and it ſeems the more ſurpriſing to thoſe who are not acquainted with the cauſe, when the wire is concealed under a floor-cloth, that the moving of their feet only one inch, ſhould occaſion them, all other circumſtances apparently the ſame, to feel a violent ſhock, or none at all. A thick carpet, inſtead of a floor-cloth, is liable to prevent the ſucceſs of this experiment, for the ſame reaſon as dry ſhoes. This experiment may aptly enough be called, the ſpringing an electrical mine.

§ 31. If, in the former experiment, the lower ſmall wire is faſten'd to an iron rod; and if, when the phials are ever ſo ſtrongly excited, that rod is held in the hand of a man ſtanding upon the floor, and with it he touches the gun-barrel, he perceives no ſhock, for reaſons preſently to be aſſigned. But if he takes this iron rod in one hand, and touches the gun-barrel with the other, he then is violently ſtruck. We muſt here obſerve, that the violence of the ſtroke is always felt in our bodies, in proportion to the loudneſs of the exploſion, and the quantity of fire ſeen. Therefore, as both theſe are equally perceptible, whether the Electricity paſſes only thro' the iron, as in the firſt of theſe inſtances, or thro' our bodies equally with the iron, as in the ſecond; we conclude, that in both there is the ſame degree of electrical force. By the firſt of theſe methods you are capable of making others ſenſible of the electrical force, without feeling it yourſelf. This experiment, as well as the laſt, will admit of infinite variation.

§ 32. If a man, ſtanding upon an electrical cake, takes the phial ſuſpended to the gun-barrel in his hand, by theſe means he acquires ſome electrical power; for if, under theſe circumſtances, he touches the gun-barrel, he only receives a ſlight ſtroke. If then, without having had any communication with unexcited non-electrics, he touches the gun-barrel again, the globes being yet in motion, he receives no ſtroke at all.

If

If to the gun-barrel an egg, either raw or boiled, is fufpended by a piece of wire, and a perfon, grafping the electrified phial in one hand, brings the palm of his other near the bottom of the egg; at that inftant he receives a fmart ftroke, and his hand feems full of a more red fire than is ufually obferved. In this experiment the ftroke is more confined to the hand without fhocking the arms, than when you touch the gun-barrel itfelf; it more refembles a ftroke over the hand with a *ferula*. § 33.

If any number of people ftand upon originally-electrics, and communicate with each other by any non-electric medium, efpecially metal, they are by thefe means all equally electrified; and if a perfon ftanding upon the floor, and holding the phial of water hanging to the gun-barrel in his hand, touches the perfon furtheft from the gun-barrel, the whole number receives a fhock equal to any one touching the gun-barrel fingly. § 34.

If a number of perfons, how great foever, ftand upon the ground, communicating with each other as before, the firft of which grafps the phial, and the laft touches the gun-barrel, the whole number receive a fhock like the former. This, we are inform'd, M. *le Monnier* at *Paris* communicated through a line of men, and other non-electrics, meafuring nine hundred toifes. § 35.

Several experiments fhew, that the electrical force always defcribes a circuit; *e. g.* if a man holds the electrified phial in one hand, and touches the gun-barrel with the other, he feels the fhock in no other parts of his body than in his arms, and acrofs his breaft. So that here we fee the electrical power darts *rectiffimo curfu* between the gun-barrel and phial. This is more particularly demonftrated by the following experiment, in which, though the two lines of perfons may be of any length, we only fpecify, that each confifts of 4, for the fake of perfpicuity. § 36.

Of one line, let *A* touch the gun-barrel, ftanding upon wax, and communicate with *BCD* likewife ftanding upon wax. Of the other line, let 1 take the electrified phial in his hand, and join with 2, 3, and 4, all ftanding upon the floor. If, under thefe circumftances, the firft line is electrified, and 4 touches *D*, all eight are ftruck through. If 4 touches *C*, *D*, though electrified, feels nothing, and the remaining feven are ftruck; fo that here *D* is left out of the circuit. If 4 touches *B*, only fix feel the fhock, and *C* and *D* feel nothing; and thus you may proceed to *A*, who muft always neceffarily feel, if either himfelf or any of his line is touched. If, when both lines are as before-mentioned, *D* touches 3, 4 is left out of the circuit, and the remaining feven feel the ftroke. If *C* touches 2, the circuit confifts of five, *D*, 3, and 4 being, though under the fame circumftances, left out: always obferving, however thefe circuits are diverfified, that *A*, who touches the gun-barrel, and 1, who holds the phial, are certain to feel the ftroke. § 37. Fig. 18.

This experiment may be reverfed, the lines being as before, in the following manner, wherein likewife this circuit is always obfervable. Let *A* touch § 38.

touch the gun-barrel as before, and *D* hold the wire of the electrified phial in his finger. Let 4 grasp the phial, and 1 touch *B*; then *A* feels nothing, being left out of the circuit, and the other seven are struck. If 4 touches *C*, then *A* and *B* feel nothing, the circuit consisting of the remaining six. But it is to be observed, as in the former experiment, that 4, who grasps the phial, and *D*, who holds the wire, must of necessity be always in the circuit. I have been the more particular in this matter, as it demonstrates the course of the electrical power to be in the most direct manner between the gun-barrel and the electrified phial.

§ 39. Likewise, if a person, standing upon an originally-electric, touches the gun-barrel with his right hand, a piece of wire being placed round his left leg, and a second person, standing likewise upon the wax, takes hold of the extremity of this wire; then let another person, standing upon the floor, and grasping the electrified phial, touch any part of the second person's body. Upon this touch, the second person is shook as usual; but the first feels the stroke only in his left leg and right arm, the nearest course of the electrical power.

§ 40. If any number of persons communicate by pieces of wire, and if any one of them brings together the ends of the two pieces of wire in his hands, upon the gun-barrel's being touch'd, he will perceive no stroke. But if the ends of the wires are but a $\frac{1}{4}$ of an inch asunder, he will be shaken in both his arms; because then his body will become part of the circuit.

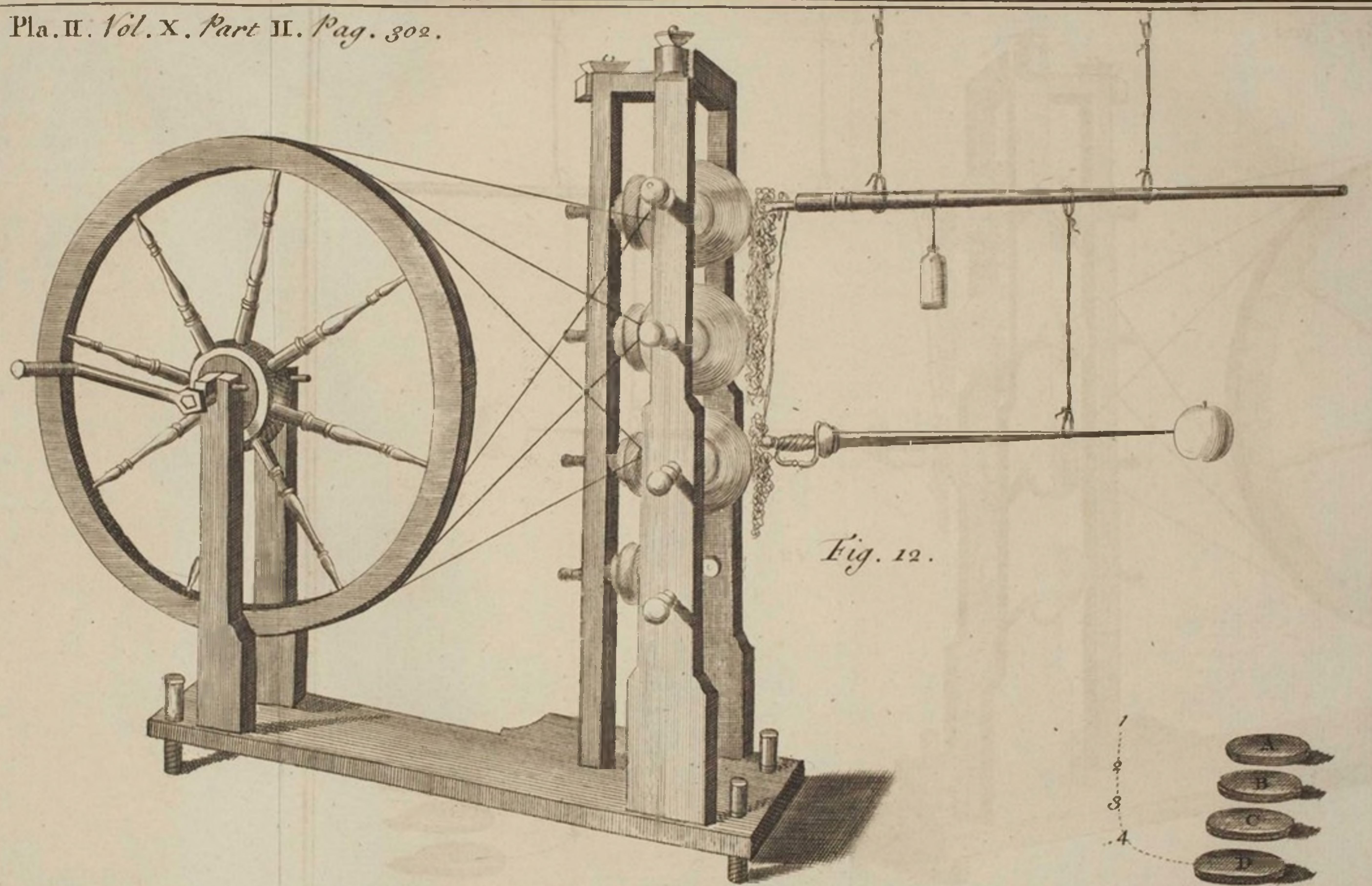
§ 41. If, when any number of persons join hands, or communicate by any metallic medium standing on the floor, one grasps the phial, and joins with the rest; upon the gun-barrel's being touch'd by the last person of the line, the whole number are struck, and he who grasps the phial, as forcibly as the rest. But if two phials are employed, and he grasps them both, with a piece of wire of sufficient length held between his fingers, which wire touches both phials, and it's end is taken hold of by the second person of the line; if then the last person touches the excited gun-barrel, all in the line are violently struck, except the person who grasps the phials; but he feels little or nothing of the stroke.

§ 42. The stroke is very violent, when a wire is put round the naked head, or under the peruke, and the person grasping the phial touches the gun-barrel with the ends of the wire, or if he holds the wire between his teeth.

§ 43. If a person, standing on the electrical cakes with gold or silver lace upon his coat, takes hold of the gun-barrel, and another person grasping the electrified phial touches the bottom of the lace, the person electrified, if he holds down his head, feels the blow under his chin. The lace in this instance has the same effects as a piece of metal; at the end of which, if placed in the same manner, you would necessarily feel the stroke.

44. I now proceed to shew, by what steps, in my inquiries into the nature of Electricity, I discover'd that the glass tubes and globes had not the electrical

Pla. II. Vol. X. Part II. Pag. 302.



electrical power in themselves, but only served as the first movers and determiners of that power.

Several months since, I observ'd that, by rubbing a glass tube, while standing upon a cake of wax, in order, as I expected, to prevent any of the electrical power from discharging itself through me into the floor; contrary to my expectation, that power was so much lessen'd, that no snapping was to be observ'd upon another's touching any part of my body. But if a person not electrified held his hand near the tube whilst it was rubbing, the snapping was very sensible. This I shew'd to several Members of the *Royal Society*, and others, who did me the honour to visit me. Afterwards I met with an experiment of the same kind, in a treatise publish'd by Professor *Bose*, intitled, *Recherches sur la cause et sur la veritable theorie de l'Electricité*, which that ingenious gentleman says, had given him great trouble by it's oddness. The experiment is, that, if the electrical machine is placed upon originally-electrics, the man who rubs the globes with his hands, even under these apparently favourable circumstances, gives no sign of being electrified, when touched by an unexcited non-electric. But if another person, standing upon the floor, does but touch the globe in motion with the end of one of his fingers, or any other non-electric, the person rubbing is instantly electrified, and that very strongly. The solution of this *phænomenon*, seemingly contrary to the already discover'd laws of Electricity, had terribly tormented him; but however he has given us the following, which he modestly calls a plausible subterfuge rather than a solution; *viz.* that a power cannot act at the same time with all it's vigour, when one part of it is already employed; as a horse, who already draws an hundred pounds, cannot draw an additional weight as freely as if he had not been loaded at all. That the hand excites the virtue already in the sphere; therefore if the same power impregnates the man, there remains none for the globe. That the virtue of the globe then cannot be communicated at the same time to the man, by whom it is created. That he, who gives it, cannot receive it himself. From these, and such-like considerations, it appears to him, that the man upon the ground, who holds his fingers to the globe in motion, instead of his diminishing it's electrical force, throws that force back again over the man, who excited it. That the finger in this case seems to operate as an electric *per se*, and drives back the electrical power. § 45.

I have seen an account of * Mr *Allamand*, lately printed at the *Hague*; wherein he takes notice of this *phænomenon*. He tells us, that as part of the electrical power of the globe passes off by the frame, upon which the globes are mounted, into the floor, and dissipated thereby; he conceived, that if the machine, and the man who rubb'd the globe, were placed upon pitch, to prevent this dissipation, the fire of Electricity would be more strong. But the consequence is extremely odd and unexpected; for the contrary happens; and the electrical power is considerably diminished, and sometimes there is even none at all. § 46.

* *Bibliotheque Britannique pour les mois de Janvier, Fevrier, et Mars*, 1747.

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§ 47. I tried this experiment ſeveral times with my machine, and the man, who turns the wheel thereof, mounted upon the electrical cakes. If the air was dry, and the machine placed at ſome diſtance from non-electrical ſubſtances, as the ſides of the room, chairs, and ſuch-like; after one or two ſmall ſnaps, the gun-barrel, ſupported by ſilk lines, and hanging in contact with the globes, would, tho' the machine were in motion a conſiderable time, attract no light ſubſtances, nor emit any fire. This induced me to conceive, that the electrical power was not inherent in the glaſs, but came from the floor of the room; and if the fact were ſo, the gun-barrel ſhould ſnap upon my touching any part of the machine. The conſequence fully anſwer'd my conjectures; for while I ſtood upon the floor, the globes ſtill in motion, I put one hand upon the frame of the machine, and touched the gun-barrel with one of the fingers of my other. Upon this, fire iſſued, and the ſnapping continued as long as I held my hand upon the machine, but ceaſed upon taking it off. This at once proved to me, that the electrical fire paſſed from the floor thro' my body to the machine. I then order'd the man to put one of his feet from the wax upon the floor; which, as ſoon as he complied with, cauſed the Electricity to ſnap at the gun-barrel, and this ceaſed upon his replacing his foot. Here I found, that the electrical power came through the man; and that, in theſe inſtances, either myſelf, or the man who touched the floor with his foot, was to be regarded as an additional part of the machine communicating with the floor. Theſe conſiderations led me to make the following experiments.

§ 48. If my conjectures were well founded, and if the electrical power, the man and the machine being placed upon originally-electrics, went through my body to the machine, a fine wire, held in my hand at a few inches diſtance, ought to be attracted by any part of the machine. This ſucceeded accordingly, but the attraction laſted a very ſmall ſpace of time, and the wire again hung perpendicularly from my finger, though the globes continued in motion. This induced me to believe, that the gun-barrel, and the other non-electrics ſuſpended in contact with the globes, would only contain a certain quantity of the electrical æther; and if this were the caſe, the attraction of the wire to the machine would be continual, if the electrical power found again a communication with the floor, as the wire was the only canal of communication between the floor and the machine. Whereupon I placed one of my fingers upon the gun-barrel, and held a wire near the machine with my other hand, and found, that as long as my finger continued upon the gun-barrel, the wire was attracted, but no longer.

§ 49. Here we find, that one cauſe of the electrical attraction is the current of the electrical æther ſetting to the machine through the wire; and this current is ſtopped from two cauſes; one, when there is no diſcharge thereof from the gun-barrel, the accumulation being complete; the other, when other currents are opened, that is, when the machine is touched in other parts.

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In theſe, and the ſubſequent experiments, I always ſuppoſe the air very dry; for if it is not, and the ſilk lines, which ſupport the non-electrics, are wetted thereby, the electrical power will be diſcharged along them, and the wire will be conſtantly attracted, as I have frequently on purpoſe experienced; and this diſcharge is in proportion as the lines are more or leſs wetted. § 50.

If a man ſtands upon the machine placed upon originally-electrics, and the gun-barrel with the other non-electrics are ſuſpended as uſual in contact with the globes, no Electricity is obſerved in that man: but if a wire, hanging to the wainſcot of the room, touches the gun-barrel, or a man ſtanding upon the floor applies his finger thereto, the man upon the machine emits fire copiouſly; and either himſelf, or the man who turns the wheel of the machine, fires inflammable ſubſtances. But this effect is no longer obſervable, when the wire, *&c.* are removed from touching the gun-barrel. So that, in this experiment, the uſual courſe of the Electricity is inverted; and that power, which, in moſt other inſtances, is brought by the wood-work of the machine to the globes, and by them diſcharged upon the gun-barrel, is now brought by the wire to the gun-barrel, and from this the globes throw it all over, not only the machine, but any non electric in contact with it, if the Electricity is ſtopped. In this experiment, if an iron rod, ſtanding upon the floor, is inclined againſt the loops of the ſilk lines which ſupport the gun-barrel, in ſuch a manner as not to touch the gun-barrel, the electrical fire, which paſſes from the iron rod to the gun-barrel, inſtead of being ſupplied conſtantly, comes in by ſnapping ſo long as any unexcited non-electric communicates with the machine, but ceaſes upon it's being removed: and if the air is very dry, and none of the Electricity conducted down the ſilk lines, the ſnapping from the iron rod to the gun-barrel will frequently correſpond to the touching of the wooden machine with your fingers, and ſtop upon your taking them off. And this experiment will look much like magic, even to thoſe who are acquainted with the operations of Electricity; for if the perſon who turns the wheel of the machine, and ſtands upon the cakes, be properly inſtructed: upon your bidding the gun-barrel ſnap, he only puts the toe of his ſhoe upon the floor, and it ſnaps immediately, and continues ſnapping as long as he keeps it there; but if you order it to ceaſe ſnapping, he almoſt imperceptibly replaces his foot upon the cakes, and it ceaſes. This may be repeated as often and as long as you pleaſe. § 51.

Many experiments demonſtrate, that if the Electricity is not ſtopt, no ſign of it's preſence, either by fire or attraction, is obſervable in the non-electric bodies ſuſpended to the globes: that is, although ever ſo great a quantity be determined by the globes over theſe bodies, the Electricity paſſes off from them *pleno rivo* to the floor, from whence it came: but if the Electricity is ſtopt, it is then accumulated upon theſe non-electrics; but this can be done only to a certain degree, as is manifeſt from a former experiment. And if, when this power is accumulat- § 52.

ed, a man ſtanding upon the floor touches now-and-then the non-electrics with his finger, the Electricity, which is here accumulated, ſnaps, and the fire is always obſervable. But this ſnapping is not, when the electrical power paſſes off continually, as from a piece of blunt wire hung to the ſuſpended gun-barrel, and the hand of a man brought near it without touching; whereby the electrical power becomes viſible, like a fine blue cone of flame, with it's point towards the wire. When the hand is placed at a proper diſtance, the blaſt, like that of cold air, is therefrom very manifeſt. If you do not determine the Electricity by theſe means to a point, the diſſipation of it is general, and from all parts of the excited non-electric; but if you do, by bringing your hand near the wire as before-mentioned, you ſee the manner of it's being diſcharged into the floor, and ſo into the earth. Theſe facts being ſo, if my conceptions are true, that the glaſs globes circulate the electrical fire, which they receive from their friction againſt the cuſhions, or the hand of a man, and which is conſtantly ſupplied to theſe laſt from the floor; the ingreſs of the electrical fire, if the machine, &c. are placed upon electrics *per ſe*, ought to be viſible, as well as the egreſs under the ſame circumſtances; and this is demonſtrated by experiment. For if, while any unexcited non-electrics touch the gun-barrel, the globes being in motion, you bring your finger, or a piece of wire near any part of the wood-work of the machine, but more eſpecially the iron axis of the wheel; you obſerve the bruſh of blue flame ſet in from it to the wood-work. We always obſerve, in this experiment, that the lambent flame from the end of the wire paſſes diverging into the machine, and this continues ſo long as the gun-barrel is touch'd. So that here the office of the globes exactly tallies with that of the heart in animals; which, as long as the quantity of blood is ſupplied, propels it into the arteries, and theſe all over the ſyſtem; or that of the pump in hydroſtatics. In the ſame manner, by the attrition of glaſs tubes, the electrical power is brought from the body of the man who rubs the tube; and he is conſtantly taking in a ſupply from the floor.

§ 53. What I here call the electrical æther, is that atmoſphere which ſurrounds both excited originally-electrics, and excited non-electrics. That this is extended to a conſiderable diſtance, appears, from a fine thread, or piece of cotton-graſs ſeed, being attracted at ſome diſtance from them, as far as which, it is preſumed, this atmoſphere extends. Here indeed it is only perceived by it's effects upon theſe light ſubſtances: but at the bruſh of flame from the end of the wire before-mention'd, from ſome bran lying upon a flat piece of metal in contact with excited non-electrics, your hand being held over it, and in many other experiments, it becomes manifeſt to your feeling as a blaſt of cold wind. You feel it likewiſe in a leſs degree, when a glaſs tube is well excited, and brought near your face. If no unexcited non-electric is near, this atmoſphere ſeems to be determined equally over all the excited non-electrics in contact with the machine; but if a non-electric unexcited is brought near,

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the greateſt part of it is determin'd that way; and hereby the attraction at any other part of theſe excited non-electrics is conſiderably diminiſhed. Hence the cauſe of the repulſion of Electricity, which does not operate, until the electrical æther is ſufficiently accumulated. This electrical repulſion is ſtrongeſt in thoſe parts of the excited non-electrics, where unexcited non-electrics are brought near them; for by theſe the electrical blaſt, which otherewiſe is general, is particularly determined to the floor.

Before I proceed further, I muſt beg leave to explain what I call the accumulation of Electricity. To put a ſimilar caſe: as we take it for granted, that there is always a determinate quantity of atmoſphere ſurrounding the terraqueous globe, we conceive, when we ſee the mercury in the barometer very low, that there then is a leſs accumulated column of this atmoſphere impending over us, than when we ſee the mercury high. In like manner, when we obſerve that the electrified gun-barrel attracts or repels only very light ſubſtances at a very ſmall diſtance, or that the ſnap and fire therefrom are ſcarcely perceptible; we conceive then a much leſs quantity of electrical atmoſphere ſurrounding the gun-barrel. This power being more or leſs, we call the greater or leſs degree of the accumulation of Electricity. This is only attainable to a certain point, if you electrify ever ſo long; after which, unleſs, otherwiſe directed, the diſſipation thereof is general. The phial of water of *Muſſchenbroek* ſeems capable of a greater degree of accumulation of Electricity, than any thing we are at preſent acquainted with: and we ſee, when, by holding the wire thereof to the globe in motion, the accumulation being complete, that the ſurcharge runs off from the point of the wire, as a bruſh of blue flame. A method has been diſcover'd here by Mr *Canton*, by which the quantity of accumulated Electricity may be meaſured to great exactneſs. The manner of meaſuring is this: when the phial is ſufficiently electrified by applying the wire thereof to the glaſs globe, and which is known by the appearance of the bruſh of flame at the end of the wire, as before-mention'd; hang a ſlender piece of wire to the ſuſpended gun-barrel for this purpoſe detached from the globes. Upon your applying the wire of the electrified phial to that hanging to the gun-barrel, you perceive a ſmall ſnap; this you diſcharge by touching the gun-barrel with your finger, which likewiſe ſnaps: and thus alternately electrifying and diſcharging, you proceed until the whole Electricity of the water is diſſipated; which ſometimes is not done, under 100 diſcharges. If you do not diſcharge the Electricity every time, the ſnaps from the wire of the electrified phial to the gun-barrel are ſcarcely perceptible. In proportion to the number of ſtrokes, you eſtimate the quantity of the acquired Electricity of the water. That you could, by ſtopping the Electricity, excite non-electrics; and, by accumulating their power, make them exert more force than originally-electrics would at any point of time, was that capital diſcovery of the late Mr *Gray*; and is to be regarded as the baſis, upon which all the preſent improvements of our knowledge in Electricity are founded; and till which discovery, although some of the effects of Electricity were observed above two thousand years ago *, little progress was made.

§ 55. The electrical æther is much more subtil than common air, and passes to a certain depth through all known bodies. It passes most readily through metals, water, and all fluids, except resinous ones; then animal bodies dead or alive, in proportion as they are more or less wet; then stones, wood, and earths. It passes to a certain thickness only thro' resins, dry animal substances, wax, and glass. For this reason bodies are called electrics *per se*, or non-electrics; not only for their rubbing the Electricity from other bodies, but likewise as they permit more or less of the electrical æther to pass through them. This æther has not only the property with air of moving light substances; but it seems to have another, and that is elasticity.

§ 56. That this fluid is more subtil than common air, is more particularly demonstrated by it's passing through several glasses at the same time; through any one of which, though ever so thin, air cannot pass. It likewise passes, as I have mention'd before, through all known bodies, except originally-electrics, and even through these to a certain degree. It's elasticity is proved by it's extending itself round excited electrics, and excited non-electrics, to a considerable distance; as well as by it's increasing the motion of fluids. This is demonstrated by the experiment with a small glass siphon, where the elasticity of the electrical æther overcomes the attraction of cohesion: I have frequently observed this experiment does not operate, unless the greatest part, if not the whole electrical blast, is determined to the floor through the water, by bringing some unexcited non-electric near the long leg of the siphon †. The stream through this slender tube is most complete, when the non-electric is brought near, so as when the room is somewhat darkened, the stream of water appears as a stream of blue flame, much like that from the blunt wire. This stream is stopped, either by touching any part of the non-electrics in contact with the globes; by placing the machine and the man who turns the wheel upon electrics *per se*, by which the current of the electrical æther from the floor to the machine is prevented; or by removing the non-electric from the leg of the siphon, by which the dissipation of the electrical æther from the excited non-electric becomes

* *Theophrastus*, who lived above 300 years before the date of the Christian Æra, takes notice of amber and the *Lyncurium*, attracting not only straws, and shavings of wood, but also thin pieces of copper and iron. See *Theophrastus* περὶ τῶν λίθων· ν. —Καὶ τὸ λυγκούριον—ἕλκει γὰρ ὥσπερ τὸ ἤλεκτρον. Οἱ δέ φασιν οὐ μόνον κάρφη καὶ ξύλον, ἀλλὰ χαλκὸν καὶ σίδηρον, ἐὰν ᾖ λεπτός· ὥσπερ καὶ Διοκλῆς ἔλεγεν. See *p.* 74. in the late Edit. by *J. Hill*.

† There is one instance, where the water will run off in a full stream without bringing a non-electric unexcited near the long leg of the siphon; and that is, by suspending a phial of water, as usual to the gun-barrel by a wire, and by letting a glass siphon through the cork into the water. When this phial is sufficiently electrified, the water therein runs off in a full stream, though no non-electric unexcited is near; because then the current of water through the siphon is the only way, by which the surcharge of the Electricity can be dissipated.

general.

general. So that we find, that although we can repel light bodies from many parts of excited non-electrics at the ſame time; the whole force of the electrical current is neceſſary, to drive off ſo ponderous a fluid as water. May we likewiſe not infer the elaſticity of electrical æther, from the ingreſs of the blue flame from the end of a blunt wire held near the axis of the wheel, or any part of the wood-work of the machine, after the revolutions of the globes are ceaſed? Certainly we ſee an influx of electrical fire to all bodies, until their determined quantity is reſtored. Is not the elaſticity of this æther deducible likewiſe from the violent ſhock we feel in our bodies in the experiments with water?

There ſeems to be a quantity of this æther in all bodies. Hence the § 57. reaſon why, though the machine is placed upon electrics *per ſe*, a ſnap or two, as I mention'd before, is obſerv'd upon touching the gun-barrel, when the machine has been ſome time in motion: but after theſe no more is perceiv'd, if the ſilk lines are very dry, and the electrical ſupporters of the machine are of a requiſite thickneſs. As ſoon as any non-eletcric unexcited touches the machine, this loſs is immediately reſtored. As the electrical æther, as has been ſpecified, is an elaſtic fluid, wherever there is an accumulation thereof, there is an endeavour by the neareſt unexcited non-electric to reſtore the *equilibrium*. The reſtoring of this *equilibrium* I take to be the cauſe of the attraction of excited glaſs tubes and globes, as well as that of excited non-electrics; for here the blaſt of electrical æther conſtantly ſets in from the neareſt unexcited non-electrics towards thoſe excited, and carries with it whatever light bodies lie in it's courſe. This ſetting in of the current of electrical æther towards excited non-electrics is likewiſe very perceptible to your feeling as a blaſt of cold wind; if when you are electrified, you hold your hand over a plate with ſome bran in it, by which blaſt the bran is carried againſt your hand. Theſe light ſubſtances are again repell'd by the blaſt from the excited bodies, as ſoon as they come in contact, and ſometimes before. The ſucceſſions of theſe alternate attractions and repulſions are extremely quick, ſo that ſometimes your eye can hardly keep pace with them. And if you put a glaſs globe of about an inch in diameter very light and finely blown into a plate of metal, and hang another plate over it; electrify the upper one, and bring the other under it, and you will find the ſtrokes from the alternate attractions and repulſions * almoſt too quick for your ear. I have ſeen a *German*, who travelled with a ſmall electrifying machine, who, by a proceſs of this ſort, made two ſmall bells ring. One of the bells was ſuſpended to an electrified wire, which was conducted without touching along the ſides of the room; at about an inch diſtance, detached from this wire, a little clapper was hung by a ſilk line; at an equal diſtance from this laſt was

* The following is an argument of the velocity likewiſe, with which theſe little globes are attracted and repelled. If they are let fall from the height of ſix feet or more upon a wooden floor, or a plate of metal, they are rarely broke; but by the attractions and repulſions of them between the plates, though at the diſtance only of $\frac{1}{6}$ of an inch, they are frequently beat in pieces.

hung

hung another little bell, which communicated with the ſides of the room. As ſoon as the machine was in motion, the electrified bell attracted the clapper, which immediately by the repulſive blaſt was blown off to the unexcited bell. By the time the ſecond bell was ſtruck, the former attracted again; and this jingling of the two bells continued not only during the motion of the machine, but ſeveral ſeconds after it was ſtopped. This was occaſioned by the ſmall volume of the clapper being able to convey away only a ſmall quantity of the electrical æther at each ſtroke; by which it was ſome time before the *equilibrium* was reſtored.

§ 58. To demonſtrate likewiſe, that the reſtoring this *equilibrium* is not imaginary, I ſhall mention an experiment of Mr *Wilſon*, who has taken great pains in theſe inquiries. Take two plates of any metal, very clean and dry, whoſe ſurfaces are nearly equal; hang one of them to any excited non-electric, and bring under it upon the other a whole leaf of ſilver. When, which you find upon application, the ſilver leaf is attracted, lower the bottom plate; if it is too low, you will obſerve the leaf ſilver jump up and down; if too high, it will only be attracted in part, and thereby diſſipate the electrical power. But if you get it at the proper diſtance, which will very eaſily be found upon trial, the ſilver will be perfectly ſuſpended at right angles with their planes, like the *trapezium* of the Geometers, and touch neither of the plates; it will be extended likewiſe to it's utmoſt dimenſions. You frequently obſerve, both at the top and bottom of the ſilver, the electrical fire. The ſame effect is produced, if you reverſe the experiment, by electrifying the bottom plate, and ſuſpending the other over it. Now I conceive, that the ſpace occupied by this leaf of ſilver, is that where the *equilibrium* of the electrical æther is reſtored; for if you take away the under plate, thro' which from the floor the flux of this æther is furniſhed, or if that plate be placed upon an electric *per ſe*, by which this flux is prevented likewiſe, the ſilver leaf is blown away.

§ 59. No body can be ſuſpended in *equilibrio* but from the joint action of two different directions of power: ſo here, the blaſt of electrical æther from the excited plate blows the ſilver towards the plate unexcited. This laſt, in it's turn, by the blaſt of electrical æther from the floor ſetting through it, drives the ſilver towards the plate electrified. We find from hence likewiſe, that the draught of electrical æther from the floor, is always in proportion to the quantity thrown by the globes over the gun-barrel; or the *equilibrium* by which the ſilver is ſuſpended, could not be maintained. I once found, that a gentleman, at that time an invalid, whoſe ſhoes were perfectly dry, and of conſequence originally-electrics, and who was employed to hold the non-electric plate through which the æther was to come from the floor; this gentleman, I ſay, did not furniſh a ſufficient quantity, becauſe of the dryneſs of his ſhoes, to maintain the *equilibrium*; and the ſilver was blown away. But upon employing another to this office, whoſe ſhoes were more wet, the æther came readily through him, and the ſilver was ſuſpended. I have likewiſe

likewiſe found a wooden pole, very dry, not conduct this æther faſt enough to keep the ſilver ſuſpended. It may be imagined, that it is poſſible for the ſilver to be ſuſpended, without ſuppoſing a flux of the electrical æther from the neareſt unexcited non-electric, as well as from the excited one; that is, by the ſimple electrical attraction. But to obviate this, it muſt be remembered, that the electrified gun-barrel both attracts and repels light ſubſtances at the ſame time. Can this attraction and repulſion be conceived without the operation of the electrical æther both to and from the gun-barrel at the ſame time? Does not this point out an afflux as well as an efflux? Are not the electrical repulſions as ſtrong at leaſt as the attractions? Do not we ſee light bodies, either between excited originally-electrics, or excited non-electrics, and unexcited non-electrics, dart like a ball between two rackets of equal force? It may be ſaid perhaps,

1. That the ſuſpended ſilver may only ſerve as a canal of communication, which diſcharges the Electricity from the excited non-electric to the un-excited one; and that when an originally-electric is placed between the lower plate in this experiment and the floor of the room, that then the ſilver is attracted only, until the lower plate is ſaturated with Electricity, and no longer. This is as much as ſaying that this effect ariſes from Electricity, without mentioning in what manner.

2. That this effect is produced by the electrical attraction, which gives the ſilver a direction towards the excited non-electric, but that it is kept down near the unexcited one by the force of gravity. Was this the cauſe, the action of gravity would operate as much thro' originally-electrics as through non-electrics.

But I am able to prove the afflux experimentally, as well as the efflux, § 60.
in the following manner. When the ſilver lies ſtill, though the motion of the globes is continued, between the two plates, one ſuſpended to the gun-barrel, and the other placed upon an electrical cake, a perſon ſtanding upon the floor needs only bring a ſmall glaſs ſiphon in a veſſel of water, and apply the long leg thereof near the plate placed upon the wax; for upon this the ſilver is immediately ſuſpended; and the water, which before only dropped, now runs in a full ſtream, and appears luminous *. Does not, in this caſe, the current of the water point out the direction of the current of electrical æther?

When the machine, &c. are placed upon originally-electrics, if a § 61.
man, ſtanding likewiſe upon an originally-electric, touches the gun-barrel while the globes are in motion, he will receive a ſnap or two; after which, though the motion of the globe is continued, he will perceive

* This experiment is more elegant, if the upper plate, attracting the ſilver, is ſuſpended high enough for a perſon ſtanding upon an originally-electric, conveniently to bring the other plate under it with one hand, and to hold a pewter plate in the other. If the originally electric is ſufficiently thick, the ſilver will not be ſuſpended; but if the glaſs ſiphon in a ſmall veſſel of water is brought very near the pewter plate, the water runs into the plate, and the ſilver is immediately ſuſpended.

no more fire from the gun-barrel. While in this posture, if he touches the wood-work of the machine with one hand, and applies a finger of his other near the gun-barrel, at that instant he receives the electrical strokes. These continue as long as he touches the machine, but cease upon his removing his hand therefrom. Here we see a circulation of part of this man's electrical fire, which operates in the following manner. First; The man, by applying one of his hands to the machine, becomes a part thereof; and, by the motion of the globes, part of the electrical fire, inherent in his body, is driven upon the gun-barrel; but it is instantaneously restored to him again, upon his touching the gun-barrel with his other hand. Thus he continues communicating the fire with one hand, and having it restored to him with the other, as long as he pleases. If, instead of touching the machine or gun-barrel, he holds his finger near either or both of them, you see the fire go out, and return back, as in a former experiment.

§ 62. It may be perhaps imagined, if one man touches the machine, himself and the machine both being placed upon the wax, and if another, standing upon the floor, constantly, or by turns, touches the gun-barrel, that by these means the man upon the originally-electrics might be divested of all his electrical fire, by constantly continuing the motion of the globes, as he receives then no supply from the floor. But the contrary proves true; and, after a considerable time, the strokes from the gun-barrel are as strong as at first. But here we must observe, that the gun-barrel suspended will not contain probably at one time $\frac{1}{1000}$ part of the whole quantity of this man's electrical fire: therefore I conceive, that, as soon as this man has parted with any portion of his necessary, his determined quantity, to the gun-barrel by the motion of the globes, he has it restored to him upon any unexcited non-electric's touching the gun-barrel, by having the usual course of the Electricity * inverted.

§ 63. We see, from many experiments, that dry wood does not conduct Electricity so well as that which is wet; and that the man standing upon the floor, who rubs the globes, excites the Electricity stronger than the cushions. This I had reason to conceive was owing not to any other difference, than that of his being more moist, and, of consequence, more readily conducting the Electricity from the floor. Therefore I order'd my machine, and even the cushions to be made damp, by causing wet cloths to be placed upon several parts thereof; and found then, that the Electricity was equally strong, as when the globe was rubbed by the hand.

§ 64. It remains now, that I endeavour to lay before you a solution why our bodies are so shocked in the experiments with the electrified water; the difficulty thereof I confess seemed unsurmountable, until I had made the following discoveries.

1. That the Electricity always described a circuit between the electrified water and the gun-barrel.

* For a further account of this matter, see Art. 28. § 7.

2. That

2. That the electrical fire came from the floor of the room.

3. That it would not paſs from the floor quick enough for the perſon to be ſhaken, if his ſhoes were dry.

4. That the force was increaſed in proportion to the points of contact of non-electrics with the glaſs containing the water.

Then the ſolution of this phænomenon became more eaſy, which I take the liberty to offer.

1. I have endeavoured to prove by experiment that a quantity of Electricity is furniſh'd from the neareſt unexcited non-electrics, equal to that accumulated in excited originally-electrics and excited non-electrics.

2. This being ſo, when the phial of water held in one hand of a man is highly electrified, and he touches the gun-barrel with a finger of his other; upon the exploſion which ariſes herefrom, this man inſtantaneouſly parts with as much of the fire from his body, as was accumulated in the water and gun-barrel; and he feels the effects in both arms, from the fire of his body ruſhing through one arm to the gun-barrel, and from the other to the phial. For the ſame reaſons, if, in the experiment with the electrical * mine, a man places his right foot upon the lower ſmall wire, and touches the gun-barrel with his left arm, the electrical force is only felt in that leg and arm.

3. As much fire as this man then parted with, is inſtantaneouſly replaced from the floor of the room, and that with a violence equal to the manner in which he loſt it.

4. But this flux of electrical æther, either from the floor to the man, or from the man to the water, is prevented for reaſons ſufficiently obvious, if the glaſs containing the water be thick; if the points of non-electric contact are few; if the man is placed upon originally-electrics; or (which is the ſame thing) if the ſoles of his ſhoes are dry.

5. As we find that the Electricity paſſes at leaſt equally quick through denſe mediums, which are non-electrics, as through thoſe which are more lax and ſpongy; may we not therefore conclude, that the cauſe why we feel moſt pain at the joints of our arms, and in the tendons of our heels †, ariſes from the texture in the tendons and tendinous ligaments of thoſe parts?

From a due conſideration of the *phænomena* before us, I take the liberty of propoſing the following queries: § 65.

1. Whether or no the effects we obſerve, in bodies being drawn to and driven from either excited originally-electrics, or excited non-electrics, are to be attributed to the flux of electrical æther?

* See more of this in Art. 28.

† This pain in the heels is felt only in the experiment with the electrical mine; and it is not perceptible only when you touch the lower ſmall wire with your feet, but likewiſe if you ſtand upon non electrics, which touch this wire. It has been ſtrongly felt by a perſon ſtanding upon a pedeſtal of *Portland* ſtone near ten inches in height, and upon one of metal more than two feet. I am of opinion, that no maſs of metal, of dimenſions however great, would in the leaſt prevent the progreſs of the electrical power from the water in the phials to the body of the man.

2. Whether or no, that, which from it's being firſt diſcover'd in amber, we call Electricity, electrical æther, electrical power, *&c.* is any other than elementary fire?

3. Whether or no this fire does not appear in different forms, according to it's different modifications? Does it not, when diffuſed under a large ſurface, appear to affect us as air? When brought towards a point, does it not become viſible, as lambent flame? When nearer ſtill, does it not explode, and become the object alſo of our feeling as well as of our hearing? Altho' it does not affect our ſkin with the ſenſation of heat; does it not, by it's lighting up inflammable ſubſtances, ſhew itſelf to be truly fire?

4. Whether or no this fire is not connected intimately with all bodies at all times, though leaſt of all, probably, with pure dry air? Have we not found and ſeparated it from water, flame, even that intenſe one of oil of turpentine, ſmoke, red-hot iron, and from a mixture 30 degrees colder than the freezing point?

5. Have we not proved it's ſubtility, from it's paſſing through all known bodies?

6. May we not infer it's elaſticity likewiſe from it's exploſions, from it's increaſing the motion of fluids, as well as from it's effect in the concuſſion of our bodies, when we diſcharge it after we have accumulated it in water?

7. May not the electrical machine, from it's uſes, be denominated a fire-pump, with equal propriety as the inſtrument of *Otto Guerick* and Mr *Boyle*, that of the air?

8. Does not the power we are now maſters of, of ſeeing the ſeparation of fire from bodies by motion *, and of ſeeing it reſtored to them again, and even after that motion has ceaſed, cauſe us rather to incline to the opinions of *Homberg (a)*, *Lemery* the younger *(b)*,

* The ſetting in of the fire to the glaſs tubes and globes has always, in theſe experiments, been viſible both from the hands and cuſhions, by which they were rubbed. But as, till now, this fire was conſidered as coming from the glaſs, that, obſerved upon the hands and cuſhions, was always believed to be ſo much loſt by running down the inſtruments of friction into the floor. I endeavoured to prevent this loſs, by ſtanding upon originally-electrics; and found, to my great ſurprize, that ſo far from increaſing the electrical power, by ſtopping what I conjectured was ſo much loſs, I could excite then no Electricity at all in the tube and globes. This diſappointment, which, I afterwards found, had occurred to Meſſ. *Boſe* and *Allamand*, was the foundation of my diſcovering the ſource of the Electricity, and the manner of it's ingreſs to the machine.

(a) Homberg du ſouphre principe. *Mem. de l'Acad. Royale des Sciences*, 1705. La matiére de la lumiére eſt la plus petite de toutes matiéres ſenſibles—elle paſſe librement au travers et par les pores de tous les corps, que nous connoiſſons —Que tout l'univers eſt rempli de la matiére de la lumiére—J'ai mieux donnè à notre ſouphre principe le nom de matiére de la lumiére. que celle du feu, quoique ce ſoit proprement la même choſe

(b) Lemery le fils. *Mem. de l'Acad.* 1709. p. 527. La matiére de feu doit être regardée, comme un fluide d'une certaine nature, et qui a des proprietez particulieres, qui le diſtinguent de tout autre fluide. Pag. 8.—Qu'une matiére beaucoup plus ſubtile et plus agitee, qui remplit tous les vuides de l'univers, et ne trouve point les pores ſi étroits, qui ne lui laiſſent un libre paſſage, coule inceſſamment dans les lieux où elle eſt enfermée, et entretient ſon mouvement.

s'Graveſand

s'Gravesand (c), and *Boerhaave (d)*, who held fire to be an original, a diſtinct principle, formed by the Creator himſelf, than to thoſe of our illuſtrious countrymen, *Bacon (e)*, *Boyle (f)*, and *Newton (g)*, who conceived it to be mechanically producible from other bodies?

9. Muſt we not be very cautious, how we connect the elementary fire, which we ſee iſſue from a man, with the vital flame and *calidum innatum* of the Ancients; when we find, that as much of this fire is producible from a dead animal as a living one, if both are equally replete with fluids?

10. Whether or no it is not highly probable, that by increaſing the number and ſize of the phials of water in a certain manner, you might not inſtantly kill even large animals by the electrical ſtrokes *(h)*.

I cannot conclude theſe papers, without congratulating that excellent § 66. Philoſopher and learned Member of this Society the Abbé *Nollet* of *Paris*. This gentleman, almoſt two years ſince, in a letter to Profeſſor *Boſe* (an extract of which this laſt publiſhed with a work *(i)* of his own) without the knowledge of ſeveral experiments ſince diſcover'd; at leaſt none of his diſcoveries have yet fallen into my hands, did declare his opinion, *(k)* that the Electricity did not only proceed from the electrified bodies, but from all others about them to a certain diſtance; *(l)* that the Electricity, as well from bodies electrified, as from

(c) s'Gravesand Philoſoph. Newton Inſtitutiones, cap. 1. Ignis in corpora omnia quantumvis denſa & dura penetrat ——Corporibus ſeſe jungit——ignem ad certam diſtantiam a corporibus attrahi—nulla novimus, quæ ignem non continent—non ignis æque facile corpora omnia intrat——corporibus contentus in his a corporibus circumambientibus retinetur.—Motu celerrimo ignem affici poſſe.

(d) Boerhaavii Elementa Chem. de igne, p. 187. & ſeq.—Ipſe ignis—ſemper præſens exiſtit in omni loco—imo vero in omni corpore, etiam rariſſimo, vel ſolidiſſimo, æqualiter diſtributus hæret.—Haud ergo potui detegere, quod in rerum natura ſit vel ullum ſpatium ſine igne.

Ibid. p. 283. Huc uſque conabar—tradere ea, quæ veriſſima addiſcere potui de natura illius ignis, quem elementalem appellant philoſophi. Illum ſcilicet, ita conſiderando, prout creatus ipſe in rerum (natura) exiſtet ſeorſum, extra reliqua omnia creata, quæcunque demum ſint, corpora.

(e) Vide tractatum *De forma calidi*.

(f) Mechanical Origin of Heat and Cold, ſect. 2.

(g) See Queries at the end of his *Optics*.

(h) Monſ. *le Monnier* at *Paris* killed birds by theſe; and with me, a linnet and a rat, much more than half-grown (the largeſt I was then able to procure) have been ſtruck dead.

(i) Recherches ſur la Cauſe, et ſur la veritable Theorie de l'Electricité. Wittembergue. 1745.

(k) Voyez Nollet dans les Recherches, &c du M. Boſe, pag. xlv.—La matiére electrique vient non ſeulement du corps électrifé, mais auſſi de tous ceux qui ſont autour de lui, juſques à une certaine diſtance.

Ibid. p. xlix.—Si vous pouvez vous convaincre comme moi, que la matiére qui va au corps électrique vient primitivement de tous le corps environnans, de l'air même, vous aurez bien plus de facilité à expliquer tous les autres effets.

(l) Ibid. p. xlvi. La matiére electrique, tant celle qui ſort du corps électriſé, que celle qui vient des environs à ce même corps, ſe meut plus facilement dans les corps denſe que dans l'air même.

those which were not, passed more readily through dense mediums than air; *(m)* that the Electricity is present in all bodies; *(n)* that this matter always tends to an *equilibrium*, and endeavours to occupy those spaces in bodies, which have not their necessary quantity: all which assertions may now be proved by experiments.

§ 67. You see, Gentlemen, by my asserting, that what we have hitherto called electrical *effluvia*, do not proceed from the glass, or other electrics *per se*, I differ from *Cabeus*, *Digby*, *Gassendus*, *Brown*, *Des Cartes*, and very great names of the last as well as the present age. My differing from them would be presumption indeed, were I not induced thereto, by observations drawn from a series of experiments carefully conducted, to which many of you have been witnesses, and to whom I may therefore appeal, for taking what may seem so extraordinary a step. I have constantly had in view that excellent maxim of Sir *I. Newton* laid down in his *Optics*, that, "as in Mathematics, so in Natural Philosophy, the "investigation of difficult things by the method of analysis ought ever "to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions "from them by induction, and admitting of no objections against the "conclusions, but such as are taken from experiments, or other certain "truths. For hypotheses are not to be regarded in Experimental Philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is "the best way of arguing which the nature of things admits of, and "may be look'd upon as so much the stronger, by how much the induction is more general.——By this way of analysis we may proceed "from compounds to ingredients, and from motions to the forces producing them; and, in general, from effects to their causes, and from "particular causes to more general ones, till the argument ends in the "most general." I am desirous, that what is contain'd in these papers, you will be pleased to regard rather as the rude outlines of a system, than as a system itself; which, I am in hopes, men of better heads and more leisure will prosecute: and if hereafter, from being possessed of more observations than we at present are masters of, any opinions in these papers shall be found erroneous, I at all times shall be willing readily to retract them.

Extracts of two letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning the effects of a*

10. Being determined on making some experiments in Electricity with other bodies besides glass, a little before the Holidays I procured a stick of the best black sealing-wax, of about an inch in thickness, and of a convenient length; and exciting it with white-brown paper, or clean dry flannel (I know not which is best) I made the following trials.

I attempted to kindle common lamp-spirits, both by attraction and repulsion, the electrified person standing on a cake of bees-wax, and suc-

(m) Ibid. p. xlvii.

(n) La même. Cette matiére tend à l'équilibre, et s'empresse de remplir les espaces, qui se trouvent vuides des parties de son espece.

ceeded.

ceeded.——I made trial, at the ſame time, with my glaſs tube, and, I think, kindled the ſpirits more eaſily. Perhaps, from ſome circumſtances hereafter to be mentioned, this may, *cæteris paribus*, be generally expected.

cane of black ſealing-wax, *and a cane of* brimſtone, *in electrical experiments.* N°. 478. p. 27. Jan *and* Feb. 1746. *Dated* Jan. 15. 1745-6. *Read* Jan. 23 1745-6.

I was then minded to repeat that experiment of the late ingenious and induſtrious Dr *Deſaguliers*, and others; by which it appears, that when any light body is put into a ſtate of repulſion by vitreous Electricity, it is in a ſtate of attraction in reſpect of reſinous Electricity, and ſo *è contra.* This I found conſtantly to hold good. —I made this trial with a down-feather, which was tied to the end of a penduious thread, which thread was tied to a ſilk line, faſtened horizontally to the oppoſite ſides of the room, and alſo with a ſmall piece of writing paper, of about the ſame dimenſions as the feather. Here I found the feather would retain the *effluvia* (whether of the tube or cane) about five or ſix minutes longer than the paper would; that is, the feather remained ſo much longer in a ſtate of repulſion The time in which the paper was in a ſtate of repulſion, after many trials, I found to be about 20′, more or leſs; at about which time the paper would indeed ſomewhat ſenſibly decline the tube, *&c.* but in a moment would be attracted by them; and if I ſtaid longer, I could not perceive any repulſive force remaining.

I ought to tell you, that when I had, by ſeveral trials, found out about what time the *effluvia* would be quite diſſipated, I forbore making any trials till then, leſt that, by bringing the tube or cane near the body of trial, I might communicate freſh *effluvia*, and perpetuate the ſtate of repulſion longer than it would otherwiſe have been; ſo that, in the laſt trials I made, I never came near with the tube, *&c.* till full 20′ after the body of trial was put into a ſtate of repulſion. I obſerved not any material difference of time between the diſſipation of the *effluvia* of the glaſs tube, and thoſe of the wax cane, when the ſame body of trial was made uſe of for both: if there was any difference, I think the vitreous *effluvia* were the moſt laſting.

I made another trial with the cane and tube in a dark room; being led to it from a ſuſpicion I had, that the *effluvia* from the wax cane were groſſer, and more in quantity, than thoſe from the glaſs tube; and, upon exciting both as quick as I could in ſucceſſion, I found the luminous *effluvia*, when I brought my forefinger near the wax, to proceed in a much greater quantity to the cane from the tip of my finger, than they did on the ſame trial with the tube of glaſs And I ſeveral times obſerved a ſmall globular ſpot of fire to appear firſt on my finger, from which iſſued regular ſtreams in form of a comet's tail.

When I made uſe of the glaſs tube, as the quantity was leſs, ſo the ſparks were finer, leſs in thickneſs and in length, but much more active; nor did they proceed ſo regularly towards the tube, nor make ſo regular an appearance (being ſeldom, if ever, altogether regular, as the others); frequently breaking in pieces, as if by colliſion, or not altogether unlike the ſparks from a brand in a wood fire, which has lain long without

being

being ſtirred. Another difference I remarked was, that the reſinous *effluvia* were more deeply coloured than the vitreous.

Q. Whether it be not probable, that the reſinous *effluvia* are more unctuous or ſulphureous than the vitreous; and becauſe not ſo active and nitrous, leſs apt to kindle inflammable ſpirits, as I think I found them to be?

I intreat I may not be conſidered as pretending, in the above trials, to eſtabliſh laws, but, as plainly relating matters of fact. Perhaps future trials may not confirm theſe.

I think it not a circumſtance too impertinent to be mentioned, that the trials relating to repulſion were made in a ſmall room, and near a fire; the air pretty moiſt.

Extract of the ſecond letter dated Jan. 22. 1745-6.

I am dubious whether I did not expreſs myſelf in a manner liable to be miſunderſtood, when I ſaid to this purpoſe, *that I would not be underſtood to eſtabliſh laws by the fore-mentioned experiments, but only to relate facts; and that future experiments might not confirm theſe.* I did not intend this ſhould extend to that experiment, which proves the different nature of vitreous and reſinous *effluvia*; which I preſume, may be conſidered as invariable inherent properties; ſo that bodies, put into a ſtate of repulſion by the one, will be attracted by the other, *&c.* But the other *phænomena*, as depending on changeable circumſtances, the temperature of the air, the degree in which the electric bodies may chance to be excited, the quantity of *effluvia*, and perhaps others to us unknown; the other *phænomena* (I ſay) depending on ſuch like circumſtances may be variable.

I beg leave to inform you, that I have been making trial with a ſtick of ſulphur of the common ſort, which I made of a convenient ſize, by caſting it into a coffin of paper, the inſide being of writing paper: this, being excited, attracted the bunch of threads with great power, and kindled common ſpirits as quick as ever I knew it done. This was after night, and I ſaw not what the day-light afterwards diſcovered, that the inſide round of paper adhered to the ſulphur, and it had made it's way thro' the paper, which concealed the colour of the paper, and it's adherence, till next day; however it performed as above.——This was broken, by an attempt to ſtrip the paper off the ſtick by a too officious perſon, without my knowledge. —— I then caſt another with the ſame ſulphur, and an addition of freſh, melted together in a wooden mould, which came out ſmooth and well; but was perſuaded, againſt my own judgment, to put a gun-rammer into the middle of the mould, to ſtrengthen it; which ſtick anſwer'd that end; but, as I fear'd it would turn out, the ſulphur tho' of a great thickneſs round the ſaid gun-ſtick, could by no means be excited to any tolerable degree. I therefore made a third, as the firſt, which has the paper on it as before, but it performs exceeding well: having ſuffered myſelf to be electrified with it, upon the approach of a perſon's finger to mine, I had by far the moſt painful ſenſation I ever yet

yet felt in any of theſe experiments.—I believe a glaſs tube might be beſt of all for a mould (but mine are of too ſmall a bore), if one could be aſſured it would not break.

11. I am under no doubt, but that experiments with ſulphur are capable of being improved, and hope ſhortly to make it appear. I am loth to venture my glaſs tubes of flint for a mould, but intend to procure one of common glaſs; having lately had the misfortune of loſing my beſt, in ſo odd a manner, that I believe you will excuſe me if I trouble you with the account.

Extracts of two letters from the ſame, containing ſeveral Electrical Experiments. *Ibid.* p. 53. *Dated* Feb. 4. 1745-6. *Read* Feb. 13. 1745-6.

I had been uſing it but a little time in the evening; and, before I laid it up, having by me a round ruler ſmall enough to go into the bore, when it was covered with a roll or two of brown paper, it came into my head to excite it, by rubbing it a little on the inſide with the ſaid ruler and paper; but not finding any effect of it, after a few minutes trial, not ſo much as to attract the ſmalleſt thread, I laid it in my window in my ſtudy on a parcel of papers and pamphlets, where it uſed to be put; and next morning, as we were at breakfaſt, I heard a ſnap, and, on turning my head, found about two inches of my tube broke off very regularly. Upon this I took it, and placed it againſt a cupboard-door, erect, in a pocket of leather, that had been nailed up againſt the door for ſuch a purpoſe. The upper end was tied to two thongs of leather, but not tight, only to prevent it's ſtirring: thus it continued ſafe till I went to bed; but, in the morning, upon opening the ſaid door, I was ſurpriſed to find my tube in ſhivers, except about three inches, as if it had been broke with a ſmart blow of a hammer. The cupboard is over the fire-place and ſo near it, that I think it impoſſible it ſhould ever have been quite cold; and the window where it was firſt put is ſo near the fire, and it's being laid on the ſeat of the window, a foot below the ſaſh, it could not be much affected with the air from thence.——The weather was froſty, but the tube from firſt to laſt never out of the room; and I am ſure never had any blow.

The ſtick of brimſtone I laſt made, with which I kindled lamp-ſpirits ſo readily, as I informed you before, was ſet up in the forementioned cupboard in an erect poſture, has loſt all it's electric virtue, and cannot be made to attract a down-feather, or a fine thread.——This I know not how to account for, unleſs it be, that the expoſing it to the air, by it's not being wrapped up in any thing, may have deprived it of it's power: for, if I miſremember not, *Stephen Gray* uſed to keep his ſulphur conic bodies, caſt in wine-glaſs, in a box, and wrapt up in flannel; however I ſhall attempt to recover it again.——The cupboard is ſmall, and never cold.—My ſtick of wax kept in my deſk, not wrapt, will attract a thread at any time, without rubbing at all.

Laſt night, having ſeveral gentlemen with me, who were deſirous of ſeeing me ſet fire to ſome ſpirits of wine, I was willing to try whether I could not kindle the ſame with an icicle; but, not being able to get one, I attempted it with a thick piece of ice, and immediately ſucceeded, in preſence

presence of 7 or 8 persons; and I think the sparks of fire from the ice, when the finger of a non-electric person was brought nigh it, were as large and as powerful as any I ever saw; so that I am satisfied the power of them is no ways diminished by the coldness of the ice: and I doubt not, but that, if the ice be kept from melting and dropping into the spirits, ice will kindle them as readily as any other substance: the spirits were such as we use for the tea-kettle lamp, and far from being of the best sort.

One circumstance more I will mention, and release you.—— By accident one of the gentlemen approaching the electrified person with his hand near his shoulder, the said gentleman felt a very pungent stroke on his flesh, thro' his coat and waistcoat, which were both cloth. This was repeated several times, and in every one's opinion (on whom trial was made) the repulsive stroke was as smart as it is wont to be on the end of the finger, when nothing intervenes; and the sensation continued as long. I know not whether this has been before taken notice of; if it has, your goodness will excuse my impertinence.

Extract of the second letter. Since my former, I made an other trial, and succeeded with all the ease imaginable; the spirits kindling the very moment of my approaching them with the lump of ice, which was an inch and ½ thick. After this I took a clamp of iron, such as is used for heating box-irons for smoothing linen-cloths; and having heated the same red-hot applied it to the spirit, as I stood on the cake of wax electrified, holding the same in a pair of tongs.

I did not, I confess, expect much from this trial; and the event was, that I could not kindle the spirits, during the time the redness continued in the clamp; but, as soon as that disappeared, and it began to look blackish, the spirits were kindled as usual.

I shall not draw any conclusion from a single trial; perhaps some reasons might be assigned, why the red-hot iron did not kindle the spirits, provided one were sure this would always be the case; and if the experiment were repeated with the same consequence a good many times, one would venture to say, that the heat of the iron contributed no power of inflaming to the *effluvia*.

My tube I have used of late is not made of the fine flint-glass, but such as common wine glasses are made of.

I have got me a tube made of common green glass: this is exceeding light, in comparison with others; and may be excited with double the time and pain required for the others, but yet not without warming it at the fire; though this seems powerful enough to attract the bunch of threads, yet I am not able to kindle any spirits with it.

I have made these trials, that I might be able to determine which kind of glass afforded the greatest quantity of *effluvia*, or at least the strongest, as near as might be; which may not be altogether unuseful to be known.

12. Mr

12. Mr *l'Allamand* incloſed ſome mercury in a tube cloſe-ſtopp'd; and, when he afterwards rubb'd this tube, it gave a great deal more light than when the ſame had no mercury in it.

Part of a letter from Mr Trembley, *F. R. S. to* M. Folkes, *Eſq*; P. R. S. *concerning the light cauſed by* quickſilver *ſhaken in a glaſs tube, proceeding from* Electricity. *Ibid* p. 58. *Dated* Hague, 4 Feb. 1745. N. S. *Read* Feb. 13. 1745-6.

When this tube has been rubb'd, after raiſing ſucceſſively it's extremities, that the mercury might flow from one end to the other, one ſees a light ſerpenting all along the tube; that is to ſay, the mercury, as it runs along, is all luminous.

Mr *l'Allamand* then made the mercury run in the ſame manner along the tube without rubbing it, and it ſtill gave ſome light, but much leſs than before. This laſt experiment perſuaded him, that the friction of the mercury againſt the glaſs might electrify that glaſs, in the like manner as the rubbing of the hand. And he has been confirmed in the ſame notion by another experiment: He brought ſome down near to the tube, and then made the mercury run from one end to the other; and the down was attracted, as the mercury in it's motion paſſed by it.

Theſe experiments he has repeated, and varied ſeveral ways; and they have led him to conclude, that the *phoſphorus* of the barometer, known this great while, is not ſo properly a *phoſphorus*, as the effect of the mercury electrifying the tube of the barometer.

Mr *l'Allamand* has put mercury into exhauſted tubes, and, when theſe are rubb'd, they give much more light than before; there then come out from them on all ſides rays of very lively light. I have alſo ſeen at *Leyden*, at Mr *Muſſchenbroeck*'s the mechaniſt's, an exhauſted globe of glaſs, which, when rubbed with the hand, ſeemed all filled with a very bright fire.

Several perſons have obſerved, that when they had been electrified, their pulſes beat a little faſter than before. I have even myſelf felt, after having been electrified a pretty while together, a ſenſation all over my body: but within theſe few weeks, ſome perſons have felt very ſharp pains upon their being electrified.

There is an experiment that Mr *l'Allamand* has tried; he electrified a tin tube, by means of a glaſs globe; he then took in his left hand a glaſs full of water, in which was dipped the end of a wire; the other end of this wire touched the electrified tin tube: He then touched, with a finger of his right hand, the electrified tube, and drew a ſpark from it, when at the ſame inſtant he felt a moſt violent ſhock all over his body. The pain has not been always equally ſharp, but he ſays, that the firſt time he loſt the uſe of his breath for ſome moments; and he then felt ſo intenſe a pain all along his right arm, that he at firſt apprehended ill conſequences from it; tho' it ſoon after went off without inconvenience.

It is to be remarked, that in this experiment he ſtood ſimply upon the floor, and not upon the cakes of reſin. It does not ſucceed with all glaſſes; and tho' he has tried ſeveral, he has had perfect ſucceſs with none but thoſe of *Bohemia*. He has tried *Engliſh* glaſſes without any effect. That glaſs with which it beſt ſucceeded was a beer-glaſs.

Mr *Muſſchenbroeck* the profeſſor has repeated his experiment, holding n his hand a hollow bowl exceeding thin, full of water; and he ſays he experienced a moſt terrible pain. He ſays, the glaſs muſt not be at all wet on the outſide.

Part of a letter from the Rev. Dr Miles, *F. R. S. to Mr* Hen. Baker, *F. R. S. concerning* electrical fire *Ibid.* p. 78. *Dated* Feb. 15. 1745-6. *Read* Feb. 20. 1745-6.

13. You query, whether that ſubtil fire which kindles warm'd ſpirit of wine, be reſident in the body from which it evidently iſſues, and be kindled occaſionally? or, whether it comes from the excited tube pervading inſtantaneouſly the body it is applied to? or, laſtly, whether there are certain principles in the air, which are thus agitated into an extemporaneous lightning? Theſe queries are certainly very comprehenſive and important; I wiſh I were able to return you ſomewhat more ſatisfactory than ſuppoſitions.

I incline to think the electrical and luminous *effluvia* to be the ſame, and not diſtinct ſubſtances. Mr *Hauksbee* ſeems to diſtinguiſh them, intimating, that no luminous matter would be communicated from an excited cylinder of wax to his finger, when brought near to the cylinder, though it attracted light bodies; but it is to be obſerved, that this cylinder of wax was only a coat of wax, of about half an inch thick, on a wooden cylinder of four inches diameter: now I have always found my ſtick of wax, which conſiſts of nothing elſe, to emit luminous *effluvia* very plentifully, and rather in a greater degree than the glaſs tube.

If we conclude with the *Engliſh* philoſophers, that fire is mechanically producible from other bodies, by colliſion, attrition, *&c.* or, according to Sir *I. Newton*, by putting the ſulphureous particles of bodies into a very ſtrong vibratory motion; by which means they become hot and lucid, *i. e.* affect us with ideas of light and heat; on this ſuppoſition may we not conclude, that the action on the glaſs tube, when it is rubbed, by putting the parts of it into ſuch a vibration, and, conſequently, agitating violently the ſulphureous particles therein, may heat and kindle them? And may it not alſo be ſuppoſed, that when the air is in a due ſtate, nitrous or other particles in the air may contribute to the kindling them? or, perhaps, rather that ſubtil, active, elaſtic ſubſtance, which Sir *I. Newton* ſuppoſes to be the cauſe of the refraction, *&c.* of light, and which communicates heat to bodies, and is univerſally diffuſed? Theſe *effluvia*, being thus agitated and conveyed by a non-electric body intervening, in a due quantity, to the vapour of the warmed ſpirit, may be ſuppoſed to kindle them, without exciting any originally-reſident fire in the body immediately communicating with them; the luminous *effluvia* from the finger, or ice, *&c.* when brought near the inflammable body, being, as far as we can perceive, of the very ſame kind with thoſe which proceed from the tube; or there is nothing appearing in them which may lead us to ſuſpect they are not the very ſame, tho' in a greater quantity than what can come from the part of the tube you approach with the end of your finger.

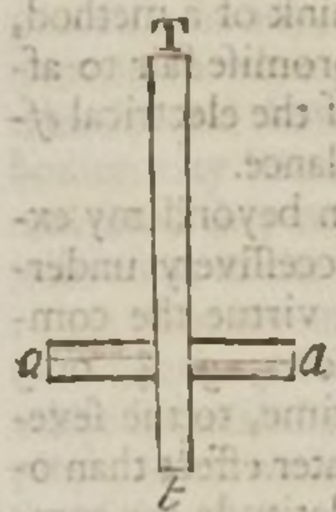
If we conclude with ſome of the foreign philoſophers, *Boerhaave*, *Homberg*, *Lemery*, *s'Graveſand*, &c. that fire is equally diffuſed throughout the univerſe by the Creator, pervading the interſtices of all bodies, and that there is no fire mechanically produced *de novo*; then, may we not conclude,

clude, that whereas, by attrition of the glaſs tube, there is produced a a very quick and ſtrong vibration of its parts, which muſt neceſſarily affect the fire contain'd in the vacuities, by compreſſion and relaxation; ſo that, as *Boerhaave* expreſſes it, there muſt be, in the bodies thus agitated, and in the fire contained in its pores, an exceeding great motion excited, and, together herewith, the ſurrounding fire from both theſe cauſes, muſt be agitated, and ſo much the more violently, the nearer it is; may we not conclude, that its force will be hereby ſufficiently increaſed to kindle the ſpirit to which it is convey'd?

In this, as in the former hypotheſis, I would not exclude the elaſtic *materia ſubtilis* from being ſuppoſed the having an influence on the *effluvia*. Whichſoever of the two *hypotheſes* we embrace, you may perceive, that I incline to think, that the kindling fire rather proceeds from the excited tube, I am very ſenſible I am in a great meaſure groping in the dark; but hope future experiments will caſt a light on this obſcure ſubject.

A letter from the ſame, concerning the Electricity of water. Ibid. p. 91. Dated Feb. 20. 1745-6. Read Feb. 27. 1745-6.

14. I this afternoon, on reflecting afreſh on Monſieur *l'Allamand*'s experiment, reſolved to make the following trial, tho' I was in no doubt what the iſſue would be: I took my tin tube, which has two arms to it, directly oppoſite one to another; and at that diſtance from one end of the tube, which is equal to the length of one of the arms, as you may perceive by the figure in the margin (not to trouble you with the uſe it was made for, at preſent): this I ſuſpended by a ſilk line from the ceiling of the room, letting it hang down of a length convenient for my purpoſe. I then took a china baſon, holding better than a quart, and, having nearly filled the ſame with water, I ſtood on the wax cake, with this baſon of water in my hand, ſo near the pendulous tube, that I could apply the baſon to it with convenience: then, having ſuffered myſelf to be electrified, I held the baſon ſo under the tube, that the lower end dipped an inch more or leſs in the water: upon this, a perſon approached one end of one of the arms with the ſpirit of wine in a ſpoon, and it was immediately kindled with vehemence; and at the ſame time I received on one of my fingers that held the baſon a pungent ſtroke; and that ſtroke was given the very inſtant of time the ſnap was at the ſpoon, or any other object that was applied. The wind was then S. and hard rain, as moſt part of the day; and yet, if one were diſpoſed to indulge imagination, the *effluvia* ſeemed to act more ſtrongly than is uſual. I think there can be no doubt, but that water is as good a *medium* of communication to the *effluvia*, as any ſubject whatever; for, that all thoſe which came to the ſpirit were conveyed to the tube by the water, I am certain; ſince the tube dipped in the centre, and was then motionleſs; ſo that it never came ſo near the baſon as to receive any *effluvia* from it.



T, t, the tube.
a, a, the arms.
At *t* the baſon of water was held.

 15. As

A letter from —— to Mr John Ellicot, *F. R. S. of weighing the strength of electrical* effluvia. N°. 479. p. 96. Mar. *and* Apr. 1746. *Read* Mar. 6. 1745-6.

15. As you were the only perfon who ever fhewed me any electrical experiments, and have been fo kind, according to your wonted candour, to affift me freely upon this and all other like occafions; I think it proper to give you this firft account of what I have thought of towards gaining a farther infight into the nature, power, and laws of Electricity.

From the time I faw thofe experiments at your houfe about 3 years ago, I had little or no opportunity of making any myfelf, until within this month; when, having got fome good utenfils, I repeated, or imitated moft of the trials I had heard of, with fuccefs. And particularly having heard, that Mr *Gray* gave an account of balls caufed to move round one another by means of electrical *effluvia*, I was very defirous of feeing fo delightful a fight. And though I was difappointed in my expectation of a circular motion, yet I found it eafy to make two balls act upon each other, in a very entertaining manner, for a long time; and that with fuch a conftancy and regularity, as to the effect, that I apprehend one may thence deduce a gauge or ftandard, whereby to meafure electrical powers, and compare the quantities and ftrength of the virtue infufed into, or remaining in, non-electrical bodies after given times, *&c.*

This, together with a great defire to be able to eftimate and compare the effects of experiments with fome certainty, and to do fomething more than amufe myfelf and friends with the feveral furprifing *phænomena* which thofe experiments produce, led me, about 10 days ago, to think of a method, which, for aught I know, is quite new, and feems to promife fair to afford much new light: it is to try or weigh the ftrength of the electrical *effluvia*, virtue, or power, by caufing it to act upon a balance.

I found, the firft day, that this method anfwered even beyond my expectation; fo that feveral non-electrical balls placed fucceffively underneath one of the fcales, and then imbued with electrical virtue the common way, would prefently caufe that fcale to defcend 2, 3, 4, or 5 inches, and feem to cleave, for 10 or more feconds of time, to the feveral bodies fo placed underneath, fome having much greater effect than others. Whence it appeared, that there was a fufficient latitude for comparing very different forces, if any fuch there were. At the next and only opportunity I have had fince (my *apparatus* being made more commodious), I ufed flat inftead of globular bodies, and then I found the effects far more confiderable; fome of them, whofe upper furface was about 3 inches fquare, having attracted and held down one fcale, when there were about 200 grains weight in the other.

Though I am tempted to communicate fome things, which I have already obferved by this means, with much delight, I referve them at prefent for a farther examination; defiring in the mean time, that you will communicate or divulge this in fuch manner as you think proper (only concealing my name), that others, who may have an inclination, may purfue and improve the hint. And, for the eafe of fuch, I muft add, that the ftrings of that fcale which is to be acted on, muft be long, and non-electrical, and, I think, thick; that there may be a ready paffage

for

for the electrical virtue to run off as faſt as it is received. Inſtead of a braſs ſcale-pan, I uſed a flat piece of cork, filed very ſmooth and even, eſpecially on the under ſurface. The other ſcale needs no alteration, provided the ſtrings be made of ſilk, as uſual, and ſhort enough to keep that ſcale out of the reach of the electric virtue, which is to act upon the former. If the beam were three or four feet long, the ſtrings of both ſcales might be of a length, which would make it leſs troubleſome to put in and take out weights.

I mounted the attracting bodies upon ſmall taper ſticks, about 2½ feet, whoſe thicker ends had a foot which ſtood upon 2 cakes of bees-wax full 10 inches thick in all.

I forbear to deſcribe the pretty little ſimple inſtrument you furniſhed me with at my firſt ſetting out; I leave that to yourſelf; only, as it has no name, I take the liberty to call it an *electrical needle.* Every body, who delights in ſuch matters, will thank you for it, if it were only for the amuſement it will furniſh for ſo many hours, after being but once well ſeaſoned, or tinctured with electrical *effluvia.*

But, I think, this little inſtrument, and the balance together, cannot fail of informing us farther concerning the properties of Electricity: ſuch as, how far it agrees and diſagrees with magnetiſm; whether it paſſes through the ſubſtance, or only along the ſurface of bodies; whether it proceeds in any, and what particular direction, or has any particular tendency; in what particular bodies the moſt of it may be collected and retained; and how long; how far the figure, ſize, denſity, or colour of bodies may be concerned; whether, as theſe *effluvia* may be felt, heard and ſeen, they may likewiſe be weighed; and many other matters, which will occur to the diligent obſerver.

16. On my making uſe of one of my boxes filled with pitch, wax, *&c.* for the perſon to be electrified to ſtand upon, after uſing it a little while ſucceſsfully, I got the man who aſſiſted to wipe the ſurface of the pitch, *&c.* with a dry clean cloth, ſuſpecting, from the place it had ſtood in, ſome dampneſs might lodge thereon. This being done, for my ſatisfaction I ſet up the box on one ſide, and held a thread of trial at a proper diſtance, and found it to attract and repel the ſame: but, on ſetting it down, and ſtanding upon it, by no means could it be made appear that I was electrified, or any other perſon who ſtood thereon afterwards. I thereupon took another box of the ſame ſort, but made uſe of it without wiping it, and it performed well. This I have not yet repeated, but intend to do it.

Part of two letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. containing ſome* Electrical *Obſervations. Ibid.* p. 58. *Dated* Mar. 20. 1745-6. *Read* April 17. 1746.

In a pint-bottle of flint-glaſs I have ſome ſmall pieces of braſs-leaf, and the bottle hermetically ſealed. Upon trying whether the excited tube would much affect the ſaid leaf, I was at firſt diſappointed in my expectations; for tho' the tube was ſo well excited, as that, upon bringing it near the bottle, ſtrong and loud ſnaps were given, there was hardly any ſenſible motion in the braſs-leaf, till I thought of warming the bottle at the fire; and then there was a conſiderable one, tho' not what I expected before I made

made any trial. But I suspect the bottle to be too thick; for, on trying a common flask, which we sealed in the fire, the leaf which I had put in was very strongly both attracted and repelled a great many times.

One odd circumstance I will tell you, and detain you no longer: Upon my lifting up the tube hastily by chance, I observed the leaf to be powerfully attracted by the sides of the bottle or flask next to the tube; this put me on trying purposely what the effect would be, if, when a person held either in his hand sideways, so as the neck was parallel with the horizon; I took the excited tube, and moved it up and down towards and from the floor, at 3 or 4 inches from the bottle, successively, as fast as I could, without hazarding my striking against it; upon which the brass leaf was as successively attracted and repelled, or seemed to follow the motion of the tube, or was affected, as it would have been if I had beat the air upon it, tho' in a very inferior degree, as you will suppose; and thus it would be, if the tube was held at a greater distance; and in the flask, I carried my hand so as that the tube described a circle about it, at the distance of 6 or 7 inches, the whole of the leaf would be put into a constant, regular gyration, which would hold as long as I could well continue the motion. This seemed to me strange, that if I brought the tube near, and removed the same slowly, no motion (especially in the bottle) was observed, or what was next to none; and yet that this sudden motion of the tube should produce such an effect; but I think it may be thus accounted for: while the tube is held near the bottle, *&c.* for any time, the leaf-brass is kept in a state of repulsion; and therefore, under that confinement in the bottle, is motionless; but on my sudden withdrawing the tube, the side of the glass opposite the leaf serves as an attractive to it, while the side on which it lay repels it; and thus, by the motion of the tube mentioned, there is a constant succession of attraction and repulsion.

The second letter, dated Apr. 16. 1746.

It may be hardly worth while to tell you, that I fired common spirit of wine, at the distance of 25 feet, the *effluvia* being conveyed by 3 persons and 2 laths of deal, tyed together thus: the person to be electrified immediately standing on a cake of wax, and holding one end of the lath, another person standing about the middle of the distance on another cake, and supporting the lath, and a third person at the further end, who held the other end of the lath, and fired the spirit; and sometimes held the spoon, while a fourth person fired them by repulsion. In this experiment, instead of common thread, I used silver and gold twist, or what, I think, the ladies call plate; and I have reason to think this much better than the former.

I am so far from being of Abbé *Nollet*'s mind, that I think no sort of glass is proof against the effects of a moist air. I conclude this from Mr *Watson*'s experiments and my own.—I told you before where I kept my tube; and I can assure you, I find as great a difference as can well be in the same tube, between what it is one day and the next, even when I have seen no great reason to expect, from any sensible change in the

air,

air, it ſhould be ſo. But whence ariſes that we call moiſtneſs in the air? I have many times known, that the wind being N. and N. E. and tho' it has rained all day inceſſantly, the air has been as dry (ſo far as I could judge from natural hygrometers, and from my tube) as in a fair day; and than ſome fair days, drier, by the ſame indications.

I begin to think, that, by careful practice, the glaſs tube may be brought to be a good hygrometer for the air. I wiſh the theory of the air were more diligently and accurately conſidered: certainly it has been neglected; ſo Mr *Locke* thought, a little before he died; and ſaid, the imperfect diſcourſe of Mr *Boyle*'s, which was printed after his deceaſe, was the beſt account we had. And what has been done ſince?

I was going to tell you (for I write in a hurry, that I may not loſe the conveyance which offers), that I believe cuſhions, the caſe hair-cloth, and the ſtuffing of horſe-hair, may be made to anſwer inſtead of wax-cakes. I have one not 3 inches thick in the middle, even when it is not compreſſed, which will do well.

Extract of a letter from Mr John Hen. Winkler, *Gr. & Lat. Litt. Prof. publ. Ordin. at* Leipſick, *to a friend in* London; *concerning the effects of* Electricity *upon himſelf and his Wife.* N°. 480. p. 211. May *and* June 1746. *Dated* Leipſick, April 22. May 3. 1746. *Read* May 29. 1746.

17. When I heard of Mr *Muſſchenbroeck's* experiment *, I tried the ſame; but I found great convulſions by it in my body. It put my blood into great agitation; ſo that I was afraid of an ardent fever; and was obliged to uſe refrigerating medicines. I felt a heavineſs in my head, as if I had a ſtone lying upon it. It gave me twice a bleeding at my noſe, to which I am not inclined. My wife, who had only received the electrical flaſh twice, found herſelf ſo weak after it, that ſhe could hardly walk. A week after, ſhe received only once the electrical flaſh; a few minutes after it ſhe bled at the noſe.

I read in the news-papers from *Berlin*, that they had tried theſe electrical flaſhes upon a bird, and had made it ſuffer great pain thereby. I did not repeat this experiment; for I think it wrong to give ſuch pain to living creatures. I therefore take, inſtead of men or brutes, a piece of metal, and I put it upon a ſtand under the electrical pipe, which pipe propagates the Electricity. To this metal is faſtened an iron chain, which goes about the bottle with water, in which the braſs wire is put, which wire is faſtened to the electrical pipe.

When then the Electrification is made, the ſparks that fly from the pipe upon the metal are ſo large and ſo ſtrong, that they can be ſeen (even in the day time) and heard at the diſtance of 50 yards. They repreſent a beam like lightning, of a clear and compact line of fire; and they give a ſound that frightens the people that hear it.

A letter to Mr Benj. Robins, *F. R. S. ſhewing that the* Electricity *of glaſs diſturbs the* Mariners

18. While ſo many gentlemen are labouring to find out the uſes of Electricity, it has been my fortune to diſcover one, at leaſt, of the inconveniencies attending that property in glaſs. And as it is ſuch whereby vaſt numbers, very likely, have been, and may hereafter be, greatly prejudiced, I deſire you will mention what follows to the *Royal Society*; to the end that it may be publiſhed, if they think proper, for the benefit of others, and particularly of thoſe who uſe the ſea.

* That with the gun barrel ſuſpended as the iron bar. See above, Art. 5.

Having

Compass, *and also nice* Balances. *Ibid.* p. 242. *Dated* June 10. 1746. *Read* June 12. 1746.

Having lately had occasion to compare together two compasses of a different make, the one having a bare needle, as usual, and the other a chart, in the manner that mariners compasses are commonly made, I happened to wipe off with my finger some dust, which lay upon the glass of the former; and thereby put the needle, which was before at rest, into a violent disorderly motion, partly horizontal, and partly vertical, or dipping. After several repetitions of the same thing, I found that the glass, by so slight a touch, was at that time excited to Electricity, so far as to disturb the needle extremely.

The same glass being rubbed a very little more with a finger, a bit of muslin, or of paper, would attract either end of the needle, so as to hold it to the glass, for several minutes, far out of the due direction, according to what part of the glass was most excited.

And when the needle has for some time adhered to the glass, and afterwards dropt loose, and made vibrations, those vibrations would not be bissected, as usual, by that point where the needle should rest, but either be made all on one side, or be very unequally divided, by means of some remains of electrical virtue in that part of the glass which had attracted the needle; until at length, after 15 minutes or more, all the Electricity being evaporated, the magnetical power took place.

The cure for this inconvenience, is to moisten the surface of the glass: even a wet finger will do it immediately and effectually.

I need not suggest, that the same quantity of friction will not at all times have the same effect upon these glasses, any more than it will upon the electrical tubes; but take the liberty to hint, that I have reason to believe that glass does, at some times, become in some degree attractive without any friction at all; and may possibly be excited by great concussions in the air, such as thunder, or the discharge of great ordnance, *&c.* and, if so, may thereby disturb the compass.

I must however observe, that the mariners compass is much less dangerously moved by wiping or exciting the glass than the other; by reason that the excited part of the glass attracts that part of the chart which lies nearest, just underneath, without giving it so much verticity, as it does to the other sort of compass with a bare needle. And farther that the deeper, or the farther distant the needle hangs below the glass, the less disturbance it is likely to receive, by wiping, rubbing or otherwise exciting the cover.

I shall make no farther reflections upon these facts than to observe, first, that all the minute, irregular, reciprocating variations which have been observed in the directions of dipping and horizontal needles, as mentioned in some of the *Transactions*, may probably have been caused by the glasses which covered the instruments made use of: and, secondly, that the flat pieces of glass, often placed under the scales of an essay-balance, are likewise very capable of attracting, and making even the lighter scale preponderate, where the whole matter weighed is so very small. I have not tried this last, but do remember, that Mr *Ellicot*, a

Member

Member of your *Society*, did ſome years ago ſuſpect, if not find it certain, that ſuch pieces of glaſs did diſturb his balance, and had given him a vaſt deal of trouble, upon a ſuppoſition, that the beam itſelf was defective.

19. It ſeems to me that a glaſs ball, which has oftentimes been employed for violent diſtillations, and other chymical operations, does ſend forth the Electricity incomparably more ſtrong than any other glaſs, which never ſince it's making had been expoſed to a violent fire. As I am the firſt that has mentioned this notable circumſtance, be pleaſed to let me have the honour of this improvement in the Philoſ. Tranſ.

Extract of a letter from Mr Prof. Geo. Matthias Boſe *of* Wittemberg, *to Mr* W. Watſon, *F. R. S. on the Electricity of glaſs, that has been expoſed to ſtrong fires.* N°. 492. p. 189. Apr. *&c.* 1749. *Dated* March 1/12, 1748-9. *Read* April 6. 1749.

20. The electrifying glaſs uſed by M. *le Monnier* is an oblong ſpheroid, whoſe diameter from pole to pole is 4 or 5 inches longer than that at the equator, which is about 12 inches. Each of theſe poles is terminated in a ſtem, or portion of a hollow cylinder, about 3 inches in length, and one in diameter, ſpirally emboſſed on the outſide into a large male ſcrew: to each of theſe male ſcrews is adapted a female ſcrew of wood, cloſed at one extremity with a piece of ſteel, excavated in the center, to receive the ſteel pivots upon which the electrifying glaſs turns. Theſe female ſcrews of wood are ſo formed at their open extremity, that they graſp and cover as much at the poles, as nearly renders what appears of the glaſs ſpheroid a perfect ſphere: this with a deſign, that the wood may fix the more effectually, and embrace the electrifying glaſs. From the exterior ſurface of one of theſe wooden female ſcrews, a circular ledge riſes, and projects to the height of about 2 inches; the *ambitus* of which ledge is excavated, to receive the cord that turns the electrifying glaſs. This is what they uſe here inſtead of our tubes, and with ſurpriſing effects, ſuch as greatly ſurpaſs what you have yet ſeen in *England*. The electrifying ſpheroid is turned by means of a wheel about 4 feet in diameter, with the ſame motion, and exactly in the ſame manner, as the ſpindle is turned round by the ſpinning-wheel: allowing a due proportion to the frame, upon which the glaſs ſpheroid is mounted, that it may anſwer to the wheel that turns it. The ſides of this frame, which ſtand perpendicular to the horizon, are near as ſtrong and as large every way, as the poſts of an ordinary cloſet-door; and, with the ledges that join them at top and bottom, form a rectangular parallelogram. The front of this frame is provided with ſilken loops, conveniently diſpoſed in ſeveral places, to bring to, and fix at a contact with the electrifying glaſs, wires, threads, packthread, or whatever elſe is to be electrified. Into one ſide of this frame, at about half it's height, the pivot that receives one of the poles of the glaſs ſpheroid is fixed; the other pivot, on the oppoſite ſide, is a round long bar of iron, ſcrewed into and paſſing through the poſt, in order to fix, or give liberty of removing the

Extract of a letter from Mr Turberville Needham *to* M. Folkes, *Eſq; P. R. S. concerning ſome new electrical Experiments lately made at* Paris. N°. 481. p. 247. Oct. *&c.* 1746. *Dated* Paris, *July* 4. N. S. 1746. *Read* Oct. 23. 1746.

electrifying glafs. This bar of iron, for the conveniency of turning it, has another in the nature of a leaver, which paffes through it's extremity at right angles with it. The whole machine is mounted upon a floor of boards, wheel, frame, glafs, *&c.* and employs two men, the one to turn the wheel, the other to fit behind the glafs fpheroid, and apply the concave of each hand to it's lower convex furface; for it is by this friction that the Electricity is excited.

When the electrifying glafs has been fome little time in motion, the perfon who defires to be electrified, applies the extremities of the nails of one hand, and ftands not upon cakes of wax, as in *England*, but within the area of a fquare drawer or box about five inches deep, and filled with five parts pitch, four of refin, and one of bees-wax: I will not call it a compofition, for they are not mixed, but difpofed in the following manner; the pitch is placed next to the fides of the box, and rifes almoft to a level with them, the refin in the middle is level with the pitch, and the wax forms a thin furface, covering both to a level with the box it felf; however, I fuppofe this to be in itfelf very indifferent, and that any one body of the electrics *per fe* would anfwer equally.

Exp. I. The perfon electrified by this machine not only emits fire from all parts of his body, upon the touch of another, with more vigour, and in a much more fenfible manner, than when electrified by a common tube; but fires alfo fpirits of wine with fuch eafe, that when the fpirits have been once but fimply fet on fire by a match or paper lighted, and the flame has been inftantly blown out, they will, with that fmall degree of heat they have acquired, take fire upon his touch 10 or 20 times fucceffively, without failing once.

I am told here, that they have frequently attempted in vain to fire fpirits with a common tube of glafs; fo that I believe the ufe of the tube has been more improved in *England* than in any other place: but it is a downright flavery, and in it's effects many degrees inferior to this machine. I fhould have thought, as this fo much exceeds in ftrength the common tube, that many glafs fpheroids, acting at once upon the fame body, would have confiderably increafed the effect; but M. *de Buffon* tells me, that M. *le Monnier* had found, upon trial, that they anfwered not his expectations; fo that it might feem there is a *ne plus ultra* in the intenfity of Electricity, as well as in the heat, which is communicated to boiling water.

Exp. II. If the perfon electrified holds a fword in one hand, the chamber being darkened, a continual flame iffues out at the point, in fmell and colour refembling the fumes of *phofphorus*, and near as ftrong as that of an enameller's lamp: with this difference, that when any other of the company applies a hand, even to the very point, where the concentred rays begin to diverge, it burns not, nor is any otherwife fenfible to the feeling, than as a continual blaft of wind.

Exp. III. This is performed with a fquare bar of iron, about 4 feet in length, and ½ an inch in thicknefs; to one extremity of which is adapted, by the

the help of a ſcrew, another piece of iron beat flat, like the end of one of the legs of a pair of tongs. This flat piece of iron being ſcrewed in, the bar is placed parallel to the horizon upon a wooden ſtand, and the ſtand within the area of the drawer or box, upon the pitch, reſin, and bees-wax, as above. The extremity of the bar, oppoſite to that, which carries the flat piece of iron, is covered with 3 or 4 folds of linen, to prevent any damage that might happen to the glaſs ſpheroid, in hitting againſt it by accident, while it revolves round it's axis; and the ſame extremity is moreover, for further ſecurity, placed at the diſtance of about $\frac{1}{4}$ of an inch from the glaſs itſelf, the effect being the ſame in every reſpect, as if in contact. The operator then orders the bar to be electrified by repeated revolutions of the glaſs ſpheroid, as above; and places one finger upon the middle of the bar, to prevent the communication of the Electricity from one end to the other, till he has covered the flat piece of iron with as much ſaw-duſt as it will carry. Some other of the company, in the mean while, takes up, on the point of a knife likewiſe, a quantity of ſaw-duſt, and holds it under the flat piece of iron, at about an inch diſtance. The effect is, that when the operator takes off his finger, the ſpheroid ſtill continuing to revolve, the ſaw-duſt above is all repelled and blown off, and that under attracted upwards. If, inſtead of ſaw-duſt, you place upon the flat piece of iron a ſmall ſquare tin box filled with water, or any other veſſel made of a matter non-electric *per ſe*, particularly metalline, and endeavour to draw off the water by a capillary ſiphon: the water, in that caſe, will fall drop by drop, as uſually; but the inſtant the bar is electrified, it will run in one continual ſtream; which, if the chamber be darkened, will alſo appear luminous. This play of the water may again be ſtopped at pleaſure, by the application of one finger to the bar, as above. If the flat piece of iron be unſcrewed and removed, the Electricity runs out at the extremity of the bar, to the eyes, in the appearance of a blueiſh flame; to the ſmell, like fumes of *phoſphorus*; and, to the feeling, like a blaſt of wind; as in the experiment of the ſword.

The moſt ſurpriſing of all, is that of Mr *Muſſchenbroeck*, improved by M. *le Monnier*. A muſquet-barrel open at both ends, is ſuſpended parallel to the horizon, by ſilken threads within reach: and at the breech end, about 3 inches from the extremity, is hung, by a ring of iron worked into the barrel itſelf, a ſmall iron chain about $\frac{1}{2}$ a foot in length. A glaſs phial, reſembling in ſize and ſhape a common vinegar-crewet, is then prepared, full of water and well corked, with an iron wire running through the cork almoſt to the bottom, and emerging ſome two or three inches above it, out of the top of the phial. The head of this wire is bent, to catch in the loweſt link of the chain; and is there to be ſuſpended, when it has been electrified. From the mouth of the barrel, which is pointed in a line parallel to the equatorial plane of the revolving ſpheroid, comes a long iron wire, inſerted into the barrel itſelf, as far as $\frac{1}{7}$ of it's length, and thence proceeding till it touches the glaſs ſpheroid; to a contact with Exp. IV.

which it is determined by one of the ſilken loops I mentioned above in the deſcription of the *apparatus*. Every thing being thus diſpoſed, the gun-barrel is to be electrified by repeated revolutions of the glaſs ſpheroid; which is to be in a continual contact with the long wire that proceeds from it. The phial is, at the ſame time, to be electrified by the operator, who takes hold of the body of the bottle, and applies to the electrifying ſpheroid the bent extremity of that wire, which paſſes from near the bottom of the phial through the cork, as I deſcribed above. The operator muſt take care not to touch the wire itſelf, while he endeavours to electrify the phial; otherwiſe he would be in the caſe of one, who ſhould aim to electrify himſelf, without ſtanding upon ſome one of the bodies, that are electrics *per ſe*. When the phial is ſufficiently electrified, which will be done in 8 or 10 revolutions of the ſpheroid; for I would not have any one be too free in beſtowing ſuch an efficacy upon it by too long an application, as might perhaps, occaſion his receiving a more violent ſhock than he would be willing to feel, particularly if the glaſs ſpheroid has been any time in action, and is much heated thereby; the phial is then, I ſay, to be ſuſpended by the iron chain, the glaſs ſpheroid continuing ſtill to revolve about it's axis, and to electrify the gun-barrel: the perſon then who has courage enough to ſuffer the experiment, for ſo I muſt expreſs myſelf, graſps the bottom of the electrified phial with one hand, and with the other touches the gun-barrel. At that inſtant, a great part of the nervous ſyſtem receives a ſhock ſo violent, that it would force the ſtrongeſt man to quit his hold, and turn him half-round.

I remember, among others of us, that tried the experiment, was a boy of about 14: I aſked him, what he thought of it; he told me, that he imagined, the inſtant he touched the gun-barrel, his arms had been broke ſhort off at the elbows, and that he had been cut into two parts juſt below the breaſt; another of the company, with a ſort of pun, termed it being broken upon the wheel. In effect, ſo far the boy was in the right, that the ſhock in the arms ſeems to extend no farther than the elbows, and that of the body no lower than the breaſt, without affecting however in the leaſt the head, or ſeeming to reach beyond the outward expanſion of the nerves: yet is it not to be termed a pain; for there is not the leaſt ſenſe of that ſort in it, but a mere ſudden convulſionary motion, or rather a ſhock, which ſurpriſes much, and is indeed an uneaſy, though not a painful ſenſation.

In this experiment, it is very remarkable, how greatly the force of the communicated Electricity is augmented, by the application of the electrified phial: but the moſt ſurpriſing circumſtance attending the uſe thereof, and which, I believe, is, among all the bodies that are ſuſceptible of Electricity, peculiar to this alone, is, that it loſes not entirely it's efficacy under ſeveral minutes; and I am told, that in a froſt it will retain it for 36 hours together.

M. *de Buffon*, who informed me that M. *le Monnier* was the firſt who diſcovered this particular, has alſo aſſured me, that this ſame gentleman had

frequently

frequently electrified the phial at home, and brought it in his hand through many ſtreets from the college of *Harcourt*, to his apartments in the king's garden, without any very ſenſible diminution of it's efficacy. The uſe of the electrified phial may be diverſified many ways: among others, are ſuch as follow.

When the phial has been ſufficiently electrified as above, the whole company join hands; the operator at one extremity of the line graſps the bottom of the electrified phial, and the perſon at the other extremity touches the wire, which riſes above the cork. At that inſtant, the whole company receives a ſhock, reſembling that in the experiment of the gun-barrel, but not ſo ſtrong; for it ſeems not at all to extend beyond the elbows. Exp. V.

This is the experiment, which abbé *Nollet* performed upon 180 of the guards, before the king, who were all ſo ſenſible of it at the ſame inſtant of time, that the ſurprize cauſed them all to ſpring up at once; as it will indeed force any perſon to do that ſubjects himſelf to the trial; though the convulſionary motion itſelf, as I obſerved before, reaches not beyond the elbows: but the greater or leſſer effect depends entirely upon the longer or ſhorter application of the phial to the electrifying ſperoid; and I am credibly informed, that when due precautions have not been taken in this particular, ſome perſons have received ſuch violent ſhocks, as have benumbed, and impaired, to a certain degree, the uſe of their arms for a day or two, before they perfectly recovered themſelves. I can aſſure you, however, from my own experience, that, with the precautions I have already taken notice of, there is no manner of danger, though at the ſame time a ſufficient efficacy may be communicated to the phial, to gratify any one's curioſity: and in this particular I have been the more prolix, leſt any bad conſequence ſhould happen to the unexperienced.

Another experiment with the electrified phial conſiſts, firſt, in placing a wire fixed in a pedeſtal, erect in a baſon of water, the head of which wire is bent, and riſes ſome 3 or 4 inches above the level of the water; and then, in touching the ſurface of the water with one hand, and the ſtanding wire with the wire of the electrified phial, which is graſped by the other hand, as in the preceding experiments. The effect of this is much more violent than that of the laſt experiment, and I think, exceeds even the ſhock of the gun-barrel; ſo that here the utmoſt precaution muſt be uſed, not to electrify the phial too much. Exp. VI.

I obſerved particularly upon the trial of this, that the operator, who appeared to be very expert, and quite familiarized with every former effect, ſhewed however ſome apprehenſion, and was unwilling to lead the way, as he had done in all the other experiments.

If the electrified phial is held in the hand, and the chamber is darkened, the wire inſerted in it is perceived to emit a ſtream of fire at it's extremity without any diſcontinuance; but if it is ſuſpended by a ſilken thread, the fiery eruption inſtantly ceaſes. Exp. VII.

This,

This, as a perfon would be apt to imagine, gives fome infight into the reafon of it's retenfion of Electricity; the ambient glafs and filken thread being in the number of the electrics *per fe*, which have a power of determining to, and confining in, any other kind of body, a communicated Electricity, though they are not fufceptible of it themfelves Yet, as the *French* obferve very well, there are fo many of what they term *bizarreries*, or unaccountable *phænomena*, in the courfe of electrical experiments, that a man can fcarce affert any thing, in confequence of any experiment, which is not contradicted by fome unexpected occurrence in another: at leaft, this is my prefent thought of the matter; and I am the more confident in advancing it, fince that I have learnt your friend M. *de Buffon* is of the fame opinion, for whofe judgment I have the greateft deference. I remember he told me one day, when I had the honour of waiting upon him, that he thought the whole fubject of Electricity, though illuftrated with fo great a variety of experiments, very far from being yet fufficiently ripe for the eftablifhment of a courfe of laws, or indeed of any certain one, fixed and determined in all it's circumftances. An inftance of this, among others that are or may be found out, will appear in the following experiment.

Exp. VIII. If the non-electrified phial is placed upon a glafs falver, it acquires from the revolution of the fpheroid no Electricity, though it's wire is in contact with it all the time; unlefs the finger of fome one in the company is approached very near to the phial itfelf: but, in that cafe, it receives it vifibly from the finger; infomuch that, if the chamber is darkened, you will fee the electrical fire ftreaming out of the finger, and entering into the water, through the body of the glafs phial, which is thereby immediately impregnated with it; and this, though the hand fhould be placed even under the glafs falver itfelf.

Here we fee an example, where an electric *per fe* is fo far from terminating or excluding the power of Electricity, that it is even made a *medium* of communication in circumftances where the wire, which is a non-electric *per fe*, refufes to perform it's expected office. When I fpeak of the power of Electricity in this cafe, I would not be underftood of the power of attracting light bodies, which is well known to be fcarce fenfibly interrupted by a glafs *medium*, as appears in the common experiment of an electrified tube, acting upon leaf-gold, in a cryftal bottle: though even this, if duly confidered, might create fome difficulty; but I would only be underftood of that communicated virtue, which renders non-electrics *per fe* electrical. In one word, the fingularity of this experiment is, that, by the addition of the glafs falver, the wire and the water, both of them non-electrics *per fe*, fhould not be in the leaft affected without the approach of the hand, and fhould then receive the electrical fire from it through a glafs *medium*; notwithftanding they are in the very fame circumftances, that a man is in, or any other non-electric *per fe*, placed upon a cake of wax and in contact with the electrifying fpheroid. Now, that in this experiment the glafs falver has a confiderable effect, is very clear. For, if the phial is placed upon the table, or upon a ftand, without the

falver,

ſalver, a few revolutions of the ſpheroid will with eaſe communicate a ſtrong Electricity to it; particularly if any one touches the table or ſtand it is placed upon: and to know whether any degree of Electricity has been communicated or not, the phial is to be brought to the teſt of any of the preceding experiments.

If the electrified phial is placed upon a table, and any light body is ſuſpended by a ſilver thread, within the diſtance of about 2 inches from the phial, what I ſaw was a ſmall braſs bell of a lap-dog's collar, the phial will attract that light body to it with force, if anyof the company touch the wire of the phial; but if the phial itſelf is touched, it will repel it with a force equal to its attraction in the former caſe. Exp. IX.

This experiment conſiſts in the communication of the electrical fire from the glaſs ſpheroid to many perſons at once, as in *England*, from a tube; with this only difference, that the company do not here join hands, but are united to each other by taking hold of iron chains, which ſurpriſingly increaſe the force of the communicated Electricity: for it is to be obſerved, that, whenever the communication is carried on by a metallic *medium*, the effects are much the more ſenſible. Exp. X.

This experiment is no other than what has been frequently tried in *England*, the attraction of leaf-gold by a hollow wooden globe, to which Electricity is communicated, by a pack-thread of a very great length ſuſpending it; after it has been conducted over ſilken threads croſſing the chamber at ſeveral diſtances, in a ſort of ſpiral, conſiſting of as many turns as the place will admit. Exp. XI.

I had almoſt forgot to take notice of two particulars, which were the conſequences of ſome of the preceding experiments, and may in ſome meaſure ſerve to illuſtrate them: the one regards the communication of Electricity; the other, it's ſurpriſing force.

At the grand convent of the *Carthuſians* here in *Paris*, the whole community formed a line of 900 toiſes, by means of iron wires of a proportionable length, between every 2; and, conſequently, far exceeding the line of the 180 of the guards above-mentioned. The effect was, that, when the two extremities of this long line met in contact with the electrified phial, the whole company, at the ſame inſtant of time, gave a ſudden ſpring, and all equally felt the ſhock, that was the conſequence of the experiment.

The other *phænomenon* was the reſult of a late experiment of abbé *Nollet*'s. He fixed, at the two extremities of a braſs ruler, two ſmall birds, a ſparrow and a chaffinch: this ruler had a handle or pedeſtal faſtened to the middle of it, for the convenience of holding it. When both the gun-barrel and the phial had been ſufficiently electrified, as in the 4th experiment, he applied the head of the ſparrow to the ſuſpended phial, and the head of the chaffinch to the barrel. The conſequence, upon the firſt trial, was, that they were both inſtantaneouſly ſtruck lifeleſs, as it were, and motionleſs, for a time only, and they recovered ſome few minutes after: but, upon a ſecond trial, the ſparrow was ſtruck dead, and, upon examination,

nation, found livid without, as if killed with a flash of lightning, most of the blood-vessels within the body being burst by the shock. The chaffinch revived, as before.

Extract of a memoir concerning the communication of Electricity; *read at the public meeting of the* R. Acad. *of* Sciences *at* Paris, Nov. 12. 1746 *by M.* le Monnier *the younger, M. D. of that* Academy, *and F. R. S. communicated by the Author to the* Pre. *of the* R. S. *Ibid* p. 290. *Read* Dec. 11. 1746.

21. The author of this memoir proposes therein to examine these 3 questions; that is to say, how is this electric virtue to be communicated to such bodies as have it not, and which are not capable of acquiring it by bare friction only? How is the electric matter propagated? And, lastly, in what proportion is it distributed?

As to the first, the author observes, that this electric virtue is no other way to be communicated, but by the near approach of a body already actually possessed of the same: That the rule laid down by M. *du Fay*, *that bodies never receive Electricity by communication, unless they are supported by bodies electric in their own nature*, does not always take place, and that it is subject to great exceptions. For, first, in the *Leyden* experiment, the phial filled with water is strongly electrified by communication, even when carried in the hand, which is not a body electric by nature. Secondly, all bodies that are electrified by means of a phial of water fitted to a wire, and which has already received a great degree of virtue by communication; all such bodies, I say, placed in any curve line, connecting the exterior wire, and that part of the bottle which is below the surface of the water, acquire Electricity, without being placed upon resin, silk, glass, or the like.

Thus one may give a violent concussion in both the arms to 200 men all at once, who holding each other by the hand, so form the curve just mentioned, when the first holds the bottle, and the last touches the wire with the end of his finger; and this, whether these persons actually touch each other's hands, or whether they are connected by iron chains, that either dip in water, or drag upon the ground; whether they are all mounted on cakes of resin, or whether they only stand on the floor; in all which cases the experiment equally succeeds.

Electricity has in this manner been carried through a wire of the length of 2000 toises, that is to say, of about a *Paris* league, or near 2 *English* miles ¼, tho' part of the wire dragged upon wet grass, went over charmil hedges or palisades, and over ground newly ploughed up.

Thirdly, the water of the bason in the *Thuilleries*, whose surface is about an acre, has been electrified in the following manner: there was stretched round half the circumference of the bason an iron chain, which was intirely out of the water: the two extremities of this chain answered to those of one of the diameters of the octogon: an observer, placed at one of these extremities, held the chain with his left hand, and dipped his right at the same time into the water of the bason; whilst another observer, at the opposite side of the bason, held the other end of the chain in his right hand, and a phial well electrified in his left: he then caused the wire of his phial to touch an iron rod, fixed upright in a piece of cork that floated near the edge of the bason; at that instant both observers felt a violent shock in both their arms. This same fact

was

was again confirmed, by experiments made upon two basons at the same time, that it might appear distinctly, that the electrical *effluvia* did really pass along the superficies of the water.

Fourthly, it has been confirmed, by repeated comparisons, that a bar of iron placed in the above-mentioned curve, does not at all acquire more Electricity, when it is suspended in silken lines, than when it is held in the bare hand. Whence it appears, that, in this case, the contiguous non-electric bodies do neither partake of, nor absorb in any way, the Electricity that has been communicated.

Besides many strong exceptions to the rule laid down by M. *du Fay*, the author adds another yet stronger, and indeed directly contrary to that rule; which is, that the same phial of water, fitted with it's wire, receives either no virtue at all, or at least none that is sensible, so long as it is either placed upon a stand of glass that is very dry, or that it is suspended by a silken thread, whilst it's wire rests upon the globe; and that, to make it receive the virtue, the part of the phial which is below the surface of the water, must communicate with some body that is not electric; as is evident, when it is touched, whilst it rests on the stand of glass, with the finger, for it then instantly becomes electric: and the same will also happen when it is touched with a peice of metal; but not when it is touched with a tube of glass that is dry.

The electrical rests produce here upon the bottle an effect so contrary to M. *du Fay*'s rule, that, if one places a phial, perfectly well electrified, and which throws out the pencil of fire copiously, upon a dry stand of glass, or upon a line of silk; it's light immediately goes out, and it's Electricity is as it were laid to sleep. One may then securely approach the finger to it's wire, and there will come no electrical sparks from it. The author has even drawn out of it entirely both the wire and the cork, and has kept it half an hour in his pocket, without destroying the Electricity. But one must only, in this case, touch the wire, and not the phial itself; for in touching the two at the same time, one returns to the *Leyden* experiment; but when one touches the phial only, the Electricity revives in the wire, and the pencil of fire displays itself again, provided one has not staid too long: but if the wire only is touched, the body of the bottle becomes strongly electric, and draws to it, from a considerable distance, any light substances.

This last case gives room to an experiment that looks at first like magic: there was hung up a little tinkling bell by a silver wire, at the height of 8 or 9 feet, and there was placed upon a glass stand well dried, a phial newly electrified; the centre of the bell, and that of the phial, were nearly in the same horizontal line; but the bell was between 6 and 7 inches from the surface of the phial. Every thing being in this state, the bell remained quite still, if the stand was very dry; but the instant one either approached a finger, or any other non-electric body, to the wire of the phial, the bell leaped to it: and one might begin again, and repeat the experiment 20 times together, without having any occasion to new-electrify the phial.

With regard to the propagation of Electricity, the velocity with which the electrical matter is conveyed, has been found too great to be yet determined with any exactness.

The author made an experiment with an iron wire of 950 toises in length, and he was not able to observe, that there passed so much as a quarter of a second of time, between the wires receiving the Electricity at one end, and his feeling the shock in both his arms at the other; which infers a velocity at least 30 times as great as that with which sounds are propagated.

In seeking what might be the force which shot forward the electric matter, with so much rapidity, through the length of the wire, he at first thought it might be performed by the explosion of the spark of fire, which is perceived when the electrified phial is brought into contact with the wire conducting the electric matter; but the following experiment soon convinced him he was mistaken.

He disposed horizontally a wire folded in two, upon lines of silk; the whole length of this wire was of 1319 feet, and the two parallel halves were at the distance from each other of about six feet: the Electricity was then communicated by means of a phial, and it preserved itself in the wire for several minutes, by reason of the silken lines upon which the same was supported: a finger was then brought to one of it's extremities to take away the virtue; and in the same instant it ceased also at the other extremity of the wire: so that, in this case, the matter in question returned to the finger, that is to say, marched backward, with the same velocity with which it was before shot forwards: the electric matter therefore now came towards the explosive spark, for this spark appeared upon the finger as soon as it approached the end of the wire to take away it's Electricity, and therefore it is not this spark which shoots forward the electric matter with so great a velocity.

The last part of the memoir concerns the proportion in which the electric matter is communicated to bodies of the same nature. And here the author first establishes, that it is not communicated to homogeneous bodies, in proportion to their masses or quantities of matter, but rather in proportion to their surfaces. Yet all bodies having equal surfaces do not receive equal quantities of Electricity: those receive the most, whose surfaces are extended the most in length. Thus a square sheet of lead receives a much less quantity of Electricity, than a strip of the same metal with a surface equal to that of the square sheet: insomuch that the only way to increase in any body it's faculty of receiving the electric virtue, is continually to increase it's length.

Observations upon so much of the preceding Article,

22. The world is much obliged to M. *le Monnier* for the many discoveries he has made of the power of Electricity; though the reason of my troubling you with this paper at this time, is my differing with that gentleman

gentleman in the conclusions which he deduces from several of the experiments contained in his memoir.

as relates to the communicating the eliƈtric virtue *to* non electrics, *by* Wm Watson, *F. R. S.* N°. 482. p. 388. Jan. *and* Feb. 1746. *Read* Jan. 29. 1746-7.

One of the questions proposed to be examined is, " in what manner " the electric virtue is to be communicated to such bodies as yet have " it not, and which are not capable of acquiring it by bare friction " only ?" M. *le Monnier* observes hereupon, " That no other manner " is known, by which the electric virtue may be communicated, besides " the near approach of a body actually possessed of the same : that the " rule laid down by M. *du Fay*, *that bodies never receive Electricity by* " *communication, unless they are supported by bodies electric in their own* " *nature, does not always take place ; and that it is liable to great excep-* " *tions :* for, first, in the *Leyden* experiment, the phial filled with water " is strongly electrified by communication, even when carried in the " hand, which is not a body electric by nature."

To this I answer, that M. *du Fay*'s rule is confirmed by all the experiments yet made public, and even by that of *Leyden* quoted by our author, or what is usually called that of Professor *Musschenbroeck*. For, in this experiment, is not the non-electric water contained in and supported by the glass phial, which is electric in it's own nature ? It's being carried in the hand is no more than it's being placed on any other non-electric body, and therefore is no proof against the general position. It is well known, that if the phial is made non-electric by wetting it's outside, so as not to leave some inches perfectly dry, between it's mouth and that part which is wetted, the water and phial part with the Electricity as fast as they receive it, unless it is stopped by another electric *per se*. But of this I treated at large, in a paper I lately did myself the honour to communicate.

Secondly, our author mentions, " that all bodies, which are electri- " fied by means of a phial of water fitted to a wire, and which has al- " ready received a great deal of virtue by communication ; all bodies, " he says, placed in any curve line, connecting the exterior wire and " that part of the bottle, which is below the surface of the water, ac- " quire Electricity without being placed upon resin, silk, glass, or the " like : that thus a violent concussion may be given to 200 men all at " once ; who holding each other by the hand so form the curve just " mentioned, when the first holds the bottle, and the last touches the " wire with the end of his finger ; and this equally, whether they are " all mounted upon cakes of resin, or stand upon the floor : that the " Electricity has in this manner been carried through a wire of the " length of 2000 toises, or near 2½ *English* miles ; part of which wire " dragged upon wet grass, went over hedges, palisado's, and over land " newly ploughed up."

The experiments in the second argument do no ways invalidate M. *du Fay*'s rule ; for the success of them depends upon keeping whatever forms the curve line mentioned by our author, whether it consists of men or wire, in a non-electric state : and if whatever forms this curve line acquires any degree of Electricity more than it's original quantity, which it is well known may be done, by being placed upon originally electrics, the effect of the ſhock is proportionably leſſened. Thus if a man, ſtanding upon electrics *per ſe*, applies his hand to the phial of water, ſuſpended by a wire to the electrified gun-barrel as uſual, this perſon will acquire Electricity, which will be ſufficiently perceptible in him, by his attracting light ſubſtances held near his body, or by his firing inflammable ones, when properly preſented to him; if, I ſay, a perſon thus electrified, by applying one of his hands to the phial, touches the electrified gun-barrel with a finger of his other, let the phial be ever ſo ſtrongly electrified, he feels but a ſlight ſtroke; and this ſtroke is greater or leſs, in proportion to the difference of the accumulation of Electricity in the body of the man, and that of the water in the phial. Thus we know from experiment, that though a conſiderable quantity of the Electricity, in impregnating the phial of water therewith, pervades the glaſs, yet the loſs thereof this way is not equal to what comes in by the wire: therefore we will, for the ſake of a more eaſy method of explanation, ſuppoſe, that the phial, when electrified in the moſt perfect manner, contains a quantity of Electricity equal to 10; that the man's body, by ſtanding upon wax, and touching the phial with one of his hands during it's Electrification, contains a quantity equal to 7: upon his touching the gun-barrel with a finger of his other hand, he will receive a ſmall ſtroke only equal to 3, the difference of the Electricity of the water and that of his body: and if he touches the gun-barrel again without removing his foot from the originally electric, the ſtroke will be ſcarcely perceptible, on account of his body being nearly of the ſame degree of Electricity with the water in the phial. So that here we ſee that the violence of the ſhock, to be felt by whatever forms the curve line, depends upon it's being, in the moſt perfect manner, free from any degree of Electricity more than the original quantity, which is contrary to the opinion of our author.

Thirdly, M. *Monnier* tells us, "That the water of the baſon of the "*Thuilleries*, whoſe ſurface is about an acre, has been electrified in the "following manner:

"There was ſtretched round half the circumference of the baſon an "iron chain, &c."

The water of the baſon in this experiment was no more electrified than the wire which dragged along the ground, *&c.* was in the former. When I was firſt informed, without being acquainted how, that an acre of water had been electrified, I was amazed, and told the gentleman who acquainted me therewith, that if my idea of Electricity was in the leaſt true, ſuch an effect could not be produced, without electrifiing the whole terraqueous globe from a larger maſs of matter. And indeed, when I heard M. *le Monnier*'s paper read I eaſily ſaw the deception: ſo that, inſtead of electrifying the whole quantity of water contained in the baſon, the

the Electricity paſſed only through ſo much of it as formed a line between the iron rod faſtened in the floating cork, and the hand of that obſerver which was dipped in the water.

Theſe experiments ſtill more and more eſtabliſh the account I lately laid before you of the Electricity's always deſcribing the ſhorteſt circuit between the electrified water and the gun-barrel; or (which is the ſame thing) the wire of the electrified phial. And this operation reſpects neither fluids or ſolids, as ſuch, but only as they are non-electric matter. Thus this circuit, in the preceding experiment between the phial and the wire, conſiſted of the two obſervers, the iron chain, the line of water, and the iron rod in the floating cork.

Fourthly, M. *le Monnier* mentions, " That it has been confirmed, by " repeated compariſons, that a bar of iron, placed in the above-men- " tioned curve, does not at all acquire more Electricity when it is ſuſ- " pended in ſilken lines, than when it is held in the bare hand: whence " it appears to him, that, in this caſe, the contiguous non-electric bodies " do neither partake of, nor abſorb in any way, the Electricity which has " been communicated."

The curve line before-mentioned, let it conſiſt of whatever non-electrics it will, unleſs the whole thereof be properly ſupported, the communicated Electricity cannot be accumulated: ſo that the ſuſpending one part thereof in ſilk lines cannot be ſuppoſed to produce any effect.

This gentleman further obſerves, " That the phial of water fitted to " it's wire does not receive the leaſt degree of Electricity, if it's wire, ſuſ- " pended by a ſilk line, is applied to the globe in motion, or if that phial " is placed upon a dry glaſs ſtand." This M. *le Monnier* takes to be directly contrary to M. *du Fay*'s rule; eſpecially as the phial cannot be replete with Electricity, unleſs, while it is exciting, ſome non-electric body touches the phial below the water.

That the phial of water receives no degree of electricity in this caſe, is not ſtrictly true: it receives as much as any other maſs of matter of the ſame bulk would, under the ſame circumſtances. For we find, that we cannot highly electrify the water, unleſs the Electricity from the globe be directed through the water and phial to the non-electric in contact; in which paſſage a great quantity thereof is accumulated, by it's not pervading the glaſs ſo faſt as it is furniſhed by the wire; and therefore we find, that when the water will contain no more, the ſurcharge runs off by the wire: ſo that this experiment, no more than thoſe which precede, contradicts M. *du Fay*'s opinion; the thinneſs of the glaſs permitting it, not wholly, but partially, to ſtop the Electricity. This matter is explained further under experiment the firſt.

I differ from this ingenious author with reluctance, inaſmuch as I greatly honour him, not only for his diſcoveries upon the ſubject of Electricity, but alſo for the pleaſure and improvement I received in my reading his learned and curious obſervations in Natural Hiſtory, made in the ſouthern parts of *France*, where he accompanied M. *Caſſini de Thury* in

meaſuring

meaſuring a degree of the meridian. Theſe obſervations are publiſhed with M. *Caſſini*'s book: but as the reverſe of ſeveral of the opinions delivered in his memoir is experimantally found to be true, and as the diſcovery of truth, and carefully ſeparating it from deception, ſhould be the only aim of our philoſophizing, I take the liberty of laying before you my opinion thereon.

Part of a letter from Mr John Browning, *of* Briſtol, *to Mr* Henry Baker, *F. R. S. Dated* Dec. 11. 1746. *concerning the effect of* Electricity *on* Vegetables. *Ibid.* p. 373. *Read* Jan. 22. 1746-7.

23. Having an operator at *Briſtol* with a good electrifying machine, I was deſirous to electriſe a tree, and therefore ſent him the following for that purpoſe; *laurus tinus*, *leucoium majus flore pleno ferrugineo*, and *ſtœchas citrina Cretica*. Theſe were not choſen with any deſign; their being the leaſt plants I had, was the only reaſon.

I promiſed myſelf the pleaſure of ſeeing their leaves erected when electriſed, but was diſappointed, (whether it's being the dormant ſeaſon of the year for all plants, might not be ſome hindrance, I cannot determine); neither did the leaves flag on their being touched. However, I was agreeably recompenſed by a ſtream of fine purple blue coloured light, much reſembling an amethyſt, that iſſued from the extremity of each leaf upwards, of an inch in length, when the finger, or any other non-electric, approached near it. This colour I attribute to the watry particles in the earth, having often obſerved the very ſame colour iſſuing from the long leg of a ſyphon. On putting my finger on the gun-barrel to ſtop the Electricity, the leaves of each tree had a trembling motion, which remained for ſome little time, and immediately ceaſed on withdrawing my finger from the barrel, and admitting the Electricity. This conſtantly happened, as I put my finger on or off the barrel.

The *ſtœchas* plant has a very long hoary leaf, and bears it's bloſſom on a very ſmall, ſlender, and almoſt naked ſtem, riſing near a foot above the body of the plant. This ſtem had a motion given it, when any non-electric was brought within about two inches of it's ſummit, much like the vibration of the *pendulum* of a clock; which vibrating motion was parallel with the breech of the gun, quite contrary to the ſame kind of motion I had before obſerved in a needle, hanging perpendicularly by a thread at the end of a gun; the needle always vibrating in the direction of the gun. The motion of the plant and needle always continued as long as the glaſs globe was excited.

I was alſo deſirous to be ſatisfied, whether Electricity could be propagated without mutual contact, by ſuſpending another gun in ſilk cords, about 2 inches from contact, and the Electricity was near as ſtrong in the ſecond gun as in the firſt. At the diſtance of between 3 and 4 inches, it was much abated, and ſo it gradually diminiſhed, as the diſtance increaſed to near 6 inches, where it would ſcarce attract a thread of trial.

I prevailed on a man to be let blood, and then placed him on a cake of pitch, but could not be ſenſible of any increaſe of velocity in his blood, by being electrized, as has been aſſerted.

I had almoſt forgot to mention, that the ſtrokes I received from the electrified garden-pots were more violent and painful to my fingers than from any other body I ever experienced.

Mr

Mr *Baker*, ſince his receiving the above account, has had an opportunity of electrifying a myrtle-tree, of between 2 and 3 feet in height, growing in a pot at the ſeat of the Duke of *Montague* at *Ditton*; in preſence of his Grace, of the *Preſident* of the *Royal Society*, and ſeveral other curious gentlemen; who found, that whenever the hand, or other non-electric body, was brought near the leaves, ſtreams of fine purple fire iſſued therefrom, together with a conſiderably cold air; and that the leaves would be attracted at ſome diſtance, and move vigorouſly towards a non-electric body.

24 Since I have read the *Tranſaction* * with reſpect to the ſparkling lady, who could communicate a kind of electrical fire to her garments, I can give you an inſtance nearly like it, of a lady who was ſurprized at ſuch an appearance from a flannel petticoat, which ſhe happened to ſhake in the dark. But at laſt, we found that new flannel, after ſome time wearing, would acquire this property; but that it loſt it by being *waſhed*.

Extract of a letter from Mr Benj. Coke, *F. R. S. to* Mr Peter Collinſon, *F. R. S. concerning the property of* new Flannel *ſparkling in the dark*. N°. 433. p. 457. Mar. *&c.* 1747. *Dated* Newport, Iſle of Wight. Jan. 13. 1746-7. *Read* March 19. 1746-7.

25. I fancy at laſt this ſparkling of the flannel, and ſuch-like bodies, will be found to be quite electrical: and it is poſſible, I conceive, that the acid ſteams of the ſulphur, burnt under the extended flannel in the time of bleaching, may unite themſelves with the oil (with which hair, as well as horns, are found by analyſis to be replete), and form an animal ſulphur, which, upon friction, vibration, or any nimble agitation of theſe hairs, may become luminous.

And that ſomething like this may be in the caſe, ſeems not improbable; ſince it hath been obſerved, that this appearance hath happened moſt conſpicuous in froſty weather; in which ſeaſon there is generally not only a greater purity of the air, and abſence of moiſture, but all hairy and horny ſubſtances (and hairs, you know, are but ſmall horns) are more elaſtic, and conſequently ſuſceptible of, and capable of exciting, the ſtrongeſt vibrations. And, on the contrary, the lixivial ſalts uſed in waſhing may deſtroy the ſulphureous acid, and diſcharge the oil; whence the hairs will become more flexible and limber, and be rendered leſs fit for exciting the electrical fire. And the ſame may happen when flannel is much worn, and by that means filled with the alcaline *effluvia*'s, which go off from moſt (of the higher order of) animals by tranſpiration; which may diſſolve the animal ſulphur, weaken the ſpring of the hairs, and ſo render the phænomenon more difficult.

Part of two letters from the ſame, concerning the ſparkling of Flannel, *and the* Hair *of* Animals *in the dark*. N° 488. p. 394. June 1748. *Dated* May 19. 1748. *Read* June 23. 1748.

It ſhould have been mentioned, that the flannel had been worn but few days; and that it was immediately upon ſhaking the under-coat from that

The ſecond letter, dated Newport, June 1. 1748.

* Art. 6.

which

which was worn above it, that the ſparks were emitted; and that their appearance was in a broad ſtreak almoſt contiguous, attended with a crackling or ſnapping, like what may be obſerved on moving the finger nimbly along over the prime conductor, when excited in the electrifying machine; of which the lady was able to form a compariſon, having afterwards ſeen ſome experiments of that ſort.

This appearance returned at the ſame time, and on the ſame occaſion, 2 or 3 nights after, but more languid, till it was quite loſt.

A lady, who was informed of this, leſſened the ſurprize (which had been thought almoſt ominous) by aſſuring, that ſhe had ſeen the ſame phænomenon often in new flannel, but never in any that had been long worn or waſhed: and that the flannel being rendered damp with ſea-water, and afterwards dried, would heighten the flaſhing, which ſhe imputed to the ſulphur uſed in bleaching. However that be, I ſhall only obſerve, that theſe ſparklings had the crackling criterion of electrical fire; and that hair and wool, as well as ſilk, are electrics *per ſe*, and unctuous and ſulphureous bodies more electric than others of the ſame denſity.

Dr *Wall* hath obliged the public with a curious diſſertation on a ſimilar ſubject, which I gueſs would be particularly entertaining while you are on this ſpeculation.

Bartholin ſuppoſes unctuous *effluvia* to have a great ſhare in theſe appearances: his words are theſe, which I chuſe to quote; the book * *De Luce Animalium* being not very common: "Imo quod admirationem "excedit, collectæ oleaginoſi effluvii reliquiæ, longo interjecto tem-"pore, in ſcintillas reſolvuntur: ſi enim faſcias vel tænias ſerico textas, "ſed uſu detritas, leviter excutiamus, igniculi ſuſcitantur ſcintillæ;" —and quotes a paſſage out of *Geſner De Herbis Lucentibus*, to confirm his opinion.

The ſame writer tells us, that *Theodore Beza* was to be ſeen in the dark, "ob fulgorem externum circa oculorum orbes;"—but whether this light proceeded from the ball of the eyes, or hairs of the brows or lids, he does not mention.—Nor does that learned author ſo exact in ſome other circumſtances, in other examples of this ſort, as could be wiſhed. However, I think what he ſays of the Duke of *Mantua* deſerves a remark. —"Quicquid ſit, pro vero habendum eſt quod de *Carolo* "*Gonzaga Mantuæ* duce conſtans fama tulit, levi per totam cutem facta "*frictione* flagrantes ſpecies exire ſolitas."—But here alſo it were to be wiſhed he had let us know whether this great man, of a moſt illuſtrious family, had not ſome particular hairy or ſcaly texture or covering to his ſkin.

By this, I gueſs, you are excited to know how this author, who lived about 100 years paſt, ſolves theſe appearances, of which he had profeſſedly written. Take it in his own words.—

* *Tho. Bartholinus* De Luce Hominum & Brutorum, *lib.* iii. *Hafniæ* 1669, 8°.

"*Ariſtoteles*

“ *Ariſtoteles* (l. i. m. cx.) docebat——quod omnis natura ejus ſit eſ-
“ ſentiæ procreatrix, qualis ipſa eſt—enimvero ſunt ad conſervationem
“ ſpeciei omnis, ejuſdem ſingulæ particulæ, vim ſe diffundendi obtinue-
“ runt, & ſpargendi, per individua multiplicata, ita ne lux primæva &
“ naturalis, ſingulari numinis conſilio, elementorum mixtioni addita,
“ mole minor intercidat, & extinguatur cum ſpeciei non revocando caſu,
“ eo modo conſervari debuit, quo ſerventur omnia, per inſitam naturæ
“ potentiam ſui generativam, *&c.*”

Deſcription and figures of an electrical machine, by Jo. Hen. Winkler, Prof. Leipſ. and F. R. S. N°. 483. p. 497. Mar. &c 1747. Dated Mar. 31. Read May 7 1747. Fig. 13.

26. In a book which I publiſhed laſt year in the *German* tongue, when I was ſpeaking of *Muſſchenbroeck*'s experiment, and deſcribing the increaſe of it in glaſs veſſels, I made mention of a machine which made ſeveral ſparks to appear and crackle, but did not at that time give any figure of it. This *electrical pyrorganon*, as I call it, is repreſented in *fig.* 14. Through the middle of a metalline ring *a b*, filled with pitch, is fixed the little metalline cylinder *c d*. The diameter of the ring is a *Paris* inch and 4 lines. The cylinder appears on both ſides at the diſtance of an inch. The diameter of the cylinder muſt not be leſs, for fear the Electricity communicated to the cylinder ſhould be diminiſhed by touching the metalline ring. To this ring is ſoldered a metalline fork, at which the cochleated ſtyle is let into a wooden cylinder *e f*, the lower extremity of which *f*, is ſo formed, that paſſing through a fiſſure of any plank, it may be faſtened by a ſcrew. Such a metalline cylinder, with a cochleated ſtyle fixed into the pitch with which the ring is filled, I call, for brevity, an *electrical cylinder*. The *electrical pyrorganon* is compoſed of 4 ſuch cylinders. Fig 14. The cylinders are ſo placed, as to have a ſufficient ſpace between them to collect the electrical ſparks. When I would excite them, I place the *pyrorganon* near ſome piece of metal *a b*, ſuſpended on ſilken threads and faſtened, leaving the neceſſary ſpace between the metal and the cylinder *c*. To the laſt cylinder *f g*, I faſten at *g* a wire *h*, reaching to a metalline veſſel *i*, full of water. When theſe things are ſo diſpoſed, as ſoon as the oblong metal *a b* is electrified, the electrical ſparks flaſh out in the 4 ſpaces, and ſhine in proportion, as the glaſs balls communicate more or leſs Electricity, by *rotation* or *friction*

Since the publication of the book above-mentioned, I have conſtructed 2 *electrical pyrorgana*, one of which has the form of a winged wheel, and the other with it's ſparks gives the figure of *Charles's* Wain.

The conſtruction of the winged wheel is as follows: into a hollow orb Fig 16. or wheel of wood *d d d d*, are fixed 6 wooden wings *c d*. The diameter of the whole wheel with it's nave, is 13 inches, and that of the nave 6 inches. The wings *c d*, which are 10 inches long, have fiſſures, in which 3 electrical cylinders may be moved to and fro', and faſtnened. Near the faſtening, at the wings in the orb *d d d d*, are made angular holes, in each of which another electrical cylinder is placed. Thus in the winged wheel appear 6 rows, each conſiſting of 4 electrical cylinders, keeping the diſtances between them which are moſt convenient for exciting the electrical ſparks. In the extremity *c* of each wheel is faſtened a metalline inſtrument *g f*, conſiſting of 3 parts, the extremes of which are joined to the middle *g* in a right angle.

Now that theſe metalline inſtruments may be rightly applied, in the poſterior ſide of the wing at the end *c*, for I call that the anterior, in which the electrical cylinders ſparkle, a metalline button being faſtened, leaves ſome ſpace between itſelf and the poſterior ſide of the wing. The middle of the button is furniſhed with a ſcrew *g*. In the above-mentioned ſpace is inſerted the ſhorter part of the metalline inſtrument. The other extremity *h*, is near the fourth cylinder; but is ſo far diſtant from it, as is neceſſary for the electrical flaſh to be excited between that part of the inſtrument and the cylinder. Hence the other ſhorter part may be moved at will, and faſtened under the button. To the buttons is applied, and wound about the poſterior parts of the wings, a wire *i k*, to which, when the electrical ſparks are to be excited, in the place *x*, another metal *y* is added, which reaches to a metalline veſſel *s*, filled with water. The Electricity, as ſoon as it is communicated to the firſt cylinder, paſſes to all the reſt.

Hence it is neceſſary, in order to excite the ſparks, that the metalline inſtruments *g f* ſhould always remain free from Electricity, which is done by the metal *y*, joined to the wire *i k*, that conducts the Electricity to the water. For between 2 bodies endued equally with Electricity, no ſparks appear. But the Electricity is communicated to the firſt cylinder by the metalline hammer *a*, faſtened to the metalline axis *b c*, which may be turned in the round holes of 2 wooden columns *d e*, by the handle *f*. The apparatus of this hammer is ſhewn in *fig.* 17.

Fig. 17. The columns *d e* reſt upon the piece of wood *m n*, which has a ſtyle fixed into the lid *g*, which covers the glaſs veſſel *h*, faſtened to it with pitch. This glaſs veſſel, therefore, is neceſſary, that the Electricity given to the metalline axis *b c*, may be preſerved by means of ſome wire hung to it.

The bottom of the glaſs veſſel *h* is joined with pitch to the wood, to which a ſtyle is added, which may be inſerted into a longer hole of the column *i k*, ſo as to be faſtened by a ſcrew *l*, after the glaſs veſſel has acquired it's due height, which is when the axis *b c* is in the middle of the nave of the wheel *d d d d*. The electrical cylinders are to be placed in the orb and rings *c d*, in ſuch a manner, that the hammer may have a ſufficient diſtance from the firſt electric cylinder to which it approaches, and the cylinders from each other, to excite the cylindrical ſparks.

Fig. 16. In the ſquare baſe *m n o p*, *fig.* 16. on which ſtands the column *i k*, is a fiſſure, in which, when the hammer *a* appears ſufficiently through the nave of the wheel *d d d d*, the column *i k* is faſtened by a ſcrew. The metalline fork is applied to the wheel *d d d d*, and fixed into the wooden column *q r*, which is faſtened in like manner in the fiſſure of the ſquare baſe *m n o p*. The metalline axis *b c*, protended through the nave of the wheel, is 21 inches high above the ſquare baſe: when therefore, on the axis *b c*, having acquired the Electricity, the hammer approaches to any

firſt electrical cylinder, 5 ſparks appear in a ſtrait row almoſt at the ſame time.

On turning the axis *b c*, thoſe rows of ſparks appear in a circle. The ſparks are ſo bright as to be ſeen by day-light at the diſtance of 100 feet. The man who turns the axis by it's handle, ought to ſtand upon a ſubſtance that does not propagate Electricity, for fear of diſſipating and loſing the Electricity.

Fig. 15. repreſents the electrical 7 ſtars, or *Charles*'s Wain: in the table *a b c d*, which may be elevated or depreſſed in the fiſſure of the column *e f*, 9 electrical cylinders may be ſo placed, that 7 ſparks may appear in the ſame order in which thoſe 7 ſtars appear in a clear night. A wire *b*, which receives the Electricity, is added to the metalline cylinder when theſe ſparks are to be ſhewn. In the extremities of the third and fourth, the bent wire *i* is faſtened, that the Electricity may reach to the fifth and the reſt of the cylinders. To the ninth cylinder is applied a wire *k*, reaching to the water in the metalline veſſel *l*, that the Electricity, being diſtributed through all the cylinders, may be propagated as far as is neceſſary into a ſubſtance which does not preſerve it. Fig. 15.

Theſe obſervations, though extending no farther than to delight the eye, I have ventured to offer to your illuſtrious Society, who have diſcovered a wonderful power of nature to be concealed in ſuch entertainments.

A collection of the electrical experiments communicated to the R. Society *by* Wm Watſon, *F. R. S. read at ſeveral meetings between* Oct. 29. 1747 *and* Jan. 21. *following.* N°. 485. p. 49. Jan. 1747-8.

27. In the paper I did myſelf the honour ſome time ſince to communicate to the *Royal Society*, I took notice, that, among the many other ſurpriſing properties of Electricity, none was more remarkable, than that the electrical power, accumulated in any non-electric matter contained in a glaſs phial, deſcribed upon it's exploſion a circuit through any line of ſubſtances non-electrical in a conſiderable degree; if one end thereof was in contact with the external ſurface of this phial, and the other end upon the exploſion touched either the electrified gun-barrel, to which the phial in charging was uſually connected, or the iron hook always fitted therein. This circuit, where the non-electric ſubſtances, which happen to be between the outſide of the phial and it's hook, conduct Electricity equally well, is always deſcribed in the ſhorteſt manner poſſible; but if they conduct differently, this circuit is always formed through the beſt conductor, how great ſoever it's length is, rather than through one which conducts not ſo well, though of much leſs extent.

It has been found, that in proportion as bodies are ſuſceptible of having Electricity excited in them by friction, in that proportion they are leſs fit to conduct it to other bodies; in conſequence whereof, of all the ſubſtances we are acquainted with, metals conduct beſt the electrical powers; for which reaſon the circuit before ſpoken of is formed through them the moſt readily. Water likewiſe is an admirable conductor; for the electrical power makes no difference between ſolids and fluids as ſuch, but only as they are non-electric matter.

In order to give an idea of what is underſtood by this circuit, we will mention an example or two, from which all the other may naturally be deduced.

deduced. If a perſon ſtands upon a dry wooden floor with a coated phial ever ſo highly charged in one of his hands, and if another perſon, without touching the firſt, ſtands but ſix inches from him, and touches the iron hook of the phial, neither of them are ſhocked; becauſe the floor between them, tho' the diſtance is ſo ſhort, will not conduct the Electricity ſufficiently quick. But if theſe two perſons tread upon a piece of wire laid between them, they each of them feel the electrical commotion in that arm, which touches the phial and hook, and in that foot which treads upon the wire; the wire here conducting the Electricity quick enough, which the dry floor would not. The circuit is here formed by the coated phial, it's hook, ſo much of the bodies of theſe two perſons as formed a curve line between the wire, the phial, and hook, and the wire between theſe perſons. If theſe perſons ſtand upon, or touch with any part of their bodies any non-electrics, which readily conduct Electricity, the circuit is completed, and the effect is the ſame: and this is occaſioned by the ſhort ſpace of time, in which the loaded phial is diſcharged, when any matter of what kind ſoever readily conducting Electricity happens to be between the coated phial and it's hook, and is ſo connected as to communicate with both upon the diſcharge of the phial.

M. *le Monnier* the younger at *Paris*, in an account tranſmitted to the *Royal Society*, takes notice of his feeling the ſtroke of the electrified phial along the water of two of the baſons of the *Thuilleries* (the ſurface of one of which is about an acre) by means of an iron chain which lay upon the ground, and was ſtretched round half their circumference.

Upon theſe conſiderations it was conjectured, as no circuit had as yet been found large enough ſo to diſſipate the electrical power as not to make it perceptible, that if the non-electrical conductors were properly diſpoſed, an obſerver might be made ſenſible of the electrical commotion quite acroſs the river *Thames*, by the communication of no other medium than the water of that river. But as perhaps, in what relates to Electricity leſs than in any other part of natural Philoſophy, we ſhould draw concluſions but from the facts themſelves, it was determined to make the experiment.

The making this experiment drew on many others, and as the gentlemen concerned flatter themſelves that they were made with ſome degree of attention and accuracy, they thought it not improper to lay a detail of all the operations relating thereto, before the *Royal Society*.

In order to try whether or no the electrical commotion would be perceptible acroſs the *Thames*, it was abſolutely neceſſary that a line of non-electric matter, equal in length to the breadth of the river ſhould be laid over it ſo as to touch the water thereof in no part of it's length; and the bridge at *Weſtminſter* was thought the moſt proper for that purpoſe, where the water from ſhore to ſhore was ſomewhat more than 400 yards.

Accordingly on *Tueſday July* 14, 1747. to ſee the ſucceſs and aſſiſt in making the experiment, there met *M. Folkes*, Eſq; *Pr. R. S.* the R. Hon. the E. *Stanhope*, *Rich. Graham*, Eſq; *Nich. Mann*, Eſq; and myſelf,

myſelf, with proper perſons to execute what was required of them in the various parts of theſe experiments.

A line of wire was laid along the bridge, not only through it's whole length, but likewiſe turning at the abutments, reached down the ſtone ſteps on each ſide of the river low enough for an obſerver to dip into the water an iron rod held in his hand. One of the company then ſtood upon the ſteps of the *Weſtminſter* ſhore holding this wire in his left hand, and an iron rod touching the water in his right: on the ſteps facing the former upon the *Surry* ſhore, another of the company took hold of the wire with his right hand, and graſped with his left a large phial almoſt filled with filings of iron coated with ſheet-lead, and highly electrified by a glaſs globe properly diſpoſed in a neighbouring houſe. A third obſerver ſtanding near the ſecond dipped an iron rod held in his left hand into the water, and touching the iron hook of the charged phial with a finger of his right hand, the Electricity ſnapped, and it's commotion was felt by all the three obſervers, but much more by thoſe upon the *Surry* ſhore. The third obſerver here was no otherwiſe neceſſary, than that the river being full, the iron was not long enough to be fixed in the mud upon the ſhore, and therefore was in want of ſome ſupport. The experiment was repeated ſeveral times, and the electrical commotion felt acroſs the river; but the gentlemen preſent being much moleſted in their operations by a great concourſe of people, who many times broke the conducting wire, and otherwiſe greatly incommoded them, and the evening growing too dark for the obſervers on different ſides of the water to ſee each other, they were prevented from diverſifying the experiments, as was intended, and only conſidered theſe trials as a ſtill further encouragement for them to proſecute the inquiry at a more favourable opportunity.

Early therefore on *Saturday* morning *July* 18, there met upon *Weſtminſter-Bridge* the *Preſ.* the R. Hon. the Lord *Charles Cavendiſh*, *Rich. Graham*, Eſq; Dr *Bevis*, and myſelf, with proper aſſiſtants, At the preceding meeting, the electrical machine's being placed at ſome diſtance from the water being found inconvenient, the following alteration was made in the diſpoſition of the *apparatus*.

A room up two pair of ſtairs in a commodious houſe neareſt the bridge on the *Surry* ſhore was provided, in which was placed the electrical machine with the gun-barrel ſuſpended in ſilk lines. From this room on account of it's height, the ſignals on both ſides of the river were eaſily obſervable. The coated phial before-mentioned with it's iron hook was placed upon the ſeat of the window of this room, and communicated with the gun-barrel by the means of a piece of iron wire. One extremity of another wire was likewiſe fixed into the bottom of the leaden coating of the phial, whoſe other extremity reached therefrom over the bridge to the ſteps upon the *Weſtminſter* ſhore, the body of the wire being placed as much as poſſible upon the parapet of the bridge. One or more obſervers took each other by the hand, the firſt of which muſt neceſſarily

ceſſarily take the wire in his left hand, and the laſt, upon the proper ſignal given, either dip his right hand into the water, or (which makes the poſture more agreeable) a rod of metal held therein. Another wire having no communication with any of the former, was let down from the before-mentioned room, and down the ſteps upon the *Surry* ſhore: one extremity of this wire was held in the hand of an obſerver ſtanding upon theſe ſteps, who dipped an iron rod held in his other hand into the water: to the other extremity of this wire was faſtened a ſhort iron rod, with which, when the electrified phial was ſufficiently charged, and the ſignal given, the gun-barrel was to be touched.

The gentlemen, by this diſpoſition of the *apparatus*, propoſed to examine principally theſe 3 queſtions: firſt, whether or no the obſervers ſtanding on each ſide of the river would perceive the electrical commotion, each putting an iron rod into the water? Secondly, whether or no the obſervers on both ſides of the river would feel the electrical commotion, when the obſerver ſtanding upon the *Weſtminſter* ſhore removed the iron rod held in his hand out of the water? Thirdly, whether or no the electrical power was perceptible to the obſervers on both ſides of the river, if the obſerver upon the *Weſtminſter* ſhore dipped his hand into a pail of water, which had no communication with the water of the *Thames*.

It was determined firſt, upon proper ſignals, to diſcharge the electrified phial in the manner before-mentioned, the obſervers on each ſide of the river holding the iron rods in the water, and this experiment was to be repeated 3 times. This was attempted accordingly; and although the obſerver on the *Surry* ſhore was each time ſmartly ſtruck, the *Preſident*, who obſerved with the utmoſt attention upon the *Weſtminſter* ſhore, gave the ſignal that he felt nothing. The company was ſurpriſed at this want of ſucceſs in the experiment; but, upon examining the wire, which was laid over the bridge, it was found to have been broken by ſome accident, after it had paſſed over about $\frac{1}{4}$ part of the bridge. The wire being refitted, it was agreed to make the ſame experiment ſix times more: this was done accordingly, and the electrical commotion was felt each time by the obſervers on both ſides of the water, but much ſmarter by thoſe on the *Surry* ſide. It was then thought proper to repeat this experiment 3 times more upon the ſignal's being given: but, in making the firſt of theſe, the obſerver in the room with the machine, diſcharged the electrified phial, before the obſerver upon the *Surry* ſhore had dipped his iron rod into the water, and therefore no effect was perceived by the obſerver on the oppoſite ſhore. The electrified phial therefore was again diſcharged 3 other times, and the commotion felt by the obſervers on both ſides of the river.

To examine the ſecond queſtion, no other alteration was neceſſary in the whole *apparatus*, than that the obſerver upon the *Weſtminſter* ſhore ſhould not dip either his hand, or the iron rod held therein in the laſt experiments, into the water of the river. The electrified phial then was

diſcharged

discharged 3 times without it's effects being in the least perceived by the observers upon the *Westminster* shore; those indeed on that of *Surry* felt the shock as before.

In examining the third question, the *apparatus* was in all other respects the same as in the last; except that the observer upon the *Westminster* shore had a pail of water placed upon a wooden table, which stood upon the stone steps, and into which he was to put his right hand upon the signal's being given. This was accordingly done, and the electrified phial being discharged 3 times, the electrical commotion was felt as before by the observer upon the *Surry* shore; but not in the least by him on the *Westminster* side, who held his hand in the pail of water.

In all these experiments, except in one before-mentioned, where the iron rod was not in the water, it was found, that whether the observers on the *Westminster* shore, upon the discharge of the electrified phial, did or did not feel it's effects, they were always perceived not only in the arms of those upon the *Surry* shore, who formed a line between the extremity of the wire there, and the water of the river; but by any other person, who standing upon the stone steps, even where they were not wet, touched the wire with his hand. They were likewise felt by a person upon the *Westminster* shore, standing upon the wet stone steps, who did not form part of the line between the extremity of the conducting wire and the water, otherwise than by touching the wire with his fingers.

As was before-mentioned, the observers upon the *Westminster* shore did not feel the effects of the discharged phial near so strong as those on that of *Surry* in the first sett of these experiments. When a line was there formed by the joining hands of two or more persons, the first of which, on account of the situation, held the conducting wire in his left hand, and the last touched the water with an iron rod held in his right, the effects were most sensible in the left arm of him who held the wire: they were indeed manifestly felt by them all; but this feeling was not great enough to be called a shock, but, as was very properly expressed by one of the company, it resembled the pulsation of a large artery.

From the examination of the first and second questions it appeared, that the observers upon the *Westminster* shore were not sensible of the effects of the Electricity, unless their bodies described part of the circuit before spoken of; and this circuit here consisted of part of the gun-barrel of the electrifying machine, the wire going from this gun-barrel to the iron hook, the phial itself, the tail wire of this coated phial which reached therefrom across the bridge and down the steps on the *Westminster* shore, the line of observers between this wire and the iron rod which dipped in the water there, this iron rod, a supposed line of water drawn quite across the *Thames*, the observers with their iron rod on the *Surry* shore, the iron wire going from the right hand of the last of these up into the room where the electrifying machine was placed, and the short iron rod to which one extremity of this wire was joined, and with which, in in making the explosion, the gun-barrel was touched. The length of this circuit, through which the Electricity was propagated was at least 800 yards, more than 400 yards of which was formed by the stream of the river.

From the examinntion of the third question it appeared, that the electrical commotion would not be felt from the observer dipping his hand in water only, unless that water was so disposed as to become part of the circuit; and this experiment was made, lest the contrary might be surmised.

The observers upon the *Westminster* shore not feeling the electrical commotion equally strong with those of *Surry*, was judged to proceed from other causes besides that of distance. For it must be considered, that the conducting wire was almost thoughout it's whole length laid upon *Portland* stone standing in water. This stone, being in a great degree non-electric, is of itself a conductor of Electricity: and this stone standing in water, no more of the Electricity was transmitted to the observers on the *Westminster* shore than that proportion, wherein iron is more non-electric, and, consequently, a better conductor of Electricity than stone. This was made more manifest, from observing, that whether the conducting wire upon the bridge was broke or no, and, consequently, whether the observers upon the *Westminster* shore felt the electrical commotion or no, not only the observers upon the *Surry* shore, who with their wire formed part of the line, felt the shock in their arms; but those persons who only stood upon the stone steps there, and touched the wire with their fingers, felt the electrical commotion in the arm of that hand which touched the wire, and down their legs. From whence, and from the person before spoken of feeling the electrical commotion standing upon the wet stone steps of the *Westminster* shore, though not forming part of the line, but only touching the wire with his fingers, it was concluded, that, besides the large circuit before spoken of, there were formed several other subordinate circuits between the same steps of the *Surry* shore, and the bridge by means of the water; whereby that part of the electrical power, felt by the observers upon the *Surry* side of the river, and not by those on the *Westminster* side, was discharged.

Dr *Bevis* having observed, and which was likewise tried here, that however well an electrified phial was charged, it's iron hook would not fire the vapours of warm spirit of wine held in a spoon and applied thereto, if the person who held the phial, and he who held the spoon did not take each other by the hand, or have some other non-electrical communication between them, it was therefore thought proper to try the effects of Electricity upon some warm spirit of wine through the large circuit before-mentioned. Accordingly the observers being placed as before both upon the *Westminster* and *Surry* shores, no other alteration was made in the before-mentioned *apparatus*, than that the wire which connected the gun-barrel with the iron hook of the coated phial being laid aside, the coated phial itself was charged at the gun-barrel, and then brought in the hands of an obſerver near the warm ſpirits in the ſpoon, which was placed upon the ſhort iron rod before-mentioned, which was connected with the wire which went to the obſervers upon the *Surry* ſhore. Upon preſenting properly the iron hook of the charged phial to the warm ſpirit, it was inſtantly fired, and the electrical commotion felt by the obſervers on both ſides of the river.

It was then thought proper to try the effects of the charged phial upon the warm ſpirit, when the wire was divided which was laid over the bridge: upon preſenting the iron hook to the ſpirit, a ſufficient ſnap was given to the ſpoon to fire the ſpirit, but nothing ſo ſmart as in the former experiment, where the large circuit was completed.

It was then tried, what the effect would be upon the ſpirit, if the charged phial was diveſted of it's long wire which lay over the bridge, and was only held in the hand of an obſerver; whilſt the ſpoon with warm ſpirit was placed in contact of the iron rod before-mentioned, to which the wire was connected, which went to the obſervers upon the *Surry* ſhore; and the ſpirit was fired with much the ſame degree of ſmartneſs as in the laſt experiment.

In theſe and all the ſubſequent operations, wires were made uſe of to conduct the Electricity preferable to chains, as it before by great numbers of experiments had been fully proved, that whatever difference there was in the bulk of the conductor, that is to ſay, whether it were a ſmall wire, or a thick iron bar, the electrical ſtrokes communicated thereby were equally ſtrong: and it had been further obſerved, beſides the difficulty of procuring chains of a requiſite length for the preſent purpoſes, that the ſtroke at the gun-barrel, when the Electricity was conducted by a chain, was *cæteris paribus*, not ſo ſtrong, as when that power was conducted by a wire. This was occaſioned by the junctures of the links of the chain not being ſufficiently cloſe, which cauſed the Electricity in it's paſſage to ſnap and flaſh at the junctures, where there was the leaſt ſeparation; and theſe leſſer ſnappings in the whole length of the chain leſſened the great one of the gun-barrel.

Encouraged by the ſucceſs of theſe trials, the gentlemen were deſirous of continuing their enquiries, and of knowing whether or no the electrical commotions were perceptible at a ſtill greater diſtance. The *New River* near *Stoke-Newington* was thought moſt convenient for that purpoſe; as at the bottom of that town, the twinings of the river are ſo circumſtanced, that from a place which we will call *A* to another *B*, the diſtance by land is about 800 feet, but the courſe of the river is near 2000. From *A* to another place, which we will call *C*, in a right line is 2800 feet, but the courſe of the water is near 8000 feet.

Accordingly, on *Friday July* 24, 1747, there met at *Stoke-Newington* the *Preſ.* of the *R. Soc.* the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, *James Burrow*, Eſq; *Peter Daval*, Eſq; Mr. *George Graham*, *Wm Jones*, Eſq; *James Lever*, Eſq; Mr *Newcombe*, *Charles Stanhope*, Eſq; Mr *Trembley*, and myſelf, who were of the *Royal Society*, and Dr

Bevis. To this gentleman the company were much obliged, not only for his great readineſs in aſſiſting in all the operations, but likewiſe for the uſe of his electrifying machine, which from it's ſize was conveniently portable. This machine was now placed in a room up one pair of ſtairs in a houſe near *A*, and the ſignals from thence might eaſily be perceived by the obſervers both at *B* and *C*.

It was propoſed, firſt to try the electrical commotion by the ſame obſervers as at *Weſtminſter-bridge*, from *A* to *B*, the diſtance as before-mentioned being about 800 feet by land, and 2000 by water, in order, if poſſible, to determine the difference of the ſtrength of the Electricity felt there, and at the ſtone bridge at *Weſtminſter*; the difference of the length of the 2 circuits being about 400 feet in favour of that of the *New River*.

To make the experiment, an iron wire was faſtened to the coating of the glaſs phial before-mentioned, and conducted from one of the windows of the room over the *New River* without touching the water; and from thence to *B*, laying in it's whole length upon the graſs in the meadows, except where it paſſed over a hedge. At *B*, when the exploſion was to be made, one or more obſervers were to take the extremity of this wire in one hand, and touch the water of the river as before with an iron rod held in the other. Another wire was let down from the other window of the room; one extremity of which was joined to the ſhort iron rod mentioned in the former experiments, the other was held in the hand of an obſerver at *A*, whoſe other hand held an iron rod dipped into the river.

It was abſolutely neceſſary that theſe wires ſhould touch each other in no part of their length, otherwiſe the before-mentioned circuit would upon the exploſion be completed from their firſt contact.

When every thing was thus diſpoſed, and the ſignals given, the charged phial was exploded 8 times, and the electrical commotion every time ſmartly felt by the obſervers both at *A* and *B*. Whether the line of obſervers at *B* conſiſted of one or more, they were always ſtruck, and that more ſharply than at *Weſtminſter-bridge*, under the ſame circumſtances. One of the obſervers, taking the wire in his hand, without having any communication either with any of the other gentlemen or the water of the river, felt the ſhock in his feet.

It was then thought proper to make right exploſions without any other alteration in the *apparatus* than that the obſervers at *B*, ſhould ſtand in the meadow at ſome diſtance from the water, without having any communication therewith other than that furniſhed by the ground. This was accordingly done, and the ſtroke felt little if at all leſs than thoſe laſt mentioned. But the electrical ſtrokes being felt ſmartly at the diſtance of at leaſt 20 feet from the water, occaſioned a very perplexing difficulty, as it was impoſſible by this experiment to determine with any certainty, whether or no the electrical circuit was formed throughout the windings of the river, or much ſhorter by the ground of the meadows. The experiment plainly ſhewed, that the meadow-ground with the graſs thereon conducted

conducted the Electricity better than ſtone; as it muſt be remembered, that the obſervers upon the ſtone ſteps upon the *Weſtminſter* ſhore felt not in the leaſt degree the electrical commotion, when their iron rod was not in the water, and themſelves ſtood upon the dry ſtone ſteps. But this effect was ſuppoſed to be owing to the meadow-ground here being encompaſſed on two ſides by the *New River*, and on the other by a wet ditch, by both which it was generally well moiſtened. To ſolve therefore this difficulty, a ſeries of experiments were executed, of which hereafter.

The gentlemen then determined to examine whether the electrical commotions were perceptible from *A* to *C*; a diſtance not leſs than 2800 feet by land, and near 8000 by water.

To execute this, to the former wire, which was already conducted to *B*, another was added, which there croſſed the river without touching the water; and reached almoſt to *C*, where the firſt of a line of gentlemen held, as before, the wire in one hand, and the laſt dipped the iron into the water. The wire from the machine to *A* was as before. Upon the ſignal's being given, the charged phial was exploded 10 times, and it's effects plainly though but faintly perceived each time by ſome or other of the obſervers, but never by them all. The electrical commotion was always felt by that obſerver who held the extremity of the wire, but never by him who held the iron rod in the water. It was in one experiment felt by the obſerver who held the wire, not felt by the next, who held the hand of the former, and yet plainly perceived by the third, who joined the ſecond. Thoſe who did not themſelves feel the electrical commotion here, did as at *B* ſee the involuntry motions of thoſe who did. The obſervers at *A* felt the ſhocks in the ſame degree, whether the other obſervers were ſtationed at *B* or *C*.

This experiment further demonſtrates the diſtance to which the electrical power may be conveyed: but the ſame difficulty occurs here as in the laſt; to wit, whether the circuit was completed by the ground, or by the water of the river?

Theſe ſame operations, which ſhewed at how great a diſtance the electrical commotion was perceptible, ſolved likewiſe 3 queſtions of a ſubordinate nature.

Firſt, Whether or no, *cæteris paribus*, any difference occurred in the ſucceſs of the experiment, if the long wire, inſtead of being joined to the coating of the phial, was faſtened to the ſhort iron rod, which, upon touching the gun-barrel, occaſioned the exploſion; and if the ſhort wire, which only went to the obſerver at *A*, a diſtance from the machine not more than 30 feet, was joined to the coating of the phial? Upon trial no difference * was found.

* No difference is obſerved when the electrical circuit is propagated through ſubſtances which readily conduct Electricity; if they conduct it in a leſs degree, the electrical commotion is moſt perceptible to the obſerver, who holds the wire, which comes from the charged phial.

Secondly, Whether or no, *cæteris paribus*, any difference in the electrical commotion would be perceived, when that power paſſes through the arms of two obſervers, whoſe bodies made part of the circuit, ſtanding in the room near the electrifying machine; one of which takes the extremity of the wire that goes to the obſerver at *A* in one hand, and touches the gun-barrel with the ſhort iron rod held in his other hand? The other obſerver takes the extremity of the wire which goes to *B* or *C* in one hand, and touches the coating of the charged phial with his other. In ſeveral trials, where each of theſe obſervers frequently changed ſtations, no difference in point of ſtrength was obſerved in the electrical commotion.

Thirdly, Whether or no theſe two obſervers laſt-mentioned received the ſhock at the ſame time? They were ſeen to be both convulſed in the ſame inſtant.

July 28. 1747, there met again at the ſame place, to proceed further in theſe enquiries, the *Preſident*, the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, Sir *Francis Daſhwood*, Baronet, *Peter Daval*, Eſq; Mr *Ellicott*, Mr *George Graham*, *Richard Graham*, Eſq; Mr *Robins*, Mr *Short*, Dr *Wilbraham*, and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

The electrical commotion was firſt tried from *A* to *B* before-mentioned, the iron wire in it's whole length being ſupported, without any where touching the ground, by dry ſticks placed at proper intervals of about 3 feet in height. The obſervers both at *A* and *B* ſtood upon originally-electrics, and, upon the ſignal, dipped their iron rods into the water. Upon diſcharging the phial, which was ſeveral times done, they were both very much ſhocked, much more ſo than when the conducting wires lay upon the ground, and the obſervers ſtood thereon, as in the former experiments. The ſame experiment was tried with the obſerver at *A*, inſtead of the iron rod, dipping a narrow ſlab of *Portland* ſtone into the water about 3 ½ feet in length; when the ſhock was felt, but not ſo ſevere as through the iron rod. This demonſtrated, as was before ſuggeſted, why the electrical commotion was not felt ſtronger by the obſervers upon the weſtern ſhore of the *Weſtminſter-bridge*; *viz.* that *Portland* ſtone ſtanding in water will conduct Electricity very conſiderably.

The gentlemen then tried what would be the effect, if the obſerver at *B* ſtood upon a cake of wax, holding the wire as before, and touched the ground of the meadow with his iron rod at leaſt 150 feet from the water; and if the obſerver uſually placed near the river at *A*, had his wire carried 150 feet over the river, as the former, ſtood upon an originally-electric, and touched the ground with his iron rod. Upon the exploſion of the charged phial, which was ſeveral times done, both the obſervers were ſmartly ſtruck: this demonſtrated, that in theſe inſtances the moiſt ground of the meadows made part of the circuit. The obſervers were diſtant from each other about 500 feet.

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The obſervers then, ſtationed as in the laſt experiment, ſtood upon the wax cakes as before, without touching the ground with the iron rods, or any part of their bodies, and the charged phial was exploded 4 times. Theſe were not at all felt by the obſerver next to *B*, and without the greateſt attention, would not have been perceived by him next to *A*; and then only in ſome of the trials, the feeling of the Electricity was like that of a ſmall pulſe between the finger and thumb of that hand which held the wire. The loaded phial was again diſcharged 4 times more, without any other alteration in the diſpoſition of the *apparatus*, than that the obſerver next to *B* ſtood upon the ground; when the electrical commotion was perceived by that obſerver, though not ſo ſharp as when the other obſerver at the ſame time ſtood upon the ground. The obſerver next to *A* felt the tingling between his finger and thumb, as before.

The gentlemen were deſirous of trying the electrical commotion at a ſtill greater diſtance than any of the former, through the water, and where, at the ſame time by altering the diſpoſition of the *apparatus*, it might be tried, whether or no that power would be perceptible through the dry ground *only* at a conſiderable diſtance. *Highbury-barn* beyond *Iſlington* was thought a convenient place for this purpoſe, as it was ſituated upon a hill nearly in a line, and almoſt equidiſtant from 2 ſtations upon the *New River*, ſomewhat more than a mile aſunder by land, though following the courſe of that river, their diſtance from each other was 2 miles. The hill between theſe ſtations was of a gravelly ſoil; which, from the late continuance of hot weather without rain, was dry, full of cracks, and conſequently was as proper to determine whether or no the Electricity would be conducted by dry ground to any great diſtance, as could be deſired. This hitherto had not been attempted; the meadows in the inſtances before quoted conducting the Electricity, was ſuppoſed to be owing to the moiſture of the ground. The ſtreets of *London*, when very dry, had been found to conduct it ſtrongly about 40 yards, and the dry road at *Newington* about the ſame diſtance. Accordingly, on *Wedneſday*, *Aug.* 5, 1747, there met at *Highbury-barn* the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, Mr *George Graham*, *Rich. Graham*, Eſq; *N. Mann*, Eſq; Mr *Short*, *Daniel Wray*, Eſq; and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

The electrifying machine being placed up one pair of the ſtairs in the houſe at *Highbury-barn*, a wire from the coated phial was conducted upon dry ſticks as before, to that ſtation by the ſide of the *New River*, which was to the northward of the houſe. The length of this wire was 3 furlongs and 6 chains, or 2376 feet. Another wire faſtened to the iron bar, with which, in making the exploſion, the gun-barrel was touched, was conducted in like manner to the ſtation upon the *New River* to the ſouthward of the houſe. The length of this wire was 4 furlongs 5 chains and 2 poles, or 3003 feet. The length of both wires, excluſive of their turnings round the ſticks, was 1 mile, 1 chain, and 2 poles, or 5379 feet.

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For the more conveniently deſcribing the experiments made here, we will call the ſtation to the northward *D*, and the other *E*.

At this diſtance the gentlemen propoſed to try, firſt, whether or no the electrical commotion was perceptible, if both the obſervers at *D* and *E*, ſupported by originally-electrics, touched the conducting wire with one hand, and the water of the *New River* with an iron rod held in the other? Secondly, whether or no that commotion was perceptible, if the obſerver at *E*, being in all reſpects as before, the obſerver at *D*, ſtanding upon wax, took his rod out of the water? Thirdly, whether or no that commotion was perceptible to both obſervers, if the obſerver at *D* was placed upon wax, and touched the ground with his iron rod in a dry gravelly field at leaſt 300 yards from the water?

As from the ſituation of the ground, trees, *&c.* neither of the ſtations could be ſeen by each other, or by the obſerver at the electrifying machine, it was agreed to diſcharge a gun as a ſignal to get ready, and to do the ſame, as near as might be, half a minute before each exploſion.

In theſe experiments, as well as the former, the coated phial was each time charged as high as it could be; ſo that if the dfference of the ſhock to the obſervers was conſiderable, it was owing to other cauſes more than to the phial's being differently electrified.

To try the firſt propoſition, 8 exploſions were made with the obſervers at *D* and *E*, touching the water, and ſtanding upon wax, with their iron rods in the water. The firſt 2 of theſe were felt but weakly by the obſerver at *D*; but in the other 6 he was ſtrongly ſhocked. The obſerver at *E* felt nothing of the firſt 6 exploſions; when, upon examination, the wire was found broken by ſome accident; but this obſerver was ſtrongly ſhocked by the 2 laſt. The obſerver at *D* being ſhocked in 4 of theſe exploſions, while in theſe 4 the obſerver at *E* felt nothing, was owing to the circuits being formed by the ground between the obſerver at *D* and the broken wire. Upon account of the wire's being broken, the gentlemen tried 3 more exploſions, when the obſervers at both ſtations felt the electrical ſhock.

To try the ſecond propoſition, 4 exploſions were made with the obſerver at *D*, ſtanding upon an originally-electric, and taking his iron rod out of the water, the obſerver at *E* as before. In each of theſe the obſerver at *D* felt a ſmall pulſation between his finger and thumb of that hand which held the wire. The obſerver at *E* felt each of theſe as ſtrong as before. This being different from the obſervations made in the experiments of the laſt trials at our former ſtations *A* and *B*, and many others; where *B* in the ſame circumſtances with *E* here felt the electrical commotion only in a ſlight degree, was owing, as we were afterwards informed, to the impertinent curioſity of the ſervants of the gentlemen, and other voluntary obſervers, who, by touching the wire which went from the coated phial to the obſerver at *D*, felt the ſhock in their arms and ankles, and formed ſubordinate circuits to *E*. The preventing theſe people from touching the wires, was impoſſible; as great part of them could be ſeen neither

neither by the obſervers at the ſtations, nor by thoſe at the houſe, and their being more than a mile long.

The 4 other exploſions were made without any other alteration in the *apparatus*, than than that the obſerver at *D* ſtood upon the ground about 4 yards from the water without any communication therewith. The obſerver at *E* felt the ſhocks in his arms as before; but the obſerver at *D* ſtanding upon the ground was ſhocked in the elbow and wriſt of that arm which held the wire, and in both his ankles.

To try the third propoſition, 8 exploſions were made with the obſerver at *D* ſtanding upon an originally-electric with his rod in the water of the river as before; but the obſerver at *E* was placed in a dry gravelly field about 300 yards nearer the machine than his laſt ſtation, and about 100 yards diſtant from the river. He there ſtood upon the wax, holding the conducting wire in one hand, and touched the ground with an iron rod held in the other. The ſhock was each time felt by the obſerver at *D*, but ſenſibly weaker than in the former trials; but the obſerver at *E* felt them all equally ſtrong with the former; the 4 firſt in his arms, when he ſtood upon the wax, and touched the ground with his iron rod; the other 4 in his arm and ankles, when he ſtood upon the ground without the iron rod.

In ſome of theſe experiments, the obſervers at *D* felt a tingling as ſoon as they laid hold of the conducting wire. This was conjectured to be owing to the Electricity, which conſtantly runs off while the coated phial is filling, and preferably by the wire, as the beſt conductor.

From the ſeverity of the ſhock, the gentlemen, in ſome of theſe trials, did not chooſe to have the Electricity paſs through their bodies: but, as it was neceſſary for them to be ſenſible of the different degrees of the electrical commotions, they bound the conducting wire round one of their thumbs, and touched the iron rod with the fore-finger of the ſame hand; when the electrical commotion was felt only in ſo much of the finger and thumb of that hand, as completed the circuit.

By the experiments of this day, the gentlemen were ſatisfied, that the dry gravelly ground conducted the Electricity as ſtrongly as water; which though otherwiſe at firſt conjectured, they now found not to be neceſſary to convey that power to great diſtances; as well as that, from difference of diſtance only, the force of the electrical commotion was very little if at all impaired. They were convinced of the truth of the firſt of theſe facts, not only from both obſervers feeling the electrical commotion in the 8 laſt experiments, when the obſerver at *E* was at ſuch a diſtance from the water, but alſo from the obſerver at *D* feeling the ſhock ſo ſtrong in 4 of the firſt 6 exploſions, when the conducting wire to *E* being broke at about 100 yards diſtance from the houſe, that obſerver felt nothing.

In this laſt inſtance the circuit was formed from the phial by the obſerver at *D* and his wire, a line of ground which reached from the ſtation at *D* to the broken wire that lay upon the ground, and ſo much of this wire as reached to the ſhort iron rod, which touched the gun-barrel in making

making the explosions. This induced the gentlemen to conclude (as from many experiments it was manifest, that when the intervening substances conduct Electricity equally well, the circuit was performed in the shortest manner possible), that when the observers holding their iron rods in the river at *D* and *E* were both shocked, the Electricity was not conveyed by the water of the river, being two miles in length, but by land, where the distance was only one mile; in which space that power must necessarily pass over the *New River* twice, through several gravel-pits, and a large stubble-field. So that, admitting the Electricity did not follow the track of the river, the circuit from *D* to *E* was at least 2 miles; *viz.* somewhat more than one mile of wire, which conducted the Electricity from the house to the stations, and another mile of ground, the shortest distance between those stations. The same inference was now drawn with regard to the experiments at *A*, *B*, and *C*, in the *New River* before recited; *viz.* that as in all of them the distance between the observers was much greater by water than by land, the Electricity passed by land from one observer to the other, and not by water.

From the shocks which the gentlemen received in their bodies, when the electrical power was conducted upon dry sticks, they were of opinion, that from difference of distance simply considered, as far as they had yet experienced, the force thereof was very little if at all impaired. When they stood upon originally-electrics, and touched the water or ground with an iron rod, the electrical commotion was always felt in their arms and wrists: when they stood upon the ground, and touched either the water or ground with their iron rods, they felt the shock in their elbows, wrists, and ankles: when they stood upon the ground without the rod, the shock was always in the elbow and wrist of that hand, which held the conducting wire, and in both ankles. The observers here being sensible of the electrical commotion in different parts of their bodies, was owing in the first instance to the whole of it's passing (because the observer stood upon wax) through their arms, and through the iron rod: in the second, when they stood upon the ground, the Electricity passed both through their legs, and through the iron: in the third, when they stood upon the ground without either wax or rod, the Electricity directed it's way through one arm, and through both legs to complete the circuit.

The gentlemen were desirous of closing the present inquiry, by examining not only whether or no the electrical commotions were perceptible at double the distance of the last experiments in ground perfectly dry, and where no water was near; but also, if possible, to distinguish the respective velocities of Electricity and sound. To execute this required the whole sagacity and address of the gentlemen concerned; for they had met with very great difficulties in the last day's operations, where the wire was conducted but little more than a mile; all which could not

but

but be greatly augmented by doubling that diſtance; becauſe it was neceſſary, that the houſe, wherein the electrifying machine was placed, ſhould be viſible at leaſt at one of the ſtations; and that the ſpace between that houſe and the ſtations, through which the wire was conducted, ſhould be very little interſected by hedges, roads, or foot-paths; neither ſhould the wire in this ſpace be ſubject to be diſturbed by the horſes or cattle, which were grazing; nor ought it to touch in it's paſſage the trees or any other vegetables, which at this ſeaſon of the year were every-where luxuriant. To find a place within a convenient diſtance of *London* with theſe requiſites was not very eaſy; but at laſt, *Shooters-Hill* was pitched upon, as the moſt convenient.

As only one ſhower of rain had fallen during the preceding 5 weeks, the ground could not but be very dry; and as no water was near, if the electrical commotion was felt by the obſervers at the ſtations, it might be ſafely concluded, that water had no ſhare in conducting it.

Aug. 14. 1747. there met at *Shooters-Hill* for this purpoſe, the Rev. Mr *Birch*, the Rev. Mr Profeſſor *Bradley*, *Peter Daval*, Eſq; Mr *G. Graham*, *R. Graham*, Eſq; Mr *Nourſe*, *George Lewis Scott*, Eſq; Mr *Short*, *Charles Stanhope*, Eſq; and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

It was here determined (as the gentlemen were ſatisfied from many of the former trials, that if, when the coated phial was diſcharged, the obſervers at the ſtations ſtood upon originally-electrics, and touched neither water nor ground with iron rods, or any part of their bodies, the electrical commotion would be ſcarcely perceptable) to make twelve exploſions of the coated phial, with an obſerver placed at the 7 mile ſtone, and another at the 9 mile ſtone, both ſtanding upon wax, and touching the ground with an iron rod. This number of exploſions was thought more neceſſary, as the obſervers at theſe ſtations were not only to examine whether or no the Electricity would be propagated to ſo great a diſtance; but if it were, the obſerver at the 7 mile-ſtone was by a ſecond watch to take notice of the time lapſed between feeling the electrical commotion, and hearing the report of a gun fired near the machine, as cloſe as might be to the inſtant of making the exploſion: and therefore, to examine this matter with the requiſite exactneſs, this number of exploſions ſhould be made.

To execute this, the electrifying machine was placed up one pair of ſtairs in a houſe upon the weſt ſide of *Shooters-Hill*; and a wire from the ſhort iron rod, with which the gun-barrel was touched in making the exploſions was conducted upon dry ſticks as before into a field near the ſeven mile-ſtone. The length of this wire, excluſive of it's turnings round the ſticks, was a mile, a quarter, and 8 poles, or 6732 feet. In great part of this ſpace it was found very difficult to ſupport the wire, on account of our ſcarcely being able to fix the ſticks in the ſtrong gravel there almoſt without any cover of ſoil; nor could the wire in ſome places be prevented from touching the brambles and bushes, nor in one field the ripe barley.

Another wire was likewise conducted upon sticks from the coated phial to the nine mile-stone. In this space, the soil being a strong clay, the wire was very well secured, and in it's whole length did not touch the bushes. The length of this wire was 3868 feet. As much as the place, where the observers were stationed in a corn-field, was nearer the machine than the 7 mile-stone, so much were the other observers placed beyond the 9 mile-stone, that their distance from each other might be 2 miles. The 40 feet of wire in these 2 measures exceeding 2 miles, was what connected the short iron rod before-mentioned, and the coated phial, with their respective conducting wires.

The observers being placed at their respective stations, the observer at the machine proceeded in making the explosions of the coated phial; he having before placed an assistant exactly in his view before the window of the house, who, upon the word of command, was to discharge a musket. As soon as ever the flash was seen to come from the mouth of the gun, the observer discharged the electrified phial. When 8 explosions had been made, a servant was sent from the gentlemen at the 7 mile-stone giving an account of the wire's being broken, and the sticks thrown down by a man riding through them; that the observers there had felt nothing; and desired, as by this time the wire was replaced, that we should begin again. This was complied with, and 12 other explosions made without further molestation.

Not only the first 8, but eleven of the last 12 very strongly shocked the observers at the 9 mile-stone: at the twelfth explosion the observer on purpose stood upon the wax without touching the ground with his iron rod, or any part of his body; and only felt a slight tingling in his finger and thumb that held the wire. In another of these experiments, as the gentlemen here were satisfied in their own persons of the strength of the electrical commotion, they indulged 2 country-fellows, who were by-standers, with feeling one: these 2 with 4 of the gentlemen formed a chain, the first of them taking hold of the extremity of the wire with one of his hands. They all stood upon the ground, and made no use of the iron rod. Upon the explosion they were all so strongly shocked in their arms and ankles, that the countrymen could by no means be prevailed upon to try the experiment again. Why, in the first eight explosions, the observers here were sensible of the electrical commotion, when the observers at the other station felt nothing, was explained in the former experiments. The observers at this station, from their situation under the hill, and from what wind there was being against it, never heard the report of the gun.

Though the observers near the 7 mile-stone from the breaking of their wire, were not sensible of the 8 first explosions of the charged phial, they felt the other 12. This demonstrated to the satisfaction of the gentlemen concerned, that the circuit here formed by the Electricity was 4

miles;

miles; *viz.* 2 miles of wire, and 2 miles of ground, the ſpace between the extremities of that wire. A diſtance without trial too great to be credited!! How much further the electrical commotion will be perceptible, future obſervations can only determine.

The electrical commotion by the obſervers near the 7 mile-ſtone was but ſlightly felt; not could it be otherwiſe expected, the wire in many parts of it's length touching, as was before-mentioned, the moiſt vegetables; which, in as many places as they were touched, formed ſubordinate circuits. We find, in all other inſtances, that the whole quantity of Electricity, accumulated in the coated phial, is felt equally through the whole circuit, when every part thereof is in a great degree non-electric; ſo here the whole quantity, or nearly ſo *, determined that way, was felt by the obſervers at the 9 mile-ſtone whilſt thoſe at the other ſtation felt ſo much of their quantity only, as did not go through the vegetables; that is, that proportion only in which iron is a greater non-electric than the vegetables.

Tho' the electrical commotions, felt by the obſervers near the 7 mile-ſtone, were not ſtrong; they were equally concluſive in ſhewing the difference between the reſpective velocities of Electricity and ſound.

The ſpace through which ſound is propagated in a given time, has been very differently eſtimated by the authors, who have written concerning this ſubject. *Roberval* gives it at the rate of 560 feet in a ſecond; *Gaſſendus*, at 1473; *Merſenne* at 1474; *Du Hamel*, in the Hiſt. of the *Acad. Sc. Par.* at 1172; the *Acad. del Cimento*, at 1185; *Boyle* at 1200; *Roberts* at 1300; *Walker* at 1338; Sir *I. Newton* at 968; Dr *Derham*, in whoſe meaſure Mr *Flamſtead* and Dr *Halley* acquieſced, at 1142. But by the accounts ſince publiſhed by M. *Caſſini de Thury* in the Memoirs of the *R. Acad.* of *Sciences* at *Paris* for the year 1738. where cannon were fired at various as well as great diſtances, under great variety of weather, wind, and other circumſtances, and where the meaſures of the different places had been ſettled with the utmoſt exactneſs, ſound was propagated at a *medium* at the rate only of 1038 *French* feet in a ſecond. The *French* foot exceeds the *Engliſh* by ſeven lines and a half, or is as 107 to 114: and conſequently 1038 *French* feet are equal to 1106 *Engliſh* feet. The difference therefore of the meaſures of Dr *Derham* and M. *Caſſini* is 34 *French* 36 *Engliſh* feet in a ſecond †. According to this laſt meaſure, the velocity of ſound, when the ‖ wind is ſtill, is ſettled at the rate of a mile, or 5280 *Engliſh* feet in $4'' \frac{21}{100}$

* The author of this paper, from a great variety of experiments, is of opinion; that in this and the like diſpoſitions of the *apparatus*, the electrical power, accumulated in the matter contained in the coated phial, is directed upon the exploſion thereof towards both obſervers at the ſame inſtant.

† M. *Caſſini du Thury* afterwards meaſured the velocity of ſound at *Aiguemortes* in *Languedoc*, and found the obſervations there from thoſe made about *Paris* vary only half a toiſe in a ſecond. See *Mem. de l'Acad. Royale des Sciences, pour l'année* 1739, *p* 126.

‖ Dr *Derham* found, that when ſound was carried againſt the wind, not only it's diſtance but it's velocity was leſſened; and in M. *Caſſini*'s Memoir, there is an experiment, where ſound being carried againſt the wind, which then blew very ſtrong, was retarded near $\frac{1}{12}$ of the uſual time in it's progreſs.

To return to our purpoſe; the length of the conducting wire from the machine to the obſervers near the 7 mile-ſtone, was (as has been before-mentioned) a mile, a quarter, and 8 poles, or 6732 feet: the length of that to the 9 mile-ſtone, 3868 feet. The firſt of theſe meaſures only was made uſe of in the preſent operations concerning the velocity of Electricity. In 12 diſcharges of the coated phial, which were felt by Mr *G. Graham*, Mr *Short*, and *Ch. Stanhope*, Eſq; the obſervers near the 7 mile-ſtone, and who, by a ſecond watch of Mr *Graham*'s, meaſured the time between feeling the electrical commotion, and hearing the report of the gun, with the utmoſt attention and exactneſs; the time, I ſay, between feeling the electrical commotion, and hearing the report of the gun, was, at a *medium*, 5″ , or $5''\frac{250}{1000}$. And as the gun was diſtant from theſe obſervers 6732 feet, it follows, from the experiments which have been made on the velocity of ſound, that the real inſtant of the diſcharge of the gun preceded that of the obſervers hearing it's report, at this time when the ſtrength of the wind was not ſo great as to enter into the computation, $6''\frac{087}{1000}$; or preceded the inſtant when the electrical commotion was felt only $0''\frac{837}{1000}$. But this inſtant was, from the nature of the experiment, neceſſarily prior to that of the electrical exploſion, which was not made 'till the fire of the gun was actually ſeen; and therefore the time between the making of that exploſion, and it's being actually felt by the obſerver, which muſt have been leſs than $0''\frac{837}{1000}$, was really ſo ſmall, as not to fall under any certain obſervation, when it is to be diſtinguiſhed from that, which muſt of neceſſity be loſt, between the firing of the gun, and the electrical exploſion itſelf.

In all the experiments, where the circuit was formed to any conſiderable length, though the coated phial was very well charged, the ſnap at the gun-barrel, upon the exploſion, was not near ſo loud as when the circuit is formed in a room; ſo that a by-ſtander, though verſed in theſe operations, from ſeeing the flaſh, and hearing the report, would imagine the ſtroke at the ends of the conducting wire to be very ſlight; the contrary whereof, when the wire has been properly conducted, has always happened.

From a review of theſe experiments, the following obſervations may be deduced.

I. That, in all the preceding operations, when the wires have been properly conducted, the electrical commotions from the charged phial have been very conſiderable only, when the obſervers at the extremities of the wire have touched ſome ſubſtance readily conducting Electricity with ſome part of their bodies.

II. That the electrical commotion is always felt moſt ſenſibly in thoſe parts of the bodies of the obſervers, which are between the conducting wires, and the neareſt and the moſt non-electric ſubſtance; or in other words, ſo much of their bodies, as comes within the electrical circuit.

III. That

III. That, upon these considerations, we infer, that the electrical power is conducted between these observers by any non-electric substances, which happen to be situated between them, and contribute to form the electrical circuit.

IV. That the electrical commotion has been perceptible to 2 or more observers at considerable distances from each other, even as far as 2 miles.

V. That when the observers have been shocked at the end of 2 miles of wire, we infer, that the electrical circuit is 4 miles; *viz.* 2 miles of wire, and the space of 2 miles of the non-electric matter between the observers, whether it be water, earth, or both.

VI. That the electrical commotion is equally strong, whether it is conducted by water or dry ground.

VII. That if the wires between the electrifying machine and the observers are conducted upon dry sticks, or other substances non-electric in a slight degree only, the effects of the electrical power are much greater than when the wires in their progress touch the ground, moist vegetables, or other substances in a great degree non-electric.

VIII. That by comparing the respective velocities of Electricity and sound, that of Electricity, in any of the distances yet experienced, is nearly instantaneous.

I shall conclude this paper with observing, that it was thought convenient to lay a detail of all the operations relating to these experiments before the *Society*; in consequence of which the gentlemen may make themselves judges, how far the deductions here recited are warrantable from the experiments.

* The gentlemen concerned were desirous, if possible, of ascertaining the absolute velocity of Electricity at a certain distance; because, although last year, in measuring the respective velocities of Electricity and sound, the time of its progress was found to be very little, yet we were desirous of knowing, small as that time was, whether it was measurable; and I had thought of a method for this purpose.

Accordingly, *August* 5. 1748. there met at *Shooters-Hill* for this purpose, the Pres of the *R. Soc.* the Rev. Mr *Birch*, the Rev. Mr Professor *Bradley*, *James Burrow*, Esq; Mr *Ellicot*, Mr *G. Graham*, *Rich. Graham*, Esq; the Rev. Mr *Lawrie*, *Charles Stanhope*, Esq; and myself, who were of the *Royal Society*, Dr *Bevis*, and Mr *Grischow*, a member of the *R. Acad.* of *Sciences* at *Berlin*.

It was agreed to make the electrical circuit of 2 miles, in the middle of which an observer was to take in each hand one of the extremities of a wire, which was a mile in length. These wires were to be so disposed,

* These experiments to measure the absolute velocity of Electricity were made whilst this paper was at the press; but as they had so near a relation to the experiments made the preceding year, it was thought proper to insert them here.

that

that this obſerver being placed upon the floor of the room near the electrifying machine, the other obſervers might be able in the ſame view to ſee the exploſion of the charged phial, and the obſerver holding the wires, and might take notice of the time lapſed between the diſcharging the phial, and the convulſive motions of the arms of the obſerver in conſequence thereof; inaſmuch as this time would ſhew the velocity of Electricity, through a ſpace equal to the length of the wire between the coated phial and this obſerver.

The electrifying machine was placed in the ſame houſe as it was laſt year. We then found ourſelves greatly embarraſſed by the wire's being conducted by the ſide of the road, which we were compelled to, on account of the ſpace neceſſary for the meaſuring of ſound: but ſo great a diſtance from the machine was not now wanted, though the circuit through the wire was intended to be at leaſt 2 miles. We had diſcovered by our former experiments, that the only caution now neceſſary was, that the wires conducted upon dry ſticks ſhould not touch the ground, each other, or any non-electric in a conſiderable degree in any part of their length: if they did not touch each other, the returns of the wire, be they ever ſo frequent, imported little, as the wire had been found to conduct Electricity ſo much better than the ſticks. It was therefore thought proper to place theſe ſticks in a field 50 yards diſtant from the machine. The length of this field being 11 chains, or 726 feet, 8 returns of the wire from the top to the bottom of the field made ſomewhat more than a mile, and 16 returns more than 2 miles, the quantity of wire intended for the Electricity to paſs through to make the experiment.

We had found laſt year, that, upon diſcharging the electrified phial, if two obſervers made their bodies part of the circuit, one of which graſped the leaden coating of the phial in one hand, and held in his other one extremity of the conducting wire; and if the other obſerver held the other extremity of the conducting wire in one hand, and took in his other the ſhort iron rod with which the exploſion was made; upon this exploſion, I ſay, they were both ſhocked in the ſame inſtant, which was that of the exploſion of the phial. If therefore an obſerver, making his body part of the circuit, was ſhocked in the inſtant of the exploſion of the charged phial in the middle of the wire, no doubt would remain of the velocity of Electricity being inſtantaneous through the length of that whole wire. But if, on the contrary, the time between making the exploſion, and ſeeing the convulſions in the arms of the obſerver holding the conducting wires, was great enough to be meaſured, we then ſhould be able to aſcertain it's velocity to the diſtance equal to half the quantity of wire employed only, let the manner of the Electricity's diſcharging itſelf be what it would.

It has been a queſtion with ſome, who have conſidered this ſubject, whether the Electricity, in compleating the circuit from the matter contained in the glaſs, paſſed, either by the wire in the mouth to the coating of the glaſs, the contrary way by the coating to the wire in the mouth,

mouth, or otherwife directed itfelf both ways at once? That the Electricity muft pafs off one of thefe 2 ways was certain, as the explofion would not be complete, unlefs in the inftant thereof fome matter very non-electric communicated between the wire in the mouth, and the coating of the glafs. Unlefs therefore the obferver was placed in the centre of the conducting wires, it might be objected, that the experiment was not made with the exactnefs neceffary; becaufe any perfon, who was of opinion that the Electricity directed itfelf from the mouth of the glafs to the coating, might object, if the wire from the fhort iron rod to the obferver was only half the length of that between the obferver and the coating of the glafs, that the Electricity in the time found, paffed only through the fhort wire, and *vice verfa.* But if, as it was here thought proper, the obferver was placed in the centre of the conducting wire, let the direction of the Electricity be what it would, no difference could happen in the refult of the experiments, if made with the neceffary caution; becaufe, if the effects in the middle and both ends of the wires were inftantaneous, the conclufion therefrom would be very obvious.

To make the experiment, the fame phial filled with filings of iron, and coated with fheet-lead, which was ufed laft year, was placed in the window of the room near the machine, and was connected to the prime conductor by a piece of wire. To the coating of this phial a wire was faftened, which, being conducted upon dry fticks to the before-mentioned field, was carried in like manner to the bottom, and being conducted thus from the bottom of the field to the top, and from the top to the bottom 7 other times, returned again into the room, and was held in one hand of an obferver near the machine. From the other hand of this obferver, another wire of the fame length with the former was conducted in the fame manner, and returned into the room, and was faftened to the iron rod with which the explofion was made. The whole length of thefe wires, allowing 10 yards for their turns round the fticks, amounted to 2 ¼ miles and 6 chains, or 12276 feet.

As the night preceding thefe experiments had been very rainy, care was taken, by filk lines properly difpofed, that the wires in their paffage from the window of the houfe might not touch the wood thereof; left, from the moifture of this wood, the electrical circuit might be fhortened.

When all parts of the *apparatus* were properly difpofed, feveral explofions of the charged phial were made; and it was invariably feen, that the obferver holding in each hand one of the extremities of thefe wires, was convulfed in both his arms in the inftant of making the explofions.

Inftead of one, 4 men were then placed, holding each other by the hand near the machine, the firft of which held in his right hand one extremity of the wire, and the laft man the other in his left. They were all feen convulfed in the inftant of the explofion. Every one who felt it, complained of the feverity of the fhock.

It

It was then deſired by one of the gentlemen concerned, that an exploſion ſhould be made with the obſerver holding only one of the wires. This was done accordingly; but the obſerver felt nothing, the phial diſcharging itſelf in a different manner to what it did before, on account of the circuit's not being completed.

It was then tried, whether an obſerver would be ſhocked upon the diſcharge of the phial, if the 2 wires at their extremities ſlightly touched each other, whilſt an obſerver at the ſame time held one of theſe about a foot from their ends in each of his hands? Upon trial he felt nothing, though the phial exploded very quick, becauſe the iron wire conducted the Electricity better than the body of the obſerver.

It was then tried, whether or no, as the ground was wet, if the exploſion was made with the obſerver holding the extremity of each wire ſtanding upon the ground near the window of the houſe, any difference would ariſe in the ſucceſs of the experiment? No difference was found, the obſerver being ſhocked in the inſtant of the exploſion as before, in both his arms, and acroſs his breaſt.

Upon theſe conſiderations we were fully ſatisfied, that through the whole length of this wire, being, as I mentioned before, 12276 feet, the velocity of Electricity was inſtantaneous.

As it was found laſt year, we obſerved again, that although the electrical commotions were very ſevere to thoſe who held the wires, the report of the exploſion at the prime conductor was little, in compariſon of that which is heard when the circuit is ſhort. From whence it was conjectured, that the very loud report, in the experiment of *Leyden*, is confined to a very ſhort circuit.

Fig. 18. *A*, The prime conductor. *BB*, the ſilk lines. *C*, the coated phial. *D*, it's hook communicating with the prime conductor. *EE*, the wire reaching from the coating of the phial to the left hand of the obſerver, being more than a mile in length. *F*, the place of the obſerver. , a ſuppoſed line, drawn upon the exploſion through his body and arms. *GG*, another wire, of the length of *EE*, which goes from the right hand of the obſerver to *H*. *H*, the ſhort iron rod to make the exploſion.

§ I. 28. The laſt paper contained ſome accounts of what had been done by ſome gentlemen of the *Society*, in order to examine, not only to what diſtance the electrical power was perceptible, but alſo to inveſtigate, as near as might be, the reſpective velocities of Electricity and ſound: Electricity indeed is the ſubject of the preſent paper, yet, as it relates to *phænomena* thereof different from thoſe mentioned in the former, I thought proper to ſeparate them.

Some further inquiries into the nature and properties of Electricity; by the ſame. Ibid. p. 93.

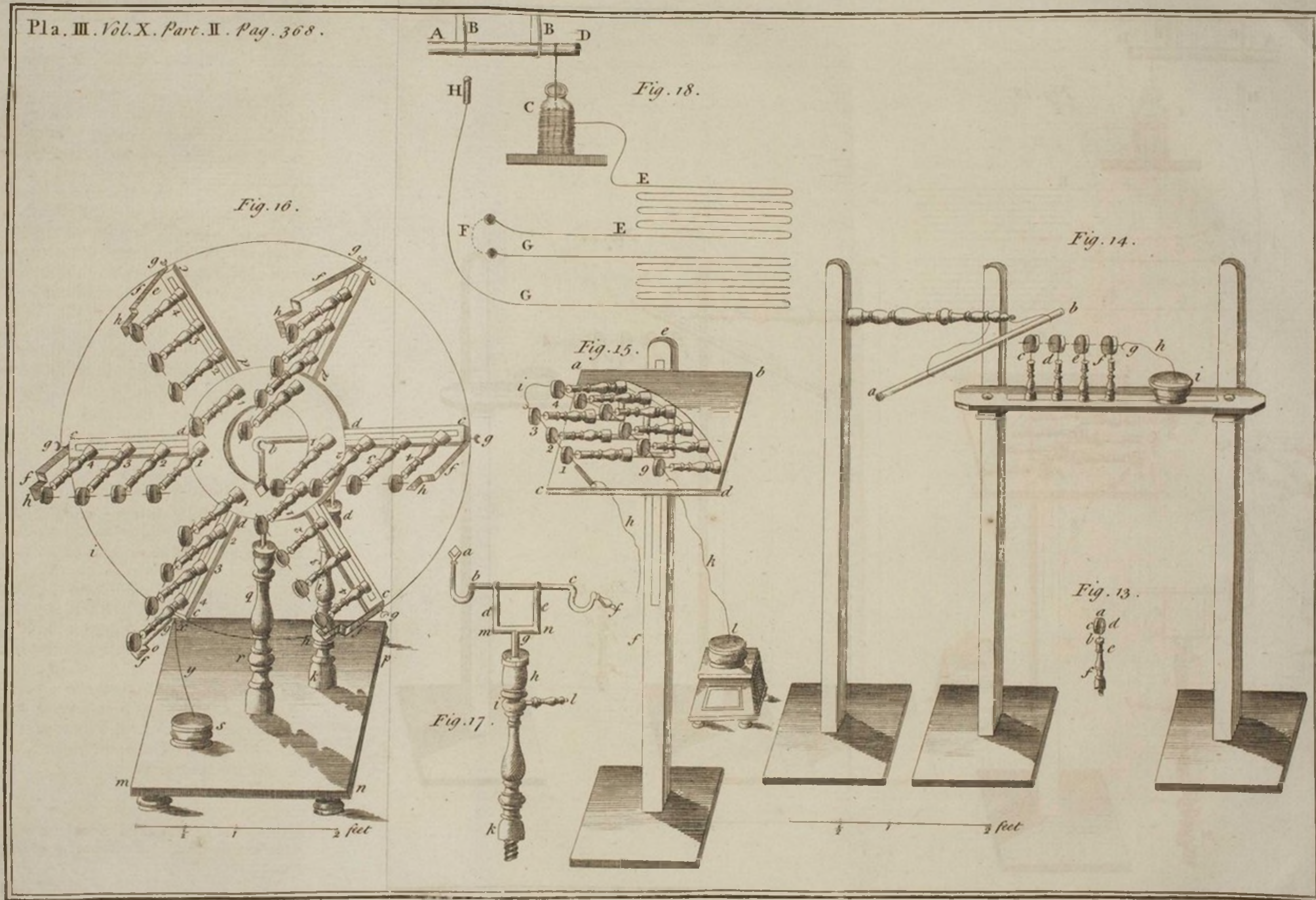
***Read* Jan. 21. 1747-8.**

§ II. I took notice, in my ſequel to the experiments relating to Electricity *, of an obſervation of the ingenious Profeſſor *Boſe* of *Wittemberg*, *viz.* ' that if the electrifying machine is placed upon originally-electrics,

* Art. 9.

' the

Pla. III. Vol. X. Part. II. Pag. 368.



'the man who rubs the globe with his hands, even under theſe apparently favourable circumſtances, gives no ſign of being electriſed when touched by an unexcited non-electric. But if another perſon, ſtanding upon the floor, does but touch the globe in motion with the end of one of his fingers, or any other non-electric, the perſon rubbing is inſtantly electriſed, and that very ſtrongly.' This experiment, almoſt a year ſince, Dr *Bevis* carried further, by placing whatever non-electric touched the globe as a conductor, whether it were a man or a gun-barrel, upon originally-electrics. If then, either the man who rubbed the globe, or he who only held his finger near the equator thereof, were touched by any perſon ſtanding upon the floor, a ſnapping from either of them, I ſay, was perceptible upon that touch

As in my ſequel I had aſſerted, and by many experiments therein had endeavoured to evince, that, contrary to the received opinion, the Electricity was not derived from glaſs, the air, or other electrics *per ſe*, I was deſired to conſider how far this experiment did not prove the reverſe of that aſſertion; inaſmuch as neither the man who rubbed the globe, or he who touched it with his finger, from their being here both ſupported by originally-electrics, could receive any ſupply from the floor; and yet both of them ſnapped upon the touch of a perſon not ſupported by electrics *per ſe*. Many experiments had proved that the Electricity was not derived from the glaſs; and therefore it was concluded, by Dr *Bevis*, and ſeveral others to whom this gentleman ſhewed the experiment, that the Electricity here was communicated to the perſon rubbing from the air, by means either of the ſuſpended gun-barrel, or of the man who touched the globe. § III.

I was by no means ſatisfied with this concluſion, as being directly contrary to numberleſs facts. From a careful conſideration therefore of the experiment itſelf, from comparing it's effects with thoſe of ſeveral others, and, in general, from ſurveying all the properties of Electricity we are hitherto acquainted with, I gave the following as my opinion. § IV.

1. That what we call Electricity is the effect of a very ſubtile and elaſtic fluid, diffuſed throughout all bodies in contact with the terraqueous globe (thoſe ſubſtances hitherto termed Electrics *per ſe* probably excepted), and every-where, in it's natural ſtate of the ſame degree of denſity.
2. That this fluid manifeſts itſelf only, when bodies capable of receiving more thereof than their natural quantity are properly diſpoſed for that purpoſe; and that then, by certain known operations, it's effects ſhew themſelves by attracting and repelling light ſubſtances, by a ſnapping noiſe, ſparks of fire, *&c.* directed towards other bodies, having only their natural quantity, or, at leaſt, a quantity leſs than thoſe bodies from which theſe ſnappings, *&c.* proceed.
3. That no ſnapping is obſerved in bringing any two bodies near each other, in which the Electricity is of the ſame denſity, but only in thoſe bodies in which the denſity of this fluid is unequal.

4. That this ſnapping is greater or leſs, in proportion to the different denſities of the Electricity in bodies brought near each other, and by which ſnapping each of them becomes of the ſame ſtandard.
5. That glaſs, and other bodies, which we call Electrics *per ſe*, have the property of taking this fluid from one body, and conveying it to another, and that in a quantity ſufficient to be obvious to all our ſenſes.
6. That, in the experiment in queſtion, the reaſon why no ſnapping is obſerved by a perſon upon the floor touching him who rubs the globe with his hands ſtanding upon wax, without at the ſame time ſome other non-electric ſupported by originally-electrics, or otherwiſe being in contact with the globe, is owing to whatever part of this man's natural quantity of Electricity, taken from himſelf by the globe in motion, being reſtored to him again by the globe in it's revolutions; there not being any other non-electric near enough to communicate the Electricity to; and that therefore, in this ſituation, the Electricity of this man ſuffers no diminution of it's denſity.
7. That the fact is otherwiſe, when every thing elſe being as before, either a gun-barrel ſuſpended in ſilk lines, or a man ſupported by wax, or ſuch like, is placed near the globe in motion; becauſe then, whatever part of the Electricity of the perſon rubbing is taken from him, is communicated either to the other man or to the gun-barrel, theſe, from their ſituation, being the firſt non-electrics, to which the Electricity taken from the perſon rubbing can be communicated.
8. That, under theſe circumſtances, as much Electricity as is taken from the perſon rubbing, is given to the other; by which means the Electricity of the firſt man is more rare than it naturally was, and that of the laſt more denſe.
9. That the Electricity in either of theſe perſons is in a very different ſtate of denſity from what it naturally was, or from that of any perſon ſtanding upon the earth; this laſt being in a middle ſtate between the two other perſons; that is, he has not his Electricity ſo rare as the man rubbing the globe, nor ſo denſe as that of him ſupported by electrics *per ſe*, and touching the equator of the globe.
10. That therefore the ſame effect, a ſnapping, is obſerved, upon bringing any non-electric near either of theſe perſons, from very different cauſes: for it is apprehended, that, by bringing the non-electric near him, whoſe Electricity is more rare, this ſnapping reſtores to him what he had loſt; and that, by bringing it near him, whoſe Electricity is more denſe, it takes off his ſurcharge, by which means their original quantity is reſtored to each.

§ V. This ſolution of this *phænomenon*, without allowing any part of the Electricity of either of theſe two perſons to be furniſhed by the circumambient air, was ſatisfactory, not only to the gentleman who propoſed it, but to many of the *Royal Society*, excellent judges of this matter,

matter, to whom I ſhewed the experiment: and this the more ſo, as it is to be obſerved, that if, under the before-mentioned circumſtances, the perſon rubbing the globe was touched by him who held his finger to the globe, the ſnapping was much greater than if either of them touched a perſon ſtanding upon the floor; as the denſity of the Electricity between theſe two perſons was ſo much more different than that of either of them to him on the floor: whereas did their Electricity proceed from the air, from their being both electriſed they ought not to ſnap at all from their touching each other; or, admitting they did touch each other, they both of them, upon a ſuppoſition that they did receive their Electricity alike from the air, ſhould manifeſt the accumulation thereof, and ſnap upon the touch of a man ſtanding upon the floor, the contrary of which invariably happens.

At this time I am the more particular concerning the ſolution of this ſingular appearance, as Mr *Collinſon*, has received a paper concerning Electricity from an ingenious gentleman, Mr *Franklin*, a friend of his in *Penſylvania*. This paper, dated *June* 1. 1747, I very lately peruſed, by favour of our moſt worthy *Preſident*. Among other curious remarks there is a like ſolution of this fact; for though this gentleman's experiment was made with a tube inſtead of a globe, the difference is noways material. As this experiment was made, and the ſolution thereof given, upon the other ſide of the *Atlantic Ocean* before this gentleman could poſſibly be acquainted with our having obſerved the ſame fact here, and as he ſeems very converſant in this part of Natural Philoſophy, I take the liberty of laying before you his own words. § VI.

'1. A perſon ſtanding on wax, and rubbing a tube, and another perſon 'on wax drawing the fire; they will both of them, provided they do 'not ſtand ſo as to touch one another, appear to be electriſed to a perſon 'ſtanding on the floor; that is, he will perceive a ſpark on approach-'ing each of them with his knuckle.

'2. But if the perſons on wax touch one another during the exciting 'of the tube, neither of them will appear to be electriſed.

'3. If they touch one another after the exciting the tube and draw-'ing the fire as aforeſaid, there will be a ſtronger ſpark between them, 'than was between either of them and the perſon on the floor.

'4. After ſuch a ſtrong ſpark neither of them diſcover any Electri-'city.

'Theſe appearances we attempt to account for thus:

'We ſuppoſe, as aforeſaid, that electrical fire is a common element, of 'which every one of theſe three perſons has his equal ſhare before 'any operation is begun with the tube. *A*, who ſtands upon wax, 'and rubs the tube, collects the electrical fire from himſelf into the 'glaſs; and his communication with the common ſtock being cut off 'by the wax, his body is not again immediately ſupplied. *B*, who 'ſtands upon wax likewiſe, paſſing his knuckle along near the tube, 'receives the fire which was collected by the glaſs from *A*; and his 'communication with the common ſtock being cut off, he retains the 'additional quantity received. To *C* ſtanding on the floor, both ap-'pear to be electrifed: For he, having only the middle quantity of 'electrical fire, receives a ſpark upon approaching *B*, who has an over 'quantity, but gives one to *A*, who has an under quantity. If *A* and '*B* approach to touch each other, the ſpark is ſtronger; becauſe the 'difference between them is greater. After ſuch touch, there is no 'ſpark between either of them and *C*, becauſe the electrical fire in 'all is reduced to the original equality. If they touch while electri-'ſing, the equality is never deſtroyed, the fire only circulating. Hence 'have ariſen ſome new terms among us. We ſay, *B*. (and bodies 'alike circumſtanced) is electrifed poſitively; *A*, negatively; or, 'rather, *B* is electrifed *plus*, *A*, *minus*. And we daily in our experi-'ments electriſe *plus* or *minus*, as we think proper. To electriſe *plus* 'or *minus*, no more needs be known than this; that the parts of the 'tube or ſphere that are rubbed, do in the inſtant of the friction at-'tract the electrical fire, and therefore take it from the thing rubbing. 'The ſame parts immediately, as the friction upon them ceaſes, are 'diſpoſed to give the fire, they have received, to any body that has 'leſs. Thus you may circulate it, as Mr *Watſon* has ſhewn *; you 'may alſo accumulate or ſubſtract it upon or from any body, as you 'connect that body with the rubber, or with the receiver, the com-'munication with the common ſtock being cut off.'

The ſolution of this gentleman, in relation to this *phænomenon*, ſo exactly correſponds with that which I offered very early laſt ſpring, that I could not help communicating it.

§ VII. In Sect. 51. and 62. of my ſequel, from not having conſidered this experiment in a ſtatical view, and from not then imagining the velocity of Electricity ſo great as we ſince have found it, I concluded, that the ſnapping obſerved, if a perſon ſtanding upon the floor touched the man ſtanding upon wax, who turned the wheel of the electrifying machine placed likewiſe upon wax, to be owing to the inverſion of the uſual courſe of the Electricity; as that ſnapping was only conſtant, when the gun-barrel ſuſpended in ſilk lines was touched by non-electrics. As from divers experiments I had found that Electricity was not furniſhed by dry air, by many more that it could not come down clean ſilk lines; and as, from his ſnapping, the man upon the wax argued the preſence of Electricity, I conceived that this could happen no other way, than that the rubbing of the globe by a cuſhion or the hand of a man, gave it a fitneſs to take off the Electricity, furniſhed by the ſuſpended gun-barrel from the non-electric upon the floor, and lodge it upon the machine, and upon the man who turned the wheel thereof. But the expe-

* *See* Art. 9. § 64.

riment

riment of circulating the electrical fire *, where the brush of blue flame from a blunt wire properly disposed, can always be seen to pass diverging into the machine, though not so, when brought near the gun-barrel under the most favourable circumstances; as well as the experiment before-mentioned brought to shew that the Electricity came from the air, have induced me to change my opinion; and instead of the course of the Electricity being inverted, the *phænomena* arose, as far as I am capable of judging, from the man who turned the wheel of the electrifying machine having less than his original quantity of Electricity, and the gun-barrel from having more: to these add, that the person, who touched these while standing upon the floor, had a quantity different from each of these, that is, his natural quantity.

I beg leave to correct also what I mentioned in my *sequel*, in relation to my suggesting, that, in the explosion of the charged phial through the body of a man, or other non-electrics, as much Electricity as was taken from his body, was immediately replaced by the floor of the room upon which he stood: I having since found, that the charged phial would explode with equal violence, if the hook of the wire, which is usually run through the cork of the phial, was bent in such a manner as to come near the coating of the phial, without any other non-electric being near, from which such quantity could be supplied. § VIII.

I take notice of these, inasmuch as, notwithstanding the very great progress that has been made in our improvements in this part of Natural Philosophy within these few years, posterity will regard us only as in our noviciate; and therefore it behoves us, as often as we can be justified therein by experiment, to correct any conclusions we may have drawn, if others yet more probable present themselves. § IX.

I laid down and considered largely in my sequel, that the stroke from the phial, in the experiment of *Leyden*, was not in proportion to the quantity of matter contained in the glass, but was increased by the quantity of matter in the glass, and the number of points of non-electrical contact on the outside of the glass. This fact I have pursued further, and increased thereby the electrical explosion to an astonishing degree. To this end I procured 3 cylindrical phials blown very thin, about 17 inches in height and 4 in. diameter: after these were coated within an inch of their necks with sheet-lead, I put into each 50 pounds of leaden shot. I chose this form for the glasses, that the matter therein contained might be exposed under as large a surface, as could conveniently be obtained. These glasses were placed near each other in a convenient part of my room, and did communicate with each other by means of a small iron rod lying upon all their mouths, and touching pieces of strong wire stuck into the shot contained in them: by this management one of these could not be electrised without communicating with the rest. The leaden coatings of these glasses were also connected together by small wires, all which centered in one tail wire; so that, when the matter contained in § X.

* Art. 9. § 65.

these

these 3 glaſſes was replete with Electricity, which was done by a wire from the gun-barrel faſtened to the iron rod lying upon their mouths, the whole quantity of Electricity here accumulated might be diſcharged at once by touching the gun-barrel with an iron rod faſtened to the tail wire. When the glaſſes are ſufficiently electriſed, if the room is dark, you will ſee bruſhes of blue flame from ſeveral parts of the conducting wire; and theſe indicate the proper time of making the exploſion. Theſe glaſſes, from the thinneſs of their ſides, and from the weight of their leaden ſhot, are very liable to burſt; and if one of them happens to have the leaſt crack in any part of it's ſurface, which is under the lead, none of them can be electriſed; all the Electricity paſſing off by that crack. The electrical exploſion from 2 or 3 of theſe glaſſes is not double or treble to that from one of them; but the exploſion from three is much louder than that from two, that from two much louder than that from one.

§ XI. The experiment juſt mentioned induced me to imagine, that the exploſion from theſe phials was owing to the great quantity of non-electric matter contained in them: and whilſt I was conſidering of ſome certain method of aſſuring myſelf whether the fact were ſo, Dr *Bevis* informed me, that he had found the electrical exploſion to be as great, as when he had accumulated the Electricity in a half pint phial of water, by the following method. He covered a thin plate of glaſs, of about a foot ſquare on both ſides, with leaf-ſilver; this he made to adhere to the glaſs with very thin paſte. A margin of an inch was left on both ſides; otherwiſe, upon electrifying this plate, the Electricity would be prevented from being accumulated upon one of it's ſurfaces, by being propagated from the ſilver on one ſide to that of the other. When the glaſs plate was thus prepared, if it was placed upon a table in ſuch a manner, that when fully electriſed by a wire or ſuch-like from the prime conductor, a perſon touched the under ſurface with a finger of one of his hands, and brought one of the fingers of his other near the upper ſurface thereof, or near the prime conductor, he was ſhocked in both his arms and acroſs his breaſt. The ſame effect happened, if, when this plate was electrified in the before-mentioned manner, a perſon holding it in his hand by the margin, and without touching the ſilver, preſented it, even ſome time after it had been taken from the prime conductor, to another perſon who touched the under ſurface with his finger, and held it there till he touched the upper ſurface with a finger of his other hand.

§ XII. This experiment was ſufficiently convincing, that the greatneſs of the electrical exploſion, in my former trials, was not owing ſolely to the great quantity of non-electric matter contained in the glaſſes; as the exploſion from the glaſs plate ſilvered was occaſioned by about ſix grains of ſilver, upon which the Electricity was accumulated; more eſpecially as this exploſion was equal, if not ſuperior, to that from half a pint of water contained in a thin glaſs as uſual, under the moſt favourable circumſtances.

As

As each of the ſurfaces of the glaſs plate juſt mentioned meaſured 64 ſquare inches, I was deſirous of purſuing this inquiry further; and accordingly procured a cylindrical glaſs jar blown very thin, of 16 inches in height, and 18 inches in circumference. This I cauſed to be covered both within and without with leaf-ſilver, to within an inch of it's top. This glaſs with it's margin made very clean (upon which the ſucceſs of the experiment conſiderably depends) was fully electriſed by the means of a piece of chain, let down to the bottom of the jar, by a wire from the prime conductor; and the exploſion made by it's being placed upon a plate of metal, to which was faſtened a wire connected to an iron rod, and this rod was brought near ſome gilded leather lying upon the prime conductor. This exploſion was equal to that from the 3 glaſſes before-mentioned, containing 150 pounds of leaden ſhot; though here the weight of the ſilver lining the internal ſurface of the glaſs, upon which the Electricity was accumulated, did not exceed 30 grains. So much of the internal ſurface of this jar, as was covered with ſilver, amounted, as the ſurfaces of cylinders are as their length multiplied by their periphery, and allowing 36 ſquare inches for the bottom, to 306 ſquare inches. If this exploſion was made in a dark room, the coruſcations within the jar, at the inſtant of the exploſion, were extremely brilliant. § XIII.

When this jar is fully electriſed, if, inſtead of making it explode, you only bring the ſhort iron rod, with which the exploſion is uſually made, near a piece of gilded leather lying upon the prime conductor, though not near enough to make the glaſs explode at once, you hear the Electricity, accumulated within the jar, eſcape with a noiſe very like that of a ſmall heated iron bar quenching in water.

The great exploſion from the jar before-mentioned, when ſo little non-electric matter was included therein, has cauſed me to be of opinion, that the effect of what we call the experiment of *Leyden* is greatly increaſed, if not principally owing, not ſo much to the quantity of non-electrical matter contained in the glaſs, as to the number of points of non-electrical contact * within the glaſs and the § XIV.

* Bodies having the power of readily conducting Electricity ſeems to depend very little upon their ſpecific gravity ſimply conſidered: metals, for inſtance, and water, are in a great degree non-electrics, and conſequently conduct Electricity the beſt of any ſubſtances, that have yet fallen under our notice; whereas the *calces* of metals, though very denſe bodies, and very greatly more ſo than water, prevent in a great degree the quick propagation of the electrical power So that a phial coated within and without with ceruſe, *i. e.* the *calx* of lead, and electriſed, did not, upon the application as uſual of one hand to the external ſurface thereof, and touching the prime conductor with the other, occaſion any ſhock, or make any exploſion more than the ſimple ſtroke from the prime conductor. The ſame obſervation holds good with regard to red lead, litharge, and lunar cauſtic or the *calx* of ſilver, none of which ſnap, when electriſed. For the ſame reaſon, filings of iron, which are ruſty, *i. e.* have their ſurfaces converted into a *calx*, are much leſs proper to be put in glaſſes to make the experiment of *Leyden*, than thoſe that are not; inaſmuch as theſe laſt cauſe a much louder exploſion than the firſt. The making uſe of ruſty filings of iron was the occaſion of my mentioning in my *ſequel* § 16. that the ſtroke from theſe was leſs than that from water; the contrary of which I afterwards found true, when filings of iron not ruſty were ſubſtituted.

denſity * of the matter conſtituting thoſe points, provided this matter be in it's own nature a ready conductor of Electricity. For this reaſon it is preſumed, that ſo much of the lead contained in the ſhot in the before-mentioned experiment, only concurred to make the electrical exploſion, as touched the internal ſurface of the glaſs: as a great part of this ſurface was without contact, occaſioned by ſuch of the ſhot as preſented themſelves thereto, touching, from their ſpherical figure, only in one point, there conſequently remained without contact comparatively great ſpaces between each ſhot. This defect was obviated by the univerſal contact of the ſilver, and thereby was occaſioned the greater exploſion.

§ XV. The following experiment has ſome relation to the preceding. If a phial of warm water, without being coated with ſheet-lead, or other non-electrical matter, is electriſed by connecting it to the prime conductor; and a ring of ſmall wire, in lieu of the uſual coating, is put round this phial, the wire being continued of a ſufficient length to touch the prime conductor; upon diſcharging the phial, you have a ſlight exploſion, and a flaſh of fire ſeems at that inſtant to fill the glaſs. But if this experiment is made in a very dark room, and with great attention, this flaſh in the phial will not then ſeem to proceed from the whole quantity of water contained therein; but, as far as the ſuddenneſs of the exploſion will permit the eye to follow it, will be ſeen to occupy only the internal ſurface of the phial.

§ XVI. I ordered another glaſs jar as large as poſſible to be blown, ſo that the glaſs thereof might be very thin; and after many attempts of the glaſs-makers I procured one, the height of which was 22 inches, the periphery 41. This was covered within and without, leaving a margin of an inch at top, with leaf-braſs. As much of the internal ſurface as was covered amounted to 1129 ſquare inches. But the difficulty I met with in procuring this glaſs, was ſufficiently recompenſed by the great increaſe of the exploſion therefrom, when fully electriſed, and diſcharged in the ſame manner as the glaſs jar before-mentioned. The report was vaſtly louder; all the attendant *phænemona* greatly exceeded any thing of this kind I was before acquainted with. As the quantity of metal within this jar did not exceed 2 drams, this experiment gives further weight to my opinion before-mentioned § 14. in relation to the manner of increaſing the effects of the experiment of *Leyden*; and from what the *phænemona* of that ſurpriſing experiment principally proceed; *viz.* not from the volume of the prime conductor, nor from the quantity of non-electrical matter contained in the glaſs, but from the number of points of non-electrical contact both within and withoutſide of the glaſs, and from the denſity † of the matter conſtituting thoſe points.

* I heretofore, took notice, how much the effect of this experiment depended upon the quantity of non-electric contact upon the outſide of the glaſs.

† Though the denſity of the matter conſtituting theſe points proceeds from their number in a mathematical ſenſe, yet in a popular one I take the liberty to diſtinguiſh them.

It

It muſt be obſerved, that, *cæteris paribus*, the electrical exploſion is greater from hot water included in glaſſes than from cold; and from theſe glaſs jars warmed than when they are cold. § XVII.

The exploſions from the large glaſſes juſt mentioned fully electriſed, as well as from ſmall ones under the ſame circumſtances, will not be conſiderable, unleſs the circuit, frequently mentioned in my writings upon this ſubject, be completed; that is, unleſs ſome matter, non-electric in a conſiderable degree, and in contact, with the coatings of the phials, is brought into contact, or nearly ſo, with ſuch non-electrics as communicate with the matter contained in the phials themſelves When indeed the circuit can be completed, the exploſion from the large glaſſes is prodigious; the whole quantity of Electricity therein accumulated, or nearly ſo, being diſcharged in an inſtant. But the fact is otherwiſe, if the circuit is not completed, and the iron rod in the mouth of one of theſe phials is touched by a non-electric (the hand of a man, for inſtance) not in contact with the tail wire: for then there will be no exploſion, no ſhock; but the perſon, approaching his finger near the iron rod, will ſee a ſucceſſion of ſmall ſparks, more intenſely red than that large one ſeen, when the phials explode at once; and the perſon making the experiment, will feel a very pungent pain, but confined to that finger which touches the iron rod. This ſucceſſion of ſparks continues, until the Electricity accumulated in the phials is nearly exhauſted. So that the exploſion from any given quantity of Electricity, accumulated as before-mentioned, is greater or leſs in proportion to the time expended in making that exploſion: in like manner as a given quantity of grained gunpowder rammed hard in a piſtol, is almoſt inſtantaneouſly fired, and that with a great report; when the ſame quantity of gunpowder rubbed fine, and rammed hard, takes a conſiderable time in burning as a ſquib, and makes no exploſion. § XVIII.

The cauſes why the charged phial will not explode quick, without the Electricity therein deſcribing a circuit through ſubſtances non-electric in a great degree, may be very difficult to be aſſigned. It is ſufficient for us in the preſent inquiry to be aſſured of it's being a certain, an invariable law: and in order to prove, that the Electricity, upon the exploſion, paſſes with it's whole force through the circuit of non-electrics, contrary to what has been ſuggeſted, I made the following experiment. § XIX.

I procured 2 ſmall ſquare iron bars, of about 14 inches long: an inch at each end of theſe I cauſed to be bent at right angles. Theſe iron bars were ſupported in ſuch manner (by ſubſtances whether originally-electric, or not, was no ways material) that each of their ends came within about $\frac{2}{10}$ of an inch of ſome warm ſpirit of wine, or eſſence of lemons, in 4 ſpoons placed upon a table. I then ſuſpended a common coated phial filled with filings of iron to the gun-barrel, the tail wire of which reached to a table at a few feet diſtance, and was placed under a braſs weight which ſupported the handle of the firſt of the ſpoons: over this ſpoon, at the diſtance juſt mentioned, I placed one of the ſquare iron bars, and § XX.

at it's other end was placed another ſpoon: this ſecond ſpoon touched the handle of the third, which was placed under one end of the other ſquare bar, whoſe other end came near to the ſpirit in the fourth ſpoon, the handle of which lay upon a weight; and under this was placed a wire connected to the ſhort iron rod, with which the exploſion was made, when the coated phial was charged. When the phial was well charged, if the ſpirit of wine ſent forth vapours, and the ſquare iron bars were at a proper diſtance from it; upon making the exploſion at the gun-barrel the Electricity ſnapped between the ſpirit and the iron bars, and the ſpirit was ſet on fire at the ſame inſtant in all the ſpoons. It ſometimes happened, that ſome of them only were fired. If the iron bars were too near the ſpirit, it was not fired, though the circuit was completed; becauſe then no electrical flame ſnapped between the rods and ſpirit; that effect happening only, when the parts of the non-electrics deſcribing the circuit are not in immediate contact; on the other hand, if the ſpace left between the bars and ſpirit was too great, the circuit could not be compleated, and there would be no exploſion.

§ XXI. This experiment will ſeem more ſurpriſing in the following manner. When the *apparatus* is diſpoſed of as before, the tail wire from the coated phial, before it reaches to the table, is faſtened to an iron rod ſtanding in a pail of water: another iron rod is likewiſe placed in the ſame pail of water, and a wire from this laſt reaches under the weight, which ſupports the firſt of the before-mentioned ſpoons. From beneath the weight which ſupports the handle of the fourth ſpoon, a wire reaches to an iron rod ſtanding in a ſecond pail of water, in which is placed alſo another iron rod, to which is faſtened another wire connected with the ſhort iron rod, which is employed to make the exploſion. When, with this diſpoſition of the *apparatus*, the charged phial is cauſed to explode, the ſpirit or eſſence of lemons in ſome or all of the ſpoons is ſet on fire; to accompliſh which, the Electricity muſt neceſſarily paſs through one of the pails of water, and poſſibly through both. But here it muſt be underſtood, that the pails of water ſtand upon a dry wooden, floor; for if they ſtand upon one that is wet, or upon the ground, the circuit will be, for reaſons frequently mentioned in the courſe of theſe inquiries, completed between the two pails, where the non-electric matter is continuous, and be prevented from paſſing by the ſpoons where it is not ſo; and this will defeat the ſucceſs of the experiment. The number of ſpoons in the manner before-mentioned, and their diſtance from each other, may be varied as far as is thought neceſſary. The circuit may likewiſe be directed through any number of men, provided that each of them holds in one of his hands a ſpoonful of warm ſpirit, and brings one of the fingers of his other hand at the proper diſtance to the ſpirit held in the hand of the perſon next him: by theſe means the exploſion of the charged phial will ſet on fire the ſpirit in ſeveral of the ſpoons at the ſame time, provided the perſons employed hold their hands ſufficiently ſteady.

This

This experiment exhibits new and unexpected *phænomena* : in all the § XXII.
experiments to kindle inflammable substances by Electricity hitherto attempted both here and abroad, either the spirit or the non-electric, wherewith it was intended to be set on fire, were placed upon originally-electrics. But here, on the contrary, although both one and the other are placed upon non-electrics, we see the same effect produced. Nor is the electrical power lessened, by exciting several different quantities of flame; in doing which, it passes so quick as to prevent the possibility, in several spoonfuls of spirit, fired by the same operation, of determining which of them was on fire first : And though we know from it's effects, that the Electricity goes through the whole circuit of non-electrics with it's whole vigour, it's progress is so quick as not to affect, by attracting or otherwise, light substances disposed very near the non-electrics, through which it must necessarily pass.

I would here recommend to those gentlemen of the *Royal Society*, who § XXIII.
last summer measured the respective velocities of Electricity and Sound, a process of this sort to be executed at a proper time; whereby they would be able to a very great nicety to ascertain the absolute velocity of Electricity. For it may be contrived, that a man may be placed in the same room with the electrifying machine, taking hold of a wire in each of his hands : these wires may be so managed, that by means of the electrical circuit, the man holding them may be made sensible of the electrical commotion, even under the eye of an observer at the machine; though before the Electricity can arrive at the person holding the wires, it will be obliged to pass through whatever large space shall be thought convenient for the observation. The time then spent between the explosion of the charged phial, and the person holding the wires feeling the electrical commotion, will give the absolute velocity of Electricity to great exactness *.

As my inquiries upon the subject of Electricity have always tended as § XXIV.
much as possible to the analysis thereof, I have often observed, that if, when the electrifying machine stands upon the floor, the globes thereof are rubbed with their cushions, or with hands covered with originally-electrics of a sufficient thickness, and perfectly dry, no Electricity will be perceptible upon the touch of a gun-barrel suspended in silk lines, and touching the globe in motion, or upon the touch of any other substances supported by electrics *per se*; or, in other words, there will be no accumulation of Electricity. The only originally-electrics fit for this experiment (as all unctuous substances, as wax, resin, and such-like, though electrics *per se*, by sticking to the outside of the glass render it unfit to excite Electricity from other bodies) are to be obtained from the animal kingdom : and of these only such as do not partake, from their manufacture or otherwise, of any non-electric substances. Those of this sort,

* This has been since put in execution. *See the preceding* Art.

which I have tried, and always with the ſame ſucceſs, when perfectly dry, have been ſilk (woven or not), velvet, hair-cloth, woollen-cloth, and the dry ſkins of rabbets dreſſed in their fur; and the event has been the ſame, whether theſe ſubſtances have been rubbed under a greater or a leſs degree of friction: and ſcarce any Electricity has been perceptible, when thoſe parts of theſe ſubſtances, which immediately are in contact with the globes, have been rubbed over with dry chalk, a non-electric ſubſtance. But the ſucceſs is different, when theſe originally-electric ſubſtances have lain in damp places, or have been held over the ſteam of warm water; becauſe then the water imbibed by theſe ſubſtances, ſerves as a canal of communication to the Electricity between the hands or cuſhions and the globes in the ſame manner, as the air, replete with vapours in damp weather, prevents the accumulation of Electricity in any conſiderable degree, by conducting it as faſt as excited to the neareſt non-electrics. On the contrary, moſt ſubſtances of the vegetable kingdom, whoſe form makes them fit for this treatment, though made as dry as poſſible, furniſh Electricity, though in different quantities. I have tried hemp, linnen-cloth of various kinds, paper both of linnen and hemp, cotton in the wool, fuſtian, cotton-velvet, and many others of this claſs. I have covered at one time the cuſhion, with which I rubbed a globe, with eight lamina of ſheet-lead, and have excited Electricity from that metal: and however improper a deal-board may ſeem for the purpoſe of rubbing a globe, I have more than once accumulated Electricity from that, though it's ſubſtance has the appearance of being much leſs fit than every one of the originally-electrics I mentioned before.

§ XXV. To the doctrine here laid down it may be objected, that leather is an animal ſubſtance, which, though perfectly dry, excites Electricity the ſtrongeſt of all the ſubſtances hitherto diſcovered; that dry leather ought to be conſidered as an originally-electric; and therefore, according to the rule before-mentioned, ſhould not furniſh, from rubbing the globe therewith, any Electricity at all. To this I anſwer, that though the dry ſkins of animals are electrics *per ſe*, dry leather is far from being ſo; and this is owing to the vaſt quantities of reſtringent vegetable ſubſtances imbibed by the ſkins throughout their whole contexture in the operation of tanning in ſome ſpecies of leather, and of ſaline ſubſtances, ſuch as alum, in others; both which ſubſtances are non-electric, and of theſe leather very conſiderably partakes: for by theſe the hides and ſkins of animals (and any muſcle of their bodies is liable to the ſame treatment), which otherwiſe are as putreſcent as any part of their bodies ſoever, are made to laſt through many ages, and be ſubſervient to many valuable purpoſes of life. The ſame concluſion muſt be drawn concerning hats, which, tho' made of the hair of animals, furniſh Electricity, though but in a ſmall degree: and this is occaſioned by the mucilaginous and gummy ſubſtances made uſe of by the Hatmakers, to give their manufacture a ſuitable ſtiffneſs.

From

From what I have advanced § XI. XII. XIII. XIV. XV. XVII. it may poſſibly be conjectured, that the electrical *effluvia* occupy only the ſurfaces of bodies electriſed; as we there found, that a very ſmall quantity of matter, diſtributed under a very large ſurface, would occaſion a greater accumulation of Electricity, than a very much more conſiderable quantity of matter under a leſs. But that the Electricity occupies the whole maſſes of bodies electriſed, and paſſes through their conſtituent parts, is clearly demonſtrated by the following experiments. § XXVI.

When I firſt engaged in theſe inquiries, to aſſure myſelf of this fact, I enveloped an iron rod about 3 Feet in length with a mixture of wax and reſin, leaving free from this mixture only one inch at each end. This iron was warmed, when thus fitted, that the whole of it's ſurface, where it was intended, might be covered. This rod, when electriſed at one of it's ends, ſnapped as ſtrongly at the other, as though it was without the wax and reſin. This could not have happened from the Electricity's paſſing along the ſurface of the iron rod, becauſe there it was prevented by the originally-electrics, and conſequently muſt of neceſſity paſs through it. § XXVII.

A phial of water, in the experiment of *Leyden*, can be electriſed, and may be cauſed to explode, though the wire, touching the water in the phial in making that experiment, be run through a wax ſtopple, exactly fitted to the mouth of the phial. § XXVIII.

I cauſed a glaſs tube, open at each end, and about 2 feet ½ long, to be capped with braſs cemented to the ends of the tube. In the centre of each of thoſe caps was faſtened a ſlender braſs rod; and theſe were diſpoſed ſo in the tube as to come within half an inch of each other. When the tube was properly ſuſpended in ſilk lines with one of it's extremities near a glaſs globe in motion, the braſs work at both ends ſnapped equally ſtrong. As the Electricity could not paſs along the ſurface of this tube warmed and wiped clean, this effect could not have happened, unleſs the Electricity pervaded the ſubſtance of the braſs caps. Upon touching the braſs at the end of the tube moſt remote from the electrifying machine, the ſnaps from one of the braſs rods within the tube to the other were ſeen to correſpond with the ſnaps without. More experiments of this kind might be added, but theſe, I preſume, are ſufficient to ſhew, that the Electricity occupies the whole maſſes of non-electric bodies electriſed. That the Electricity paſſes through originally-electrics to a certain thickneſs I took notice of in a paper I did myſelf the honour to communicate in *Feb.* 1745. § XXIX.

I ſhall forbear at preſent to lay before you a ſeries of experiments *in vacuo*; from the compariſon of which, with the experiments in open air it appears, that our atmoſphere, when dry, is the agent, whereby, with the aſſiſtance of other electrics *per ſe*, we are enabled to accumulate Electricity in and upon non-electrics; that is, to communicate to them a greater quantity of Electricity than they naturally have: from hence alſo we ſhall ſee, that, upon the removal of the air, the Electricity pervades. § XXX.

vades the *vacuum* to a considerable distance, and manifests it's effects upon any non-electrics, which terminate that *vacuum*: and by these means that originally-electric bodies, even in their most perfect state, put on the appearance of non-electrics, by becoming the conductors of Electricity. But these matters may possibly be the subject of a future communication.

Part of a letter from Abbé Nollet, *of the* R. Acad. *of* Sc. *at* Paris, *and F. R. S. to* M. Folkes, *Esq*; Pres. *concerning* Electricity. *Translated from the* French, *by* T. Stack, *M. D. F. R. S.* N° 486. p. 187 Feb. *and* Mar. 1748. *Read* Feb. 11. 1747-8.

29. For several years past Electricity has been my chief occupation. Last summer I read 3 Memoirs at our weekly meetings, which contained many particulars on this subject: but as these were matters of mere curiosity, and of no real use, they almost tired out my patience. I now send you some experiments, which I made during the vacation, which seem to promise at least the being of some service; but of this you will be the best judge. I will describe them in the same order as I made them, and to which I was not led by mere accident. You know, that when a vessel full of liquor, which runs out through a pipe, is electrified, the electrified jet or stream is thrown farther than usual, and is diverged into several divergent rays, much in the same manner as the water poured out from a watering pot. Every body at first sight will judge, that the stream is accelerated, and that the electrified vessel will soon be empty. I was unwilling to rely on the first appearances, and therefore resolved to ascertain the fact, by measuring the time, and the quantity of the liquor running out. And in order to know if the acceleration, supposing there was any, was uniform, during the whole time of the running out, I made use of vessels of different capacities, terminating in pipes of different bores, from 3 lines diameter to the smallest capillaries: and I give you in gross the result of upwards of 100 experiments, as it is not so easy a task to draw a safe conclusion, as may at first be imagined.

1. The electrified stream, though it divides, and carries the liquid farther, is neither accelerated nor retarded sensibly, when the pipe, through which it issues, is not less than a line in diameter.
2. Under this diameter, if the tube is wide enough to let the liquid run in a continued stream; the Electricity accelerates it a little, but less than a person would believe, if he judged by the number of jets that are formed, and by the distance to which it shoots.
3. If the tube is a capillary one, from which the water ought naturally to flow, but only drop by drop, the electrified jet not only becomes continued and divided into several, but is also considerably accelerated; and the smaller the capillary tube is, the greater in proportion is this acceleration.
4. And so great is the effect of the electrical virtue, that it drives the liquid out of a very small capillary tube, through which it had not before the force to pass, and enables it to run out in cases, where there would not otherwise have been any discharge.

These

These laſt facts have ſerved as a baſis to my inquiries. I conſidered all organized bodies as aſſemblages of capillary tubes, filled with a fluid that tends to run through them, and often to iſſue out of them. In conſequence of this idea, I imagined, that the electrical virtue might poſſibly communicate ſome motion to the ſap of vegetables, and alſo augment the inſenſible perſpiration of animals. I began, by ſome experiments, the reſult of which confirmed my notions. I electrified, for 4 or 5 hours together, fruits, green plants, and ſponges dipped in water, which I had carefully weighed; and I found, that, after this experiment, all theſe bodies were remarkably lighter than others of the ſame kind, weighed with them, both before and after the experiment, and kept in the ſame place and temper. I alſo electrified liquors of all ſorts in open veſſels; and I remarked, that the electrification augmented their evaporation, in ſome more, in others leſs, according to their different natures. Wherefore I took 2 garden pots, filled with the ſame earth, and ſowed with the ſame ſeeds; I kept them conſtantly in the ſame place, and took the ſame care of them, except that one of the two was electrified for 15 days running, for 2 or 3, and ſometimes 4 hours a day. This pot always ſhewed it's ſeeds raiſed two or three days ſooner than the other, a greater number of ſhoots, and thoſe longer, in a given time: which makes me believe, that the electrical virtue helps to open and diſplay the germs and facilitates the growth of plants. I advance this, however only as a conjecture, which deſerves further confirmation: as the ſeaſon was already too far advanced, to allow me to make as many experiments as I could have wiſhed: but here are yet other facts, of which I have a greater certainty, and which are not leſs intereſting.

I choſe ſeveral pairs of animals of different kinds, cats, pigeons, chaffinches, ſparrows, *&c.* I put them all into ſeparate wooden cages, and then weighed them. I electrified one of each pair for 5 or 6 hours together: then I weighed them again. The cat was commonly 65 or 70 grains lighter than the other; the pigeon from 35 to 38 grains; the chaffinch and ſparrow 6 or 7 grains: and in order to have nothing to charge upon the difference that might ariſe from the temperament of the individual, I again repeated the ſame experiments, by electrifying that animal of each pair, which had not been electrified before; and notwithſtanding ſome ſmall varieties which happened, the electrified animal was conſtantly lighter than the other in proportion.

Electricity therefore increaſes the inſenſible perſpiration of animals: but in what proportion? In the *ratio* of their bulks, or in that of their ſurfaces? Neither of the one or the other, ſtrictly ſpeaking, but in a *ratio* much more approaching to the latter than to the former. So that there is no room to apprehend that a human perſon electrified would loſe near a 50th part of his weight, as it appeared to me that it happened to one ſort of bird; nor the 140th part, as to the pigeon, *&c.* All that I have been hitherto able to learn upon this head, is, that a young man or woman, from 20 to 30, being electrified during 5 hours, loſt ſeveral

ounces

ounces of their weight, more than they were wont to lose, when they were not electrified. These last experiments are difficult to pursue with exactness; because the cloathing, which cannot strictly be compared to the hair or feathers of animals, retains a good share of the perspired matter, and hinders one from forming a good judgment of the whole effect of the electrical virtue.

This forced electric perspiration is very naturally accounted for, if we consider, that the electrical matter pervades the interior parts of bodies, and that it visibly darts from within outward: for it is very plain, that these electrical emanations must carry with them whatever they find in the small vessels, thro' which they are seen, or at least are known, to issue.

This explanation will, in my opinion, occur to every one, who has seen the principal *phænomena* of Electricity. But how shall we account for all the following effects? All those animals, whose perspiration is increased upon their being electrified, all those seeds, which shoot and grow quicker; all those liquors, which evaporate; all that acceleration of liquids flowing thro' tubes; all those particulars, I say, happen in the same manner, when, instead of electrifying those bodies themselves, they are only held near electrical bodies of a pretty large bulk. The notion which I have, for these 3 years past, formed of Electricity, not only affords me an explication of this, as simple as the former, but I venture to say, it was this same notion, that led me to the experiments, and made me even foresee their success.

I am not only satisfied of the existence of an *effluent* electric matter, which all the world allows, and which shews itself 1000 ways; but many convincing reasons have also assured me, that there is, round every electrified body, an *affluent* matter, which comes to it not only from the ambient air, but likewise from all the other bodies, whether solid or fluid, that are round about, and within a certain distance of it. If these surrounding bodies are of a simple nature, as a stone, a piece of iron, *&c.* nothing issues from them but pure electrical matter: but if they are animals, plants, or fruits, or, in a word, any organized bodies, or such, in the pores of which there is any substance capable of giving way to the impulses of the electric matter; this matter will, in issuing forth with the great rapidity, which it is known to have, carry along with it whatever it finds moveable enough to be displaced by it; and by so much will the weight of the body be diminished; the same effect being here produced by the *affluent* matter, as is produced on electrified bodies by the *effluent*. If you will please to read over my essay, what I advance will be better understood. The increase or diminution of perspiration is not a matter of indifference to the animal œconomy: this new method of increasing it at will may possibly prove of use; it is neither inconvenient nor dangerous; and neither I myself, nor any body else of those on whom I made my experiments, suffered even the least inconveniency from it. One feels neither motion nor heat differing from that of the natural state. Nor

did

did the animals give any signs of uneasiness, while they were elctrifying: a little weariness, and a better appetite, were the only effects we ever perceived.

As to the facility of applying this method, 'tis well known that the electrical virtue is easily transmitted a good way off by chains, *&c.*; and one may easily imagine, that an easy chair, or even a bed, suspended or supported in a proper manner, will put the most infirm persons in a situation to be very commodiously electrified. But as there is no necessity to electrify them actually, it will become easier still; for nothing more will be requisite, than to place near them a basket of old iron rendered electrical. The commonest degree of sagacity will suffice to put this method in practice, whenever it is found to be useful.

I shall observe further, that, when I electrify an animal, I render his perspiration more copious; and this effect is universal thro' every part of it. When I only place it near an electrified body, it perspires as much. But is it's whole body equally sensible of this effect? I mean, what exhales in consequence of the Electricity, does it issue from every part of his surface? I believe it does not; and that for these reasons.

If it be the electrical matter of the skin that drives out the matter of perspiration, by rushing towards the electrified body, it is natural to think, that this effect takes place only in the part out of which the electrical matter issues: thus the perspiration, which is electrically forced out, ought to issue from those parts only, which are the most directly applied toward the electrical body. Let us confirm this by experiments.

To an electrified body I apply a vessel full of liquor, which issues drop by drop thro' several little tubes placed in different parts of it's circumference: these drops become continued streams, and are accelerated, as if the vessel had been electrified: but this effect is observable on that side only which faces the electrified body.

I moisten a thick sponge with water, and cut it in two: I weigh these two halves separately; I join them again, and place the whole near a large electrified body, so as to make one half of the sponge face the body directly, and the other the contrary way. After an electrification of 5 or 6 hours, that half, which faced the electric body, was found to be lighter than the other, *&c.*

Wherefore I think I have good grounds to believe, that a man, who presents a shoulder, or one side of his head, to a large electrified body, perspires more thro' that part than thro' any other. Add to this, that since these animals, which I caused to perspire in this last manner, and which had but one side of their bodies exposed to the Electricity, lost as much of their weight, as the others which were throughly electrified; it follows, that they perspired as plentifully thro' the exposed part, as the others thro' the whole body. Whence we may infer, that, of the two methods, which I propose for augmenting insensible perspiration, the latter is the most powerful, and most proper to remove obstructions from the pores, or to scour them of any noxious humours which they may happen to contain.

 30. The

An Eſſay *towards diſcovering the* Laws *of* Electricity, *by Mr* John Ellicott, *F. R. S. in a letter to* M. Folkes, *Eſq; Pr. R. S.* *Ibid.* p. 195. *Read* Feb. 25. 1747-8.

30. The Abbé *Nolet* *, takes notice, that he was led to his inquiries, from the acceleration which (he found from a great number of experiments) was given to the motion of fluids thro' capillary tubes, upon their being electrified. As I formerly made ſeveral experiments on this ſubject, I ſhall ſubmit it to your conſideration, whether the following obſervations on thoſe experiments may deſerve the notice of this illuſtrious *Society*. In which I have principally endeavoured to prove, that the acceleration of the motion of fluids thro' capillary tubes or ſyphons, is not barely owing to their being electrified, but that, in all caſes whatſoever, there are ſome other circumſtances neceſſary, in order to produce this effect. And I doubt not but to make this fully appear, by ſhewing, that water, being electrified, may either be made to run in a conſtant ſtream thro' a capillary tube or ſyphon, or only to drop, as if it had not been electrified at all: and likewiſe, that the water may be made to run from the ſame ſyphon in a conſtant ſtream, without being made electrical, but ceaſe to run, and only drop, the moment it becomes electrical. Under the one or other of theſe caſes, I ſhall have an opportunity of taking notice of the ſeveral varieties obſervable in theſe experiments; all of which I ſhall endeavour to account for from the following general principles.

1. That the ſeveral electrical *phænomena* are produced by means of *effluvia*.

2. That the particles compoſing theſe *effluvia* ſtrongly repel each other.

3. That the ſaid particles are ſtrongly attracted by moſt if not all other bodies whatſoever.

That the electrical *phænomena* are produced by means of *effluvia*, is in general acknowledged by all the authors who have written upon Electricity, however they may differ in opinion with regard to the bodies in which they are contained. The properties I have mentioned of theſe *effluvia* may be eaſily deduced from moſt of the treatiſes lately publiſhed on this ſubject. But to leave no room for any objection, I would beg leave to obſerve, that the exiſtence of theſe *effluvia* is proved by all thoſe experiments in which a ſtream of light is ſeen to iſſue from the electrified body; particularly thoſe ſtreams which are ſeen to iſſue in diverging rays from the end of the original conductor, when made of metal, and reduced to a point; from their being felt to ſtrike againſt the hand like a blaſt of wind, when it is brought near the ſtream, and from that offenſive ſmell which generally accompanies theſe experiments, and which is always more perceptible, the more ſtrongly the ſphere is excited.

That the particles compoſing theſe *effluvia* repel each other, appears from thoſe experiments, in which 2 bodies, how different ſoever they may be in kind, repel each other when they are ſufficiently impregnated with theſe *effluvia*. As a feather, by the excited tube; the ſeveral fibres

* See the preceding Article.

of the fame feather, or two cork balls, which will be found ftrongly to repel each other, fo long as they retain any confiderable quantity of thefe *effluvia*. Which property will always decreafe, as the quantity they contain diminifhes.

That thefe *effluvia* are ftrongly attracted by moft if not all other bodies, is fo evident from almoft all the electrical experiments, as to make any particular examples of it needlefs here; efpecially as I fhall have occafion to take notice of the ftrong attraction between the electrical *effluvia* and water, in accounting for thefe experiments. And the firft, I would take notice of, I fhall now proceed to ftate as follows.

If a veffel of water is hung to the prime conductor, having a fyphon in it of fo fmall a bore that the water will be difcharged from it only in drops, on the water's becoming electrical by means of the machine, it will immediately run in a ftream, and continue to do fo, till the water is all difcharged, provided the fphere is continued in motion. Exp. I.

That water does not run in a conftant ftream, but only in drops, from a fyphon of a fmall bore, is doubtlefs owing to the fame caufe by which it is fuftained above the level in capillary tubes. If therefore water is made to run in a ftream barely by it's being impregnated with the electrical *effluvia*, it fhould follow, that if one or more capillary tubes be placed in a veffel of water, that which is fuftained in them would either fink down to a level with the reft of the water, on it's being made electrical, or at leaft that it would not continue at the fame height as before; but if the experiment is made, the water will be found to continue exactly at the fame height, whether it is electrified or not.

Again, if the bare electrifying the water was the caufe of it's running in a ftream, it would continue to run in the fame manner, fo long as the water continued electrical, which it will not do: for, on ftopping the motion of the machine, the ftream will immediately ceafe, and the water will only drop from the fyphon, notwithftanding it's being ftrongly impregnated with the electrical *effluvia*. To account then for the water's being made to run in a ftream in this experiment, I would obferve, that fo long as the machine is in motion, there is a conftant fucceffion of the electric *effluvia* excited, and which vifibly run off from the end of the prime conductor in a ftream, and as they are in like manner carried off from all bodies hung to it, thofe *effluvia* which run off from the end of the fyphon, being ftrongly attracted by the water, carry fo much of it along with them, as to make it run in a conftant ftream.

That the attraction between the water and electric *effluvia* is fufficient to produce this effect, might be proved by a variety of experiments; but I fhall only obferve, that to this attraction it is owing that filk lines and glafs tubes (which, from their imbibing fo very fmall a quantity of thefe *effluvia*, are generally made ufe of as fupports in many of the electrical experiments) on only being wetted become ftrong conductors: and that if an excited tube is held over a veffel of water, the water is found to imbibe a very confiderable quantity of this electric matter; and, on the approach of a finger, or any other non-electric body, the water will be perceived to riſe towards it; and if the finger is brought ſo near the ſurface as to draw off the *effluvia*, they will carry ſeveral particles of the water along with them towards the finger, in a direction directly contrary to that of gravity; and therefore may well be ſuppoſed, when acting in the ſame direction, to have an influence ſufficient to produce a ſtream, as in the experiment.

And that this current of the electric *effluvia* is the true cauſe why the water runs in a ſtream from the end of the ſyphon, is farther evident, in that whatever tends to increaſe or diminiſh the current of the *effluvia*, produces the ſame effect upon the water. I have already obſerved, that when the *effluvia* are ſtrongly excited, they will be ſeen to paſs off from the end of the prime conductor in luminous rays; and the ſame may be obſerved with reſpect to thoſe which paſs with the water from the end of the ſyphon; but if any non-electric body is brought under the ſyphon, as, by it's attraction, the current of the *effluvia* will be increaſed, ſo theſe luminous rays will likewiſe extend to a greater length. Again, if the motion of the machine is ſtopped, the current of the electric *effluvia* will thereby be ſtopped, and the water will immediately ceaſe to run in a ſtream, notwithſtanding it's being ſtrongly impregnated with the electrical *effluvia*.

And that the water is ſtrongly impregnated will not only appear from the drops being ſooner divided into ſmall particles than they would be if they had not been electrified, but from thoſe particles being ſeparated to a greater diſtance from each other, by the repulſive property of the electric *effluvia*; and if any of the water is received into a dry glaſs veſſel, on the approach of a finger towards it's ſurface, there will be ſeen a ſpark to iſſue from it in the ſame manner as from water electrified by an excited tube; or if any non-electrical body is brought under the ſyphon, by whoſe attraction the *effluvia* may be drawn off, the water will immediately be found to accompany it in a ſtream.

Exp. II. If the veſſel of water with the ſyphon in it is ſuſpended by any non-electric body over another ſtrongly electrified, the water will immediately run from the ſyphon in a ſtream; but if ſupported by a piece of ſilk, or any other electrical body, the water will immediately ceaſe running, and only be diſcharged in drops. Theſe *phænomena* may, from what has been already ſaid under the former experiment, be eaſily accounted for.

That the water is made to run in a ſtream, is plainly owing to the mutual attraction between the electrifed body and the water; which attraction will continue, ſo long as the veſſel which contains the water, by being ſupported by a non-electric, is prevented from retaining any of the electrical *effluvia*; theſe *effluvia* being drawn off by the non-electric body, to which the veſſel is ſuſpended: but, on the contrary, when the veſſel is ſuſpended by an original-electric, the *effluvia*, not being attracted thereby, will be prevented from running off, and the water will ſoon be found

found to have imbibed a quantity of them, ſufficient, by their repelling property, to greatly weaken, or wholly to deſtroy, the former attraction, when the water will ceaſe to run in a ſtream, and only drop, as if it had not been held near any electriſed body. M. *L'Abbé Nolet* has endeavoured to account for the former part of this experiment, by ſuppoſing there is, what he calls, both an affluent and an effluent electric matter; but he takes no notice of the latter part, which is not eaſily ſolved upon his ſuppoſition. But if what I have obſerved on theſe experiments is ſatisfactory, I apprehend I have accounted for the ſeveral *phænomena* on much more ſolid principles, and that thereby any leſs certain hypotheſis is rendered uſeleſs.

I intended to have taken ſome notice of the different acceleration of the fluids thro' tubes of different bores; but as this acceleration will always vary with the current of the electrical *effluvia*, unleſs ſome method could be found out to render this current uniform throughout the whole ſeries of experiments, the proſecution of this inquiry will be rendered extremely difficult, and the reſult will at beſt be very uncertain.

When the foregoing curious letter was read at the meeting of the *Royal Society* on *Thurſday* 25 *Feb.* 1747. I acquainted the gentlemen preſent, that the ſame ingenious author had communicated to me a paper ſeveral months before, in which he had more fully and particularly delivered his thoughts on the ſurprizing *phænomena* of Electricity, and as ſeveral perſons expreſſed their deſire of ſeeing that paper, I requeſted of him either a copy, or an abſtract of the ſame; in compliance with which he, ſome days after, gave me the two following papers, containing the ſubſtance of what he had before ſhewn me; and I immediately put them into the hands of Dr *Mortimer*, one of the Secretaries of the *Society*, who read them at the two meetings of the *Society*, on the ſeveral days noted at the head of thoſe papers.

M. Folkes.

31. The great difference I obſerved in the ſentiments of thoſe ingenious gentlemen who have favoured us with their diſcoveries in Electricity, made me very deſirous of finding out ſome general principles, by means of which I might be able to form a judgment of the ſeveral hypotheſes whereby they have endeavoured to account for the principal *phænomena* obſervable in thoſe experiments. In order to this I took a general ſurvey of all the more remarkable experiments, and out of them made choice of ſuch as I judged were moſt proper for my purpoſe; and from theſe I deduced the general principles hereafter mentioned. The advantage I promiſed myſelf from this method was, that the plainer and more ſimple the experiments were, which I made choice of, the leſs liable I ſhould be to miſtake in any concluſions drawn from them; and that every freſh experiment, I could account for by them, would be an additional proof in their favour; and if my attempt in explaining the following experiments

An Eſſay *towards diſcovering the* Laws *of* Electricity, *addreſſed to the* Royal Society. *Ibid.* p. 203. *Read* March 24. 1747-8.

ments from thoſe principles ſhould prove ſatisfactory, the truth of them would be thereby ſo fully confirmed, that we might ſafely rely on them in forming a judgment of any of the diſcoveries already made; and (how general ſoever they may ſeem to be) I doubt not but they will be found of ſervice in proſecuting our future inquiries on this ſubject.

The experiments from which I deduced theſe principles were theſe which follow.

Exp. I. If a glaſs tube is rubbed by a very dry hand, and a finger is brought near any part of it, a ſpark of fire will ſeem to iſſue from it, and ſtrike againſt the finger; and if the finger is carried at a like diſtance from the end of the tube towards the hand in which it is held, a number of ſparks at a ſmall diſtance from each other will be ſeen coming from it, and a ſnapping noiſe will be heard. The tube is then ſaid to be excited, or to be electrical; and at ſome times, when it is ſtrongly excited, ſparks will iſſue from the tube in ſtreams, not only while it is rubbing, but will continue to dart out from it for a conſiderable time after the rubbing has ceaſed, and a very ſtrong offenſive ſmell will be perceived.

Exp. II. If the tube, when thus excited, is held over ſome pieces of leaf-gold, or any light bodies whatſoever, they will be attracted towards it; and the more ſtrongly the tube is excited, the greater diſtance they will be attracted from; and when they come near the tube (tho' without touching it) they will be repelled from it, and continue to be ſo, unleſs touched by ſome other body, when they will be attracted by the tube as before: but if the tube is but weakly excited, they will be attracted quite to the tube, to which they will ſometimes adhere, without being repelled from it.

Exp. III. If a ball (of cork ſuppoſe for lightneſs) be hung by a ſilk line, and the excited tube is applied to it, it will not only be attracted, but will have an attractive quality communicated to it from the tube; and if any light bodies are brought near the ball, they will be attracted by it.

Exp. IV. As the tube, when ſtrongly excited, will not only attract, but afterwards repel any light bodies brought near it, in like manner the cork-ball will be endued with the ſame property; ſo that a ſmaller ball will firſt be attracted towards it, and then repelledfrom it, the ſame as the leaf-gold in *Exp.* 2. and on touching any other body it will be again attracted; and this may be repeated ſeveral times, provided the ſmaller ball is much leſs than the larger one, tho' the effect will conſtantly grow weaker and weaker, as every time the leſſer ball is attracted, it carries off with it ſome of the electric virtue, and is likewiſe endued with the ſame properties as the larger ball.

Mr *Gray*, Mr *Duſay*, and others, have obſerved, that this electrical quality is not only to be excited in glaſs, but in moſt ſolid bodies capable of friction (metals excepted); tho' in ſome it will be ſcarcely ſenſible, and that it is found to be ſtrongeſt in wax, reſins, gums, and glaſs: and as glaſs is the eaſieſt procured of a proper form, it has generally been uſed in making theſe experiments. It has been further obſerved, that

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thofe bodies in which the electrical quality is capable of being excited, the ftrongeft by friction will receive the leaft quantity of it from any other excited body, and therefore are properly made ufe of to fupport any body defigned to receive the electrical virtue. The truth of this will fufficiently appear from the following experiment.

Hang up two lines, one of filk, and the other of thread; that of thread will be attracted by the tube at a much greater diftance than the filk. Again; faften to each ftring a feather, or other light body; if the tube is brought to the feather faftened to the filk, it will be firft attracted, and afterwards repelled; and from the virtue communicated to it from the tube, the feveral fibres of the feather will ftrongly repel each other. But when the tube is brought to the feather faftened to the thread, the feather will be ftrongly attracted, and continue to be fo, without ever being repelled, the virtue paffing off by the thread it is hung to. If a glafs ball is hung to the filk line, it will be but weakly attracted by the tube; but one of cork or metal much ftronger. Exp. V.

Let a rod of iron be fuftained by filk lines, and by means of a glafs fphere (which can be more regularly and conftantly excited than a tube) be made electrical; it will be found to have all the properties of the excited tube mentioned in *Exp.* 1. A ftream of light will come from the end of it, if it is pointed; it will attract, repel and communicate this virtue to any other non-electric body: on the approach of a non-electric, a fpark of fire, with a fnap attending it, will come from it; which fpark will be greater or lefs, as the bodies approaching it have more or lefs of the electrical quality refiding in them; and there will likewife be the fame offenfive fmell as was obferved of the tube. Exp. VI.

From thefe experiments, which I think contain the principal *phænomena* of Electricity, may juftly be drawn the following conclufions.

1. That thefe remarkable *phænomena* are produced by means of *effluvia*; which, in exciting the electrical body, are put into motion, and feparated from it.
2. That the particles compofing thefe *effluvia* ftrongly repel each other.
3. That there is a mutual attraction between thefe particles, and all other bodies whatfoever.

That there are *effluvia* emitted from the tube when rubbed, and which furround it as an atmofphere, is evident, from that offenfive fmell arifing from them, from that fenfation on the hands or face, when the tube is brought near either of them, and from thofe fparks of light, on a ftill nearer approach of the finger to it.

That the particles of thefe *effluvia* repel each other, is proved by the cork-balls (*Exp.* 4). and the fibres of the feather (*Exp.* 5). repelling each other, when impregnated with them; and by the leaf-gold (in *Exp.* 2). being repelled by the tube, and not returning to it again, until, by coming near, or touching, fome non-electric body, the *effluvia* are drawn off from it.

From

From this property it is, that these *effluvia* expand themselves with so great a velocity whenever they are separated from the electric body; and as they are likewise capable of being greatly condensed, may we not from hence justly conclude they are elastic?

That there is a mutual attraction between these *effluvia* and most other bodies, appears from their collecting from the tube such quantities thereof, as to endue them with the same properties with the tube itself, as was proved by the 3*d*, 4*th*, and 5*th* but more particularly by the 6*th Experiment*.

These principles being admitted, it will follow, that the greater Difference there is in the quantity of electrical *effluvia* in any two bodies, the stronger will be their attraction. For, if the *effluvia* in each are equal, instead of attracting, they will repel each other; and in proportion as the quantity of electric matter is drawn from one of the bodies, will the attraction between them increase, and consequently be strongest, when any one of them has all the electrical matter drawn from it.

The particles of these *effluvia* are so exceeding small, as easily to pervade the pores of glass, as is evident, in that a feather, or any light bodies inclosed in a glass ball hermetically sealed, will be put in motion on the excited tube being brought near the outside of it; and it has been generally thought that they pass through the pores of the densest bodies; and there are several experiments which render this supposition not improbable; tho' I must acknowledge I have not yet met with any one that I think is quite conclusive.

I shall now proceed to shew, how, from these principles, the *phænomena* of some of the more remarkable experiments of Electricity may be accounted for.

Exp. VII. Let a rod of iron, pointed at one end, be suspended on silk lines, as in *Exp* the 6*th*, and by the sphere be made electrical. When the rod is strongly electrified, a stream of light in diverging rays will be seen to issue from it's point; and if any non-electric body is held a few inches from the point, the light will become visible to a greater distance, and if the non-electric body is likewise pointed, a light will seem to issue from that in diverging rays in the same manner as from the electrified rod. But if the non-electrical body is flat, and held at the same distance from the rod as the pointed one was, no light will be seen to come from it.

The principal *phænomena* to be accounted for in this experiment are; why a light is only seen at the point of the rod, and not through the whole length of it? Why this light is visible to a greater length, when the point is approached by a non-electric? And, why a light is seen to issue from the non-electric when it is pointed, and not when it is flat.

Upon which I observe, that whenever the sphere is excited, the electrical *effluvia* are thereby put into motion, and made to form an atmosphere round about it, from whence, by their repulsive property, they endeavour to expand themselves on all sides equally; but being strongly attracted by the iron, a great part of them are drawn off along the rod, about whose

whose surface they likewise form an atmosphere, which will be denser or rarer, in proportion as the attraction of the rod is greater or less; and as the repulsive power of these *effluvia* will always increase in proportion with their density, it will follow, that whenever the sphere is so strongly excited, that the *effluvia* surrounding it are denser than those surrounding the rod, they will, by their repulsive property, drive the *effluvia* off from the end of it in a stream, and that with a very great velocity; as is evident, from their striking against the hand like a blast of wind when brought near the end of the rod: and as this velocity is partly owing to the attraction of the rod, so this attraction continuing quite to the end of it, the velocity of the particles will there be greatest; and as they approach towards the point, they will be brought nearer together, and therefore become denser there than in any other part of the rod; and therefore if the light is owing to the density and velocity of the *effluvia*, it will be visible at the point, and no-where else.

And that the light is thus produced, will appear, in that whatever increases or diminishes either the velocity or density of the particles will increase or diminish the light. For, let the motion of the wheel which turns the sphere be stopped, the current of the *effluvia* will likewise be stopped, and the rays of light will no longer be seen to issue from the point, and yet the whole rod will continue to be electrical; but, on putting the sphere again into motion, the *effluvia* will become visible as before, and will increase, as the sphere is more strongly excited. Again, the light will be visible to a greater or less distance, as the point is more or less acute; and as this light is always brightest next the point and grows fainter, as the rays diverge, this is plainly owing to the different density of the rays at equal distances; for, when the point is more acute, the rays will diverge less, and therefore will be denser to a greater distance than when it is less acute.

When a non-electric, whose end is flat, is brought within a few inches of the point of the electrified rod, the electric stream will be attracted by it, and the rays made to diverge less than before; and the effect will be the same as if the point was more acute; *viz.* a continuation of the light to a greater distance, and which will be farther increased by the additional velocity the particles will acquire from the attraction of the non-electric. What will follow on a nearer approach of the non-electric to the rod, will be considered under the next experiment.

If the non-electric is pointed and held in the same place as the former, a light will appear from it the same as from the electrical body: for, as the points of the two rods are the parts which approach nearest each other, the attraction there will be strongest: the rays therefore, which diverged from the electrical rod, will be attracted by, and made to converge towards, the point of the non-electrical rod, and will consequently be nearly of the same density at the one as the other; and the velocity being accelerated by the additional attraction, the rays will become luminous at the point of the non-electric, the same as at the point

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