part I : 20 folding plts.
" II : 12 " "

THE

PHILOSOPHICAL TRANSACTIONS

(From the Year 1732, to the Year 1744)

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*.

In TWO VOLUMES,

VIZ.

VOL. VIII. CONTAINING,

PART I. The MATHEMATICAL PAPERS.

PART II. The PHYSIOLOGICAL PAPERS.

VOL. IX. CONTAINING,

PART III. The ANATOMICAL and MEDICAL PAPERS.

PART IV. The HISTORICAL and MISCELLANEOUS PAPERS.

LONDON:

Printed for W. INNYS, C. HITCH, T. ASTLEY in *Pater-noster-Row*, T. WOODWARD, C. DAVIS in *Holbourn*, and R. MANBY and H. S. COX on *Ludgate-Hill*. MDCCXLVII.

TO THE

PRESIDENT,

COUNCIL, and FELLOWS

OF THE

ROYAL SOCIETY

OF

LONDON,

For the improving of

NATURAL KNOWLEDGE,

This Abridgment of the PHILOSOPHICAL TRANSACTIONS is most humbly dedicated by

Chelsey, March 1, 1746-7.

JOHN MARTYN.

TO THE

READER.

AS these Volumes are only a Continuation of thoſe, which were publiſhed twelve Years ago, by the late learned Mr *Eames* and myſelf, there does not ſeem to be any Occaſion for a Preface. The only Alteration now made, is the Addition of the Dates of the ſeveral Papers in the Margin, which I hope will be thought an Improvement. But as I apprehend, that this may ſometimes have been omitted, I ſhall endeavour to ſupply that Defect here, as I did in the former Volumes, by adding the following Table, which will ſhew for what Months each Tranſaction was publiſhed.

VOL.	Nº		
XXXVIII.	427.	*January, February, March,*	
	428.	*April, May, June,*	1733.
	429.	*July, Auguſt, September, October,*	
	430.	*November, December,*	
	431.	*January, February, March,*	
	432.	*April, May, June,*	
	433.	*July, Auguſt,*	1734.
	434.	*September, October, November,*	
	435.	*December,*	
XXXIX.	436.	*January, February, March,*	
	437.	*April, May, June,*	
	438.	*July, Auguſt, September,*	1735.
	439.	*October, November, December,*	

VOL.	Nº		
XXXIX.	440.	*January*, *February*, *March*,	1736.
	441.	*April*, *May*, *June*,	
	442.	*July*, *August*, *September*,	
	443.	*October*,	
	444.	*November*, *December*,	
XL.	445.	*January*, *February*, *March*, *April*, *May*, *June*,	1737.
	446.	*July*, *August*, *September*, *October*, *November*, *December*,	
	447.	*January*, *February*, *March*, *April*, *May*,	1738.
	448.	*June*, *July*,	
	449.	*August*, *September*,	
	450.	*October*, *November*,	
	451.	*December*,	
		Supplement,	
XLI.	452.	*January*, *February*, *March*,	1739.
	453.	*April*, *May*, *June*,	
	454.	*July*, *August*, *September*, *October*,	
	455.	*November*, *December*,	
	456.	*January*, *February*, *March*, *April*, *May*, *June*,	1740.
	457.	*July*, *August*,	
	458.	*September*, *October*, *November*, *December*,	
	459.	*January*, *February*, *March*,	1741.
	460.	*April*, *May*, *June*, *July*,	
	461.	*August*, *September*, *October*, *November*, *December*,	
XLII.	462.	*January*, *February*,	1742.
	463.	*March*, *April*,	
	464.	*May*, *June*, *July*,	
	465.	*October*, Part of *November*,	
	466.	Part of *November*, *December*,	
	467.	*January* 13, 20,	1743.
	468.	From *January* 20 to *February* 3,	
	469.	From *February* 3 to *April* 21,	
	470.	From *April* 21 to *June* 23,	
	471.	*November*, *December*,	

THE

THE
CONTENTS.

VOL. VIII. PART I.

The MATHEMATICAL PAPERS.

CHAP. I.
ALGEBRA, ARITHMETICK, FLUXIONS, GEOMETRY.

CHAP. II.

Opticks.

CHAP. III.

Astronomy.

CHAP. IV.

Surveying.

CHAP. V.

Mechanicks.

2. *Experi-*

CHAP. VI.

Hydraulicks.

CHAP. VII.

Geography and Navigation.

PART II.

The PHYSIOLOGICAL PAPERS.

CHAP. I.

Physiology, Meteorology, Pneumaticks.

XXXVII. *An*

LVIII. *A*

CHAP. II.

HYDROLOGY.

CHAP. III.

MINERALOGY.

CHAP. IV.

MAGNETICS.

CHAP. V.

BOTANY.

XXII.

VOL. IX. PART III.

CONTAINING THE

ANATOMICAL and MEDICAL PAPERS.

CHAP. I.

ZOOLOGY and the ANATOMY of ANIMALS.

CHAP.

CHAP. II.

The STRUCTURE, EXTERNAL PARTS, and COMMON TEGUMENTS of the BODY.

CHAP. III.

The HEAD.

CHAP. IV.

The NECK and THORAX.

CHAP. V.

The ABDOMEN.

2. *Some*

CHAP. VI.

The HUMOURS and GENERAL AFFECTIONS of the BODY.

CHAP. VII.

The Bones, Joints, and Muscles.

CHAP. VIII.

Monsters.

V. *An*

CHAP. IX.

Period of Human Life.

CHAP. X.

Pharmacy and Chymistry.

PART IV.

CONTAINING THE

HISTORICAL and MISCELLANEOUS PAPERS.

CHAP. I.

History and Antiquities.

CHAP. II.

VOYAGES and TRAVELS.

CHAP. III.

MISCELLANEOUS PAPERS.

CHAP. IV.

THE PHILOSOPHICAL TRANSACTIONS

(From the Year 1732, to the Year 1744)

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*.

VOL. VIII. PART I.

CONTAINING THE

MATHEMATICAL PAPERS.

LONDON:

Printed for W. INNYS, C. HITCH, T. ASTLEY, in *Pater-noſter-Row*, T. WOODWARD, C. DAVIS in *Holbourn*, and R. MANBY and H. S. COX on *Ludgate-Hill*. MDCCXLVII.

THE

Philoſophical Tranſactions

ABRIDGED.

PART I.

CONTAINING THE

Mathematical PAPERS.

CHAP. I.

Algebra, Arithmetick, Fluxions, Geometry.

I. I HAVE explained, in the Appendix to *Saunderſon*'s Algebra, my Method of extracting any Root from the Binomial $a+\sqrt{-b}$; the reading of which has cauſed my learned Friend, *W. Jones*, Eſq; *F. R. S.* to deſire me to perform the ſame in the poſſible Binomial $a+\sqrt{+b}$. I ſhall obey his Commands in this particular, though I am very ſenſible, that this has been done already by Sir *I. Newton* and others.

Of the Reduction of Radicals to more ſimple Terms, by Mr A. de Moivre, *F. R. S.* N°. 451, *p.* 463. *Dec.* 1738.

To reduce the Binomial $\sqrt[n]{a+\sqrt{b}}$ *to more Simple Terms.* Prob. I.

Suppoſe that this Binomial, involved with it's general Radicality, can be reduced to the other Binomial, freed from it's general Radicality; now to find each Quantity x and y, try whether the Sum of the Solution.

Binomials $\sqrt[n]{a+\sqrt{b}}+\sqrt[n]{a-\sqrt{b}}$, which may be done by a Table of Logarithms, makes nearly a whole Number. If it be ſo, put $2x$ equal to this whole Number: then ſee whether $\sqrt[n]{aa-b}$ is a whole Number, if it is, put m equal to this new *Integer*, and there will be $y=xx-m$, wherefore the given Binomial will be reduced to the given Form. But before we proceed to the Demonſtration, it will not be improper to illuſtrate the thing by two or three Examples.

Example 1. Let the Binomial $\sqrt[2]{54+\sqrt{980}}$ be reduced to a more ſimple Term.

Put $a=54$, $b=980$; then $\sqrt{b}=\sqrt{980}=31,3049$ nearly, wherefore $a+\sqrt{b}$ will be $=85,3049$, and $a-\sqrt{b}=22,6951$.

The Square Root of the firſt Number is very near $9,236$.

The Square Root of the latter is $4,763$.

The Sum of the Roots is $13,999$, to which the whole Number 14 is very near; therefore put $2x=14$, or $x=7$; now ſince $y=xx-m$, and $m=\sqrt{aa-b}=\sqrt{2916-980}=\sqrt{1936}=44$; therefore we ſhall have $y=49-44=5$, and ſo the Binomial reduced will be $7+\sqrt{5}$.

Example 2. Let $\sqrt[3]{45+\sqrt{1682}}$ be reduced to a more ſimple Term.

Put $a=45$, $b=1682$, therefore $\sqrt{b}=41,01219$ very nearly; therefore we ſhall have $a+\sqrt{b}=86,01219$, and $a-\sqrt{b}=3,8971$.

The Cube Root of the firſt Number is $4,4142$; the Cube Root of the latter is $1,5857$; the Sum of the Roots is $5,99991$, which is very near the whole Number 6; therefore ſay $2x=6$, or $x=3$; but $y=xx-m$; but $m=\sqrt[3]{aa-b}=\sqrt[3]{343}=7$; and ſo $y=9-7=2$; therefore the Binomial reduced is $3+\sqrt{2}$.

Exapmle 3. Let $\sqrt[3]{170+\sqrt{18252}}$ be reduced to a more ſimple Term.

Put $a=170$, $b=18252$, then we ſhall have $\sqrt{b}=135,1$ very nearly; wherefore we ſhall have $a+\sqrt{b}=305,1$, and $a-\sqrt{b}=34,9$.

The Cube Root of the firſt Number is $6,73$ very nearly.

The Cube Root of the latter is $3,26$ very nearly.

The Sum of the Roots is $9,99$, which is very near the whole Number 10; therefore ſay $2x=10$, or $x=5$, we have alſo $y=xx-m$; but $m=\sqrt[3]{aa-b}=22$; therefore $y=25-22=3$; therefore the Binomial reduced is $5+\sqrt{3}$.

Take

Take any Binomial, as $\sqrt[3]{a + \sqrt{b}}$, which suppose reducible to the Binomial $x + \sqrt{y}$; therefore *Demonstration.*

$$x^3 + 3xx\sqrt{y} + 3xy + y\sqrt{y} = a + \sqrt{b};$$

say $x^3 + 3xy = a$,

and $3xx\sqrt{y} + y\sqrt{y} = \sqrt{b}$.

Whatsoever the Index of the Radicality shall be, from the Square of the first Part subtract the Square of the latter; now the Square of the former Part will be

$$x^6 + 6x^4y + 9xxyy = aa;$$

The Square of the latter $9x^4y + 6xxyy + y^3 = b$;

The Remainder will be $x^6 - 3x^4y + 3xxyy - y^3 = aa - b$, extract from both the Root, of which the Index is n, that is, in this Case, the Cube Root; therefore we shall have $xx - y = \sqrt[3]{aa - b}$, or to the *Factum* $\sqrt[3]{aa - b} = m$; we shall have $xx - y = m$; and therefore $y = xx - m$; now in the abovementioned Equation, namely, $x^3 + 3xy = a$, for y say $xx - m$, and you will obtain the Equation $4x^3 - 3mx = a$; here stop a little.

Now resume the Equation $2x = \sqrt[3]{a + \sqrt{b}} + \sqrt[3]{a - \sqrt{b}}$, and suppose you would strike out the Radicality $\sqrt[3]{\ }$;

In order to this, make $a + \sqrt{b} = z^3$,

and $a - \sqrt{b} = v^3$,

you will then have these two new Equations,

$$z^3 + v^3 = 2a$$
$$z + v = 2x$$

It follows therefore that $\dfrac{z^3 + v^3}{z + v} \quad \dfrac{a}{x}$

But $\dfrac{z^3 + v^3}{z + v} = zz - zv + vv$; therefore $zz - zv + vv = \dfrac{a}{x}$; besides $zz + 2zv + vv = 4xx$.

Take the Difference of these Equations, you will have $3zv = 4xx - \dfrac{a}{x}$; but $z^3v^3 = aa - b$; therefore $zv = \sqrt[3]{aa - b}$; but if you say $= m$, then it will be $3m = 4xx - \dfrac{a}{x}$, or $4x^3 - 3mx = a$, which is the very Equation which came out before, and it will return to the same in every Case of Radicality whatsoever.

If therefore you would try whether the Expression $\sqrt[n]{a+\sqrt{b}}$ can be reduced to a more simple Term; say $2x = \sqrt[3]{a+\sqrt{b}} + \sqrt[3]{a-\sqrt{b}}$; say also $\sqrt[n]{aa-b} = m$, and $y = xx - m$; and the Expression reduced will be $x + \sqrt{y}$, if so be these can be done by integral, or at least rational Quantities.

But in case these should not be integral, or rational Quantities, yet the Rule which we have delivered, will be of Use in the Solution of Equations of any Kind, as will hereafter be seen.

In the mean time, this Doubt may perhaps arise, whether this Rule will obtain universally in any Powers whatsoever of a Binomial; for Instance, whether in any Binomial whatsoever, of which the Index is n, if from the Square of the Sum of those Terms, which are in unequal Places, you subtract the Square of the Sum of those which are in equal Places, the Remainder will be another Binomial, of which the Index also will be n.

To this I answer, that it has been observed by many before me, and therefore may be looked upon as confirmed by Experiments; but however, it may not be amiss to produce a Demonstration of it, which I do not remember to have seen any where.

Take the Binomial $\overline{x+y}|^n$ and expand it; take also another Binomial $\overline{x-y}|^n$, which expand in like Manner; say $\overline{x+y}|^n = s$, and $\overline{x-y}|^n = p$; now it will appear at first Sight, that, if the expanded Binomials are joined by Addition, their Sum will be equal to double the Sum of the unequal Terms of the first Binomial; but if the latter be subtracted from the former, that then the Remainder will be equal to double the Sum of the equal Terms of the first Binomial; hence it follows, that $\frac{s+p}{2}$ is the Sum of the unequal Terms; and $\frac{s-p}{2}$ the Sum of the equal Terms.

From the Square of the first Sum, that is, from the Square $\frac{ss+2ps+pp}{4}$ subtract the Square of the latter, namely, $\frac{ss-2ps+pp}{4}$ the Remainder will be $\frac{4ps}{4} = sp = \overline{x+y}|^n \times \overline{x-y}|^n = \overline{xx-yy}|^n$ of which the Root (the Index of which is n) is $= xx - yy$.

If

If you put $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, and take besides $\sqrt[n]{aa - b} = m$, and interpret n successively by 1, 2, 3, 4, 5, 6, 7, 8, *&c.* there will arise the following Equations. *Coroliary.*

1. $x = a.$
2. $2xx - m = a.$
3. $4x^3 - 3mx = a.$
4. $8x^4 - 8mxx + mm = a.$
5. $16x^5 - 20mx^3 + 5mmx = a.$
6. $32x^6 - 48mx^4 + 18mmxx - m^3 = a.$
7. $64x^7 - 112mx^5 + 56mmx^3 - 7m^3x = a$, &c.

Now these Equations are of the same Form as the Equations to the Cosines, though they are naturally quite different.

Let r be the Radius of a Circle, l the Cosine of any given Arch, x the Cosine of another Arch, which may be to the first, as 1 to n.

1. there will be $x = l.$
2. $2xx - rr = rl.$
3. $4x^3 - 3rrx = rrl.$
4. $8x^4 - 8rrxx + r^4 = r^3 l.$
5. $16x^5 - 20rrx^3 + 5r^4 x = r^4 l.$
6. $32x^6 - 48rrx^4 + 18r^4 xx - r^6 = r^5 l.$
7. $64x^7 - 112rrx^5 + 56r^4 x^3 - 7rx = r^6 l$, &c.

But the general Form of these is by putting for the Sake of Brevity $r = 1$

$$2^{n-1} \times x^n - 2^{n-3} \times \frac{n}{1} x^{n-2} + 2^{n-5} \times \frac{n}{1} \cdot \frac{n-3}{2} x^{n-4} - 2^{n-7} \times \frac{n}{1} \cdot \frac{n-4}{2} \cdot \frac{n-5}{3} x^{n-6} + 2^{n-9} \times \frac{n}{1} \cdot \frac{n-5}{2} \cdot \frac{n-6}{3} \cdot \frac{n-7}{4} \times x^{n-8} \ \&c. = l.$$

The Difference of these Equations consists chiefly in this, that the first are derived from the Equation $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, but the latter from the Equation $2x = \sqrt[n]{a + \sqrt{-b}} + \sqrt[n]{a - \sqrt{-b}}$, and if this latter Equation be freed from it's general Radicality, we shall obtain Equations to the Cosines.

Let there be therefore the Equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt{a - \sqrt{-b}}$, which must be freed from it's radical Sign $\sqrt[3]{}$.

Say

Say $\sqrt[3]{a + \sqrt{-b}} = z$, and $\sqrt[3]{a - \sqrt{-b}} = v$; say also $z + v = 2x$. Hence you will have

1. $z^3 = a + \sqrt{-b}$
2. $v^3 = a - \sqrt{-b}$

hence it will be $z^3 + v^3 = 2a$.

But $z + v = 2x$, therefore it will be $\frac{z^3 + v^3}{z + v} \quad \frac{a}{x}$;

But $\frac{z^3 + v^3}{z + v} = zz - zv + vv$; wherefore $zz - zv + vv$ will be $= \frac{a}{x}$.

But $zz + 2zv + vv = 4xx$; whence $3zv = 4xx - \frac{a}{x}$; but now $z^3 v^3 = aa + b$.

Therefore it follows, that zv is $= \sqrt[3]{aa + b}$; which if you make $= m$, therefore $4xx - \frac{a}{x}$ will be $= 3m$, or $4x^3 - 3mx = a$.

Hitherto we have had two Kinds of Equations; the first in which m was put $= \sqrt[3]{aa - b}$; the latter, in which it was $= \sqrt[3]{aa + b}$. Let us call the first Hyperbolical, the latter Circular.

Prob. II. *To extract the Cube Root from an impossible Binomial,* $a + \sqrt{-b}$.

Solution. Suppose that Root to be $x + \sqrt{-y}$, of which if you take the Cube, you will find it to be $x^3 + 3xx\sqrt{-y} - 3xy - y\sqrt{-y}$.

Now put $x^3 - 3xy = a$.
and $3xx\sqrt{-y} - y\sqrt{-y} = \sqrt{-b}$.

Then by taking the Squares there will arise two other Equations;

$x^6 - 6x^4y + 9xxyy = aa$
$-9x^4y + 6xxyy - y^3 = -b$.

Now take the Difference of the Squares, there will be $x^6 + 3x^4y + 3xxyy + y^3 = aa + b$; wherefore $xx + y$ is $= \sqrt[3]{aa + b}$: now say $\sqrt[3]{aa + b} = m$, whence $xx + y$ will be $= m$, or $y = m - xx$; now in the Equation $x^3 - xy = a$, instead of the Quantity y, substitute it's Value $m - xx$, you will have $x^3 - 3mx + 3x^3 = a$, or $4x^3 - 3mx$ $= a$,

$= a$, which is the very Equation, which had before been deduced from the Equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt[3]{a - \sqrt{-b}}$; but it does not follow, that in the Equation $4x^3 - 3mx = a$, the Value of the Quantity x may be known by the former Equation, as it consists of two Parts, each of which includes the imaginary Quantity $\sqrt{-b}$; but this will be best done by the help of a Table of Sines.

Therefore let the Cube Root be to be extracted from the Binomial $81 - \sqrt{-2700}$; say $a = 81$, $b = 2700$; now $aa + b = 6561 + 2700 = 9261$, of which the Cube Root $= 21$, which suppose $= m$, so that $3mx$ may be $= 63x$; the Equation therefore to be resolved, will be $4x^3 - 63x = 81$, and if this is compared with the Equation to the Cosines, namely, $4x^3 - 3rrx = rrl$; rr will be $= 21$; and therefore r will be $= \sqrt{21}$; and moreover, l will be $= \frac{a}{rr} = \frac{81}{21} = \frac{27}{7}$.

Therefore let there be an Arc of a Circle, of which the Radius is $= \sqrt{21}$, and the Cosine $= \frac{27}{7}$.

Let the whole Circumference be C, take the Arches $\frac{A}{3}$, $\frac{C - A}{3}$, $\frac{C + A}{3}$, which will easily be known by a Trigonometrical Calculation, especially if you make use of Logarithms, then the Cosines of the Arcs to the Radius $\sqrt{21}$, will be three Roots of the Quantity x; wherefore since y is $= m - xx$, they will therefore be so many Values of the Quantity y, and so the Cube Root will be triple of the Binomial $81 + \sqrt{-2700}$, but let us accommodate it to Numbers.

Say as $\sqrt{21}$ to $\frac{27}{7}$, so Radius of the Tables to Cosine of any Arc, to which Arc, suppose A to be equal; but that Arc will be found near 23°, 42′; hence the Arc C—A will be 327°, 18′, and C + A 392°, 42′, the third Parts of which will be 10°, 54′; 109°, 6′; 13°, 54′; but now as the first of them is less than a Quadrant, it's Cosine, that is, the Sine 79°, 6′, ought to be looked upon as positive; as both the others are greater than a Quadrant, that is, the Sines of the Arcs 19°, 6′; 40°, 54′, ought to be looked upon as negative; but by the Trigonometrical Calculation, it will appear that these Sines to Radius $\sqrt{21}$ will be 4,4999, — 1,4999, — 3,0000, or $\frac{9}{2}$ — $\frac{3}{2}$, — 3; whence there will be so many Values of the Quantity y, namely all those which

which $m - xx$ represents, that is, $21 - \frac{81}{4}$, $21 - \frac{9}{4}$, $21 - 9 = \frac{3}{4}$, $\frac{75}{4}$, 12, of which the Square Roots are $\frac{1}{2}\sqrt{3}$, $\frac{1}{2}\sqrt{3}$, $2\sqrt{3}$; wherefore 3 Values of the Quantity $\sqrt{-y}$ will be $\frac{1}{2}\sqrt{-3}$, $\frac{1}{2}\sqrt{-3}$, $2\sqrt{-3}$; whence the three Values of the Quantity $\sqrt[3]{81+\sqrt{-2700}}$ will be $\frac{1}{2}+\frac{1}{2}\sqrt{-3}$, $-\frac{3}{2}+\frac{5}{2}\sqrt{-3}$, $-3+\frac{1}{2}\sqrt{-3}$, and after the same Manner of proceeding will be found three Values of the Quantity $\sqrt[3]{81-\sqrt{-2700}}$, namely these $\frac{9}{2}-\frac{1}{2}\sqrt{-3}$, $-\frac{3}{2}-\frac{5}{2}\sqrt{-3}$, $-3-\frac{1}{2}\sqrt{-3}$.

There have been many, among whom was the famous *Wallis*, who have thought that those cubic Equations, which are referred to a Circle, may be solved by the Extraction of a Cube Root from an imaginary Quantity, such as, $81+\sqrt{-2700}$, without having any Regard to the Table of Sines; but let them say what they will, it is all a mere Fiction, and begging of the Question; for if you attempt it, you must necessarily run back to that Equation which you had taken to solve. But this cannot be done directly, without the help of a Table of Sines, especially if the Roots are irrational; and this has been observed by many before me.

Prob. III. *To extract a Root, of which the Index is n, from an impossible Binomial* $a+\sqrt{-b}$.

Solution. Let the Root be $x+\sqrt{-y}$, then having made $\sqrt[n]{aa+b}=m$; also $\frac{n-1}{n}=p$, describe, or suppose to be described, a Circle, of which the Radius is $\sqrt{m}$, and therein take any Arc A, of which let the Cosine be $\frac{a}{m^p}$; let C be the whole Circumference. Take to the same Radius, Cosines of the Arcs $\frac{A}{n}$, $\frac{C-A}{n}$, $\frac{C+A}{n}$, $\frac{2C-A}{n}$, $\frac{3C-A}{n}$, $\frac{3C+A}{n}$, &c. till the Multitude of them is equal to the Number n; which being done, stop there; then all those Cosines will be so many

many Values of the Quantity x; as for the Quantity y, that will always be $m - xx$.

I muſt not omit, though it has been mentioned already, that thoſe Coſines muſt be reckoned affirmative, of which the Arcs are leſs than a Quadrant, and thoſe negative, the Arcs of which are greater than a Quadrant.

Any Equation of the Kind of thoſe mentioned above being given, to know whether the Solution of it is to be referred to the Hyperbolical, or to the Circular Species. Prob. IV.

Let n be the higheſt Dimenſion of the Equation; divide the Coefficient of the ſecond Term by $2^{n-3} \times n$, and let the Quotient be $= m$; now ſee whether the Square aa be greater or leſs than the Power m; if the former Caſe ſhall happen, the Equation is to be referred to the Hyperbola; if the latter, to the Circle. Solution.

Let the Equation $16x^5 - 40x^3 + 20x = 7$ be given, where $n=5$, therefore $2^{n-3} \times n = 20$: Divide 40 by 20, the Quotient is $2 = m$, moreover $m^n = 32$, and the Square $aa = 49$; and as this is greater than the Power 32, the Conſequence is, that the Equation is to be referred to the Hyperbolical Species; but as in the Hyperbolical Caſe it was put $\sqrt[5]{aa - b} = m$, it follows, that $aa - b = m^5 = 32$, and ſo $b = aa - 32 = 49 - 32 = 17$: But now the Root of the Equation in this Caſe is $\frac{1}{2}\sqrt[5]{7 + \sqrt{17}} + \frac{1}{2}\sqrt[5]{7 - \sqrt{17}}$; but $\sqrt{17} = 4{,}123105$ nearly, therefore $7 + \sqrt{17} = 11{,}123105$, and $7 - \sqrt{17} = 2{,}876895$; moreover, the fifth Root of the former Number will be found $= 1{,}6221$, the fifth Root of the latter $= 1{,}2353$, the Sum of the Roots $= 2{,}8574$, the half Sum $1{,}4287$ is the Value of the Quantity x in the given Equation.

Now let the Equation $16x^5 - 40x^3 + 20x - 5$ be given; in which m is $= 2$, but $a = 5$; it is plain that the Square aa is leſs than the fifth Power of the Number 2; wherefore the Value of the unknown x cannot be obtained without the Quinqueſection of an Angle; and this is performed by the Help of our general Theorem, mentioned before, by taking to the Radius $\sqrt{2}$, the Arc of which the Coſine is $\frac{a}{m^p} = \frac{a}{4} = \frac{5}{4}$, and that Arc will be found $27°, 55'$, nearly, of which the fifth Part is $5° \ 35'$; now if you take the Logarithm of that Coſine of the Arc to Radius 1, you will find it to be $9{,}9979347$; but ſince our Radius ought to be $\sqrt{2}$, add to the former Logarithm the Logarithm $\sqrt{2}$, that is $0{,}1515150$, the Sum will be $10{,}1484497$, out of

which

which if you take 10, the Remainder, namely, 0,1484497, will be the Logarithm of the Number sought, which will therefore be 1,4075 very nearly, and in the fame Manner the other four Roots may be found.

Some few things remain to be obferved, which I fhall add in this Place.

If the Equation is of the Hyperbolical Kind, and befides n is an odd Number, there will be only one poffible Root, the reft will be impoffible; but if n is an even Number, there will be only one Value of the Square xx, the reft are impoffible.

If the Equation is of the circular Kind, all the Roots will be poffible.

In order to know how many affirmative Roots there will be, and how many Negative, in Equations to Cofines, let this Rule be obferved.

If n is an even Number, there will be as many affirmative Roots as negative.

If n is an odd Number, but fuch that $\frac{n+1}{2}$ is an even Number, the Number of Affirmatives will be $\frac{n-1}{2}$, the Number of Negatives $\frac{n+1}{2}$.

But if $\frac{n+1}{2}$ is an odd Number, the Number of Affirmatives will be $\frac{n+1}{2}$, of Negatives $\frac{n-1}{2}$.

A Demonftration of Newton's *Method of raifing any Polynomial to any Power, by Means of an affumed Binomial, by* J. Caftillioni, N°. 464. *Read* May 6, 1742.

II. Every Index is either Integer or Fraction; and thefe are either pofitive or negative. 1. If the Index is an Integer and pofitive, then to raife the Binomial to a Power, of which the Index is m, is nothing but writing the given Binomial, as often over as there are Units in m, and to draw all thefe Binomials in their Turns.

2. If the Index is a Fraction and pofitive, to raife the Binomial to the Power $\frac{r}{n}$ is to raife the given Binomial to the Power r, and, this Power being given, to feek the Quantity, which being given to the Power n, equals the Power of the given Binomial r.

3. But when the Index is negative, whether it is an Index or a Fraction, to raife the Binomial, we muft do as in N°. 1, or 2, and then Unity is to be divided by the Power found.

I take

I take a Binomial $p+q$, that it may shew me any Polynomial.

Between p^m and q^m there are as many Geometrical Means, in the Ratio $p . q$, as there are Units in $m-1$.

Being to find these Terms, I note that p^m is to q^m in a compound Ratio of $p^m . 1$, and $1 . q^m$, also p to q has a Ratio compounded of $p . 1$, and of $1 . q$; but if there are two Serieses of Powers, in one of which, the Indices of the Power p decrease in the same arithmetical Proportion, of which the Difference is 1, by which the *Indices* of q increase in the second Series, there will be had a Series of continual Proportionals in the Ratio $p . 1$, and $1 . q$.

$$\text{So } p . 1 :: p^m . p^{m-1} . p^{m-2} . p^{m-3} . p^{m-4} \ldots\ldots p^{m-m} = p^0 = 1$$

$$1 . q :: 1 . q . \quad q^2 . \quad q^3 . \quad q^4 \ldots\ldots\ldots \quad q^m .$$

Therefore the corresponding Terms being multiplied in their Turns,

$$p . q :: p^m . p^{m-1} q . \quad p^{m-2} q^2 . p^{m-3} . p^{m-4} q^4 \ldots q^m .$$

Now I say, that $\overline{p+q}|^m$ is composed of the Terms found above, as is easily proved by their Generation.

Therefore all the Terms which are in $\overline{p+q}|^m$ disposed in Order, are in continual Proportion. And indeed any two Numbers following each other immediately, are as the first Term of a Binomial Root to the second. This appears by the Generation, for p any Number of Times multiplied, is to q as many Times drawn into p as $p . q$.

Therefore the Number of all is $m+1$; but also in the decreasing arithmetical Series $m . m-1 . m-2 \ldots\ldots 0$ — the Terms are in Number $m+1$, or in the increasing $0 . 1 . 2 . 3 \ldots\ldots m$; therefore the component Terms $\overline{p+q}|^m$ ought to have these Indices, or to be $p^m , p^{m-1} q \ldots\ldots q^m$.

But by the Laws of Multiplication, the Number of the Terms ought to be $2^m > m+1$, therefore in this Factum some repeated Terms must be found.

The common *Facta* (namely those of which the Multiplicator and Multiplicand consist of different Quantities) contain all the different Terms, because they are all formed of different Factors. In Powers therefore, it must be seen what Terms were different, unless the Factors were always the same, and how many of the different ones are made equal by the Restitution of Letters; for so we shall find how often every one ought to be repeated in the Power.

Now it appears, that if the Factors were always different, all the Terms also in the Product would be different.

But when the first in the Product is made only of the first of the Multiplicators, and the last of them is made of the last, these *Facta* will

 always

always be different, though the making Binomials are the ſame, becauſe the firſt Term of the Binomial differs from the ſecond.

But of the reſt, ſome may be made equal, becauſe they are compoſed of the firſt of the Efficients multiplied into the ſecond, and joined in different Manners.

It muſt therefore be enquired, after how many different Manners the Quantities, of which the Number is given, may be joined.

In our Caſe, the Index of the Things is m, the different Things two, of which one is repeated s Times, the other t, ſo that $s+t=m$; therefore the Number of the Changes will be

$$\frac{m\,.\,m-1\,.\,m-2\,.\,m-3\,.\,.\,.\,.\,.\,.\,.\,1}{s\,.\,s-1\,.\,s-2\,.\,.\,.\,.\,1\,.\,t\,.\,t-1\,.\,t-2\,.\,t-3\,.\,.\,.\,1}$$

So let $t=1$, $s=m-1$, the Term will be $p^{m-1}q$, and it's Coefficient $\frac{m\,m-1\,.\,m-2\,.\,m-3\,.\,.\,.\,.\,1}{m-1\,.\,m-2\,.\,m-3\,.\,.\,.\,.\,.\,1}=m.$

Let $t=3$, $s=m-3$; it's Coefficient will be had $p^{m-3}q^3$, $=\frac{m\,.\,m-1\,.\,m-2\,.\,m-3\,.\,m-4\,.\,.\,.\,.\,.\,.\,1}{1\,.\,2\,.\,3\,.\,m-3\,.\,m-4\,.\,m-5\,.\,.\,.\,.\,.\,.\,.\,.\,1}=\frac{m\,.\,m-1\,.\,m-2}{1\,.\,2\,.\,3.}$, and ſo of the reſt.

If any one ſhall doubt, whether the former Demonſtration will prove that all the Terms are neceſſarily formed after ſo many Manners, as they may, and ſhall contend that it only ſhews that it may happen, I ſhall anſwer thus.

Certainly $\overline{p+q}|^m=\overline{p+q}\times\overline{p+q}|^{m-1}$; but amongſt the Terms of this are $p^{m-n-1}q^n$ which will neceſſarily be multiplied into p and q, and $p^{m-n-1}q^n\times p=p^{m-n}q^n=p^{m-n}q^{n-1}\times q$, therefore $p^{m-n}q^n$ by all poſſible Ways will be made into $\overline{p+q}|^m$, if $p^{m-n-1}q^n$ and $p^{m-n}q^{n-1}$ are generated as many Ways as poſſible into $\overline{p+q}|^{m-1}$; which will neceſſarily be, if $p^{m-n-2}q^n$, and $p^{m-n}q^{n-2}$ are in the lower Power $\overline{p+q}|^{m-2}$, and ſo on always to the Square in which pp, pq, and qq are had, formed after as many Ways as poſſible, (4. II. *Euclid.*) therefore alſo in the former.

This reaſoning requires, that I ſhould ſhew the ſame alſo after a Manner ſomething different.

We have now ſhewn that the Coefficient of the firſt is Unity.

The

The ſecond Term $p^{m-1}q$ is formed of $p^{m-2}q \times p$, and $p^{m-1} \times q$, that is, of the firſt of the Roots multiplied into the ſecond of $\overline{p+q}|^{m-1}$, and of the ſecond of the Root into the firſt of $\overline{p+q}|^{m-1}$, therefore in $\overline{p+q}|^{m}$ there is $p^{m-1}q$ once more as often as the ſecond is in $\overline{p+q}|^{m-1}$ which is there once more as often as the ſecond in $\overline{p+q}|^{m-2}$ which again is there once more as often as the ſecond is in $\overline{p+q}|^{m-2}$ and ſo always till you come to $\overline{p+q}|^{m-m}$, where the ſecond is once; therefore you muſt ſeek the Sum of as many Units as there are in m, which is m.

Alſo the third $p^{m-2}qq$ is formed of $p^{m-3}qq \times p$, the third of $\overline{p+q}|^{m-1}$ into the firſt of the Root, and of $p^{m-2}q \times q$ the ſecond of $\overline{p+q}|^{m-1}$ into the ſecond of the Root; therefore $\overline{p+q}|^{m}$ will contain $p^{m-2}qq$ as often as the ſecond is contained in $\overline{p+q}|^{m-1}$, that is $m-1$ times more, as often as the third is there, that is, as often as the ſecond is in $\overline{p+q}|^{m-2}$ $(m-2)$ more than the third is there, which again is as often as the ſecond is in $\overline{p+q}|^{m-3}$ $(m-3)$ more than the third is there, and ſo on till we come to $\overline{p+q}|^{2}$ where the third is once, or to $p+q$, where there is no third; for we muſt always ſeek the ſum of the arithmetical Progreſſion $m-1 . m-2 . m-3 \ldots\ldots 1$, or $m-1 . m-2 \ldots\ldots 0$, in the former the Number of the Terms is $m-1$, in the latter m, as is manifeſt; wherefore this Sum $= \overline{m-1+1} \times \frac{m-1}{2} = m \times \frac{m-1}{2} = \overline{m-1+0} \times \frac{m}{2}$.

By the ſame Means, the Coefficients of the other Terms will be proved to make the Series, in which the ſecond Differences are in arithmetical Progreſſion, *&c.*

Whence always, where m is an Integer and poſitive, the Formula will be $p^{m} + mp^{m-1}q + \frac{m . m-1}{2}p^{m-2}qq + \frac{m . m-1 . m-2}{2 . 3}p^{m-3}q^{3} + \frac{m . m-1 . m-2 . m-3}{2 . 3 . 4}p^{m-4}q^{4} + \frac{m . m-1 . m-2 . m-3 . m-4}{2 . 3 . 4 . 5}p^{m-5}q^{5}$, *&c.*

If we make $p+q=p\times\overline{1+\frac{q}{p}}$ there will arise the very *Formula* of Sir *Isaac Newton*; for $\overline{p+q}|^{m}=p^{m}\times\overline{1+\frac{q}{p}}|^{m}=p^{m}\times\overline{1+\frac{m}{1}\times\frac{p}{q}+\frac{m.\overline{m-1}.}{1.2}\times\frac{p^{2}}{q^{2}}+\frac{m.\overline{m-1}.\overline{m-2}}{1.2.3}\times\frac{p^{3}}{q^{3}}}$, &c. $=$ (if A, B, C, D, *&c.* are supposed to equal the first, second, third, fourth, *&c.* each with their Coefficients) $p^{m}\times\overline{1+mA\frac{q}{p}+\frac{m-1}{2}B\frac{q}{p}+\frac{m-2}{3}C\frac{q}{p}+\frac{m-3}{4}D\frac{q}{p}+\frac{m-4}{5}E\frac{q}{p}+\frac{m-5}{6}F\frac{q}{p}}$ &c.

Let us now seek the *Formula*, or raising the same Binomial to the Power $\frac{r}{n}$, where r and n are whole Numbers, and both either positive or negative.

Now $p.q::p^{\frac{r}{n}}.x=\frac{p^{\frac{r}{n}}q}{p}=p^{\frac{r}{n}-1}q$, wherefore the Terms will be $p^{\frac{r}{n}}.p^{\frac{r}{n}-1}q.p^{\frac{r}{n}-2}qq.p^{\frac{r}{n}-3}q^{3}$, *&c.*

The Coefficients to be found are A, B, C, D, E, so that the whole $\overline{p+q}|^{\frac{r}{n}}$ Root $=Ap^{\frac{r}{n}}+Bp^{\frac{r}{n}-1}q+Cp^{\frac{r}{n}-2}qq+Dp^{\frac{r}{n}-3}q^{3}+Ep^{\frac{r}{n}-4}q^{4}$, *&c.* therefore $\overline{p+q}|^{r}$ $(p^{r}+rp^{r-1}q+\frac{r.\overline{r-1}}{2}p^{r-2}qq+\frac{r.\overline{r-1}.\overline{r-2}}{2.3}p^{r-3}q^{3}$, *&c.*$)=\overline{Ap^{\frac{r}{n}}+Bp^{\frac{r}{n}-1}q+Cp^{\frac{r}{n}-2}qq,\ \&c.}|^{r}=A^{n}p^{r}+nA^{n-1}Bp^{r-1}q+nA^{n-1}Cp^{r-2}qq+nA^{n-1}Dp^{r-3}q^{2}+nA^{n-1}Ep^{r-4}q^{4}$, &c. $+\frac{n.\overline{n-1}}{2}A^{n-2}B^{2}p^{r-2}qq+n.\overline{n-1}A^{n-2}BCp^{r-3}q^{3}+n.\overline{n-1}A^{n-2}BDp^{r-4}q^{4}$, *&c.* $+\frac{n.\overline{n-1}.\overline{n-2}}{2.3}A^{n-3}B^{3}p^{r-3}q^{3}+\frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}}{2.3.4}A^{n-4}B^{4}p^{r-4}q^{4}$. And therefore

therefore the Terms being compared $1 = A^{n} = A^{n-1} = A^{n-2}$, &c. $nB = r$, and $B = \frac{r}{n}$, $nC + \frac{n.n-2}{2} \times \frac{rr}{nn} = \frac{r.r-1}{2}$, and C $= \frac{r.r-n}{2.nn}$, $nD + n.n-1 \times \frac{r}{n} \times \frac{r.r-n}{2nn} + \frac{n.n-1.n-2}{2.3} \times \frac{r^3}{n^3} = \frac{r.r-1.r-2}{2.3}$, and $D = \frac{r.r-n.r-2n}{2.3.n^3}$, &c.

If therefore we make $\frac{r}{n} = m$, and the first Term A, &c. the first *Formula* will revive, and $\overline{p+q}|^{\frac{r}{1}} = \overline{p+q}|^{m} = p^{m} \times \overline{1 + mA\frac{q}{p} + \frac{m-1}{2}B\frac{p}{q} + \frac{m-2}{3}C\frac{q}{p}}$, &c.

Let the Binomial $p+q$ be to be raised to the negative Power, either perfect or imperfect, $-s$.

Now $\overline{p+q}|^{-1} = \frac{1}{\overline{p+q}|^{1}} = \overline{p^{s} + sp^{s-1}q + \frac{s.s-1}{2}p^{1-2}qq}$, &c. $=$ (by Division) $\frac{1}{p} - \frac{sp^{s-1}q}{p^{2s}} - \frac{s.s-1}{2} \times \frac{p^{s-2}qq}{p^{2s}} - \frac{s.s-1.s-2}{2.3} \times \frac{p^{s-3}q^{3}}{p^{2s}} - \frac{s.s-1.s-2.s-3}{2.3.4} \times \frac{p^{s-4}q^{4}}{p^{2s}}$

$= p^{-s} - sp^{-s-1}q - \frac{s.s-1}{2}p^{-s-2}qq.$

From this *Formula*, by insisting on the former Methods, is easily drawn the usual and most general $p^{m} \times 1 + mA\frac{q}{p} + \frac{m-1}{2}B\frac{q}{p}$,&c.

I do not think it an unpleasant thing, that in this *Formula*, if $m = -2$, the coefficient Numbers will be natural, if $m = 3$, Triangular; and Pyramidal, if $m = 4$, &c.

But it is plain, that this *Formula* always gives an infinite Series; for (if m expounds a positive Number) the last Term ought to be q^{-m}; but

but $p \,.\, q :: p^{-m} \; p^{-m-1} :: p^{-m-1} \; q \,.\, p^{-m-2} \; qq$, &c. therefore the Ratio of $p^{-m} \,.\, q^{-m}$ ought to be compounded of some Ratios $p \,.\, q$. which cannot be done, because $p^{-m} \,.\, q^{-m} :: \frac{1}{p^m} \,.\, \frac{1}{q^m} :: q^m \,.\, p^m$ in a Ratio compounded of the Reciprocals of $p \,.\, q$.

The Indices of p make an arithmetical Progression, of which the Terms $-m$, $-m-1$, $-m-2$, *&c.* are negative indeed, but increase or decrease from 3; but the last Term ought to be $q^{-m} = p \; q^{-m}$, therefore it will never come to it.

Description and Use of an Arithmetical Machine, invented by Christian-Ludovicus Gersten, *F. R. S. Prof. Math. at* Giessen. No. 438. p. 79. *July, &c.* 1735. *Fig.* 1, 2, 3, 4, 5.

III. Sir *Samuel Morland* was, for ought I know, the first who undertook to perform Arithmetical Operations by Wheel-work. To this end he invented two different Machines, one for *Addition* and *Substraction*, the other for *Multiplication*, which he published in *London*, in the Year 1673, in *small Twelves*. He gives no more than the outward Figure of the Machines, and shews the Method of working them. But as by this every one, who has any Skill in Mechanicks, will be able to guess, how the inward Parts ought to be contrived; so it cannot be denied, that these are two different Machines, independent of one another; that the last, which is for *Multiplication*, is nothing else but an Application of the *Nepairian* Bones on flat moveable Disks; consequently that his Invention alone is not fit to perform justly all Arithmetical Operations.

After him the celebrated Baron *de Leibnitz*, the Marquis *Poleni*, and Mr *Leupold* took this Undertaking in hand, and attempted to perform it after different Methods.

The first published his Scheme in the Year 1709, in the *Miscellanea Berolinensia*, but then he gave only the outward Figure of the Machine. S. *Poleni* communicated his, but explaining at the same time it's inward Construction, in his *Miscellanea* of the same Year, 1709. Mr *Leupold's* Machine, together with those of Mr *de Leibnitz* and S. *Poleni*, were inserted in his *Theatrum Arithmetico-Geometricum*, published at *Leipzig* in 1727, after the Author's Death, yet imperfect, as it is owned in the Book itself.

Besides these, I learned from several *French* Journals, that Monsieur *Pascal* invented one, which however I never had the sight of. *

I took the Hint of mine from that of Mr *de Leibnitz*, which put me upon thinking how the inward Structure might be contrived: But as it was

* The Description of this Machine is since printed by Monf. *Gallen*, in his Collection of Machines and Inventions approved by the *Academy of Sciences*, at *Paris*, (published in *French* at *Paris*. 1735, in Quarto, in six Tomes) in Tom. IV. p. 137; and likewise another by Monf. *Lespine*, Tom. IV. p. 131; and three more by Monf. *Hellerin de Boistissandeau*, Tom. V. p. 103, 117, and 121.

was not poſſible for me to hit upon the original Ideas of that Great Man, an exact Enquiry into the Nature of Arithmetical Operations furniſhed me at laſt with others, which I expreſſed in a rough Model of Wood, and ſhewed to ſome Patrons and Friends, who encouraged me to have another made of Braſs: But the want of an Artificer, able enough to execute my Ideas, made me delay it till the Year 1725; when having ſpare Time, and finding an Inclination to divert myſelf with Mechanical Operations, I ſet about it, and finiſhed the whole Work fitted to a Reckoning not exceeding ſeven Places. And in *Dec.* of the ſame Year, I had the Honour to lay this Machine before the preſent Landgrave of *Heſſen Darmſtadt*, and the Hereditary Prince his Son, to whom I demonſtrated the Mechaniſm of the whole Invention.

I was checked from publiſhing it at that time by the Uncertainty I was under, whether poſſibly Mr *Leibnitz*'s Machine had not been brought to it's Perfection; in which caſe there is no doubt but the Operation of his Machine, if it would really perform what is promiſed in the Deſcription, would have been eaſier than mine, and conſequently preferable to it, provided it's Structure did not prove too intricate, nor that the working of it took up too much time.

But at preſent, being certain that none of Mr *Leibnitz*'s Invention has yet appeared in ſuch a State of Perfection, as to have anſwer'd the Effect propoſed, and that theſe of mine differ from all thoſe mentioned above, fancying at the ſame time, that Perſons who underſtand Mechanicks, will find it plain, practicable, and exact, in regard to it's various Effects, I make no Scruple to preſent this Invention to the *Royal Society*.

The Particulars of it are as follows:

There are as many Sets of Wheels and moveable Rulers as there are Places in the Numbers to be calculated. *Fig.* 1. ſhews three of them. Fig. 1. by which one may eaſily conceive the reſt. A A ſhews the firſt Syſtem or the Figures of Unites, according to it's inward Structure. BB and CC ſhews the ſecond and third Syſtem, *viz.* of Tens and Hundreds, accord- to their outward Form. We ſhall firſt conſider A A; where $\alpha\,\alpha\,\alpha$ is a flat Bottom of a Braſs Plate, which may be ſkrewed on either upon a particular Iron Frame, or only upon a ſtrong Piece of Walnut-Tree, doubled with the Grain croſs'd. In this Syſtem are two moveable Rules *gggg*, and *kkk*, the firſt of which I call the Operator, the ſecond the Determinator. There are beſides two Wheel-Works, the upper one is for *Addition* and *Subſtraction*, the lower one ſerves for *Multiplication* and *Diviſion*. The upper one is provided firſt with *a*, an oblique Ratchet-Wheel of 10 Teeth, of what Diameter you pleaſe, on which, however, depends the Length and Breadth of the Syſtem itſelf. This Wheel has a Stop *r*, with a depreſſing Spring *t*; Under the Wheel *a* is a ſmaller Wheel *b* of the ſame Shape: Both *a* and *b* are rivetted together, and fixed on a common Axis. Under the Wheel *b* lies a third *f*, which is a common Tooth-Wheel of 20 or more Teeth, according as one pleaſes: It is larger than *b*, and ſmaller than *a*, turns about the ſame Axis with

 the

the other two above it, and upon it is fixed a Stop *c*, with the Spring *d*, which catches the oblique Teeth of the Wheel *b*. Immediately under this Wheel lies the upper Part of the Operator, which may be best made of Iron or Steel. The Wheels may all be of Brass, except the upper one. The Operator is of the same Thickness all over, and in it's upper Part are fixed as many round Steel Pins as there are Teeth in the Wheel *f*, which are to catch the Teeth of this Wheel, and move it backwards and forwards. The Height of those Pins ought exactly to answer the Thickness of the Wheel *f*. The Axis of the Wheels *a* and *b* is kept perpendicularly by the Bridge *ee*, which is skrewed to the Bottom, as appears by the Figure. The Operator *gggg* moves on the Side, above, and in the Middle in two Brass Grooves *i i i* and *q q*; about D it jets out, on which Projection a Piece of Iron *h* must be well fastened, having a strong Pin, on which the Handle *z* fits as you see in the System B B. The Side D itself slides in another Groove *s s*, and in it's inner Corner joins to it the Determinator *k k k*, of the same Thickness with the Operator, the Shape of which is sufficiently expressed in the Figure. This slides also up and down, on the one Side in the Groove *s s*, and *v* on the other Side, where it is smallest, in a small Piece of Brass 4, and where it is broadest, above in the Operator itself, which is either hollowed out into another Groove, or filed off obliquely. The sliding part of the Determinator ought afterwards also to be fitted to it. It's chief Part is the Lock *u*, standing perpendicular on it's broad Part. I have drawn it separately in *Fig* 4. B B, in which the sliding Stop *c*, is pressed down by it's Spring *d*, but raised by the Tricker *a a*. That Tricker has a pin *b*, on which is skrewed on the small Handle *l l* (*Fig.* 1. in the Systems B B and C C.) In the Brass Bottom A A (or *α α Fig.* 1.) you must file out 10 Ratchet-Teeth or Kerfs, purposely for the Stop of this Lock, or, which is better, you may insert into the Brass Bottom a small Piece of Iron filed out according to this Figure. The Partition and Length of these Rachet-Teeth in the Bottom must fit exactly with the Circumference of the Wheel *f*, (*Fig.* 1. *System* A A,) with this Direction, that if the Lock is kept by the uppermost Tooth in the Bottom, the Operator cannot be moved at all; but when by pressing down the Tricker *aa*, (*Fig.* 4.) the Determinator is shoved down, and is stopp'd by the second or third Tooth in the Bottom, the Operator being also drawn down as far as the Determinator permits, makes the Stop *c*, (*Fig.* 1. *Syst.* A A) slide over 1 or 2 Teeth of the second Wheel *b*; consequently the same Stop *c*, must slide over 9 Teeth, when the Lock of the Determinator will stand before the 10th Tooth in the Bottom, and the Operator is pulled down so far. If you have a mind to apply these Rachet-Teeth on the Outside of the Plate ⊙ ⊙, that covers the Whole, you may fit the Lock to it accordingly: But in this Case the Covering-Plate must be well fastened.

Fig. 4.

For *Multiplication* and *Division*, there is properly in each System but one Wheel, likewise divided into 10 Rachet-Teeth, on which is rivetted the round Plate *h*, on which are engraved the Numbers or Figures: These

Wheels

Wheels have no occasion for any Bridge, but may turn about a strong Pin of Steel, solder'd to the Bottom. The Rachet Wheel *m m* rests on one Side upon the Determinator, and upon a Piece of Brass of the same Thickness, to which are fastened the Stop *n*, and the Spring *p*. Upon the Operator is another Stop *o*, with it's Spring; which Stop has a small Arm at *o*, which is checked by a small Studd, to hinder the Spring's pressing the Stop lower down than it ought: By which Contrivance it is so order'd, that after the Operator is slid down so far as it can go, in being slid up again, the Stop *o* will turn but one Tooth of the Wheel *m m*. The round Plate *l* has in it's Middle a small hollow Axis, on which are turned first two Shoulders, and then a Skrew: This Skrew in the System A A is an ordinary one, winding from the left to the right.

But as each System ought to have Communication with the preceding one, though not with that which follows; to this end a projecting Tooth of communication made of Steel ℘ is rivetted to the upper Plane of the uppermost Wheel *a*. This Tooth must be placed exactly on the Point of a Tooth of the Wheel, and by it's Revolution catches and turns every time but one Tooth of the uppermost Wheel of the preceding System, sliding over the following one (if there be any) without touching it. For this reason the Planes of the Brass Bottoms in all the Systems ought to incline a little. This will best appear from the Vertical Section, *Fig.* 2. (cut in *Fig.* 1. in the Direction from ♄ to *f*) in which *α* is the Brass-Bottom, H H the Wood-Bottom, *g* the Operator, *i* the Groove, *f* the third common Tooth-Wheel, *b* the second Wheel, *a* the first or uppermost Ratchet-Wheel, *e* the Bridge, ⊙ the Covering-Plate, and ℘ the Tooth of Communicaton. I have represented all these Pieces of one Thickness; but every Artist will easily know where to add or take off. Fig. 2.

Fig. 5. shews the Plan and true Disposition of the Teeth in the several uppermost Wheels; that is to say, The Parallel Lines A B and C D ought always to cut the Brass Bottoms (which are like one another in Length and Breadth) length-wise into two equal Parts: Then the perpendicular Intersection E F will determine the Centers *a* and *b*, of the two Wheels H and G. The stop *r* ought every time to hold it's Wheel in such a manner, that the Points of two Teeth coincide with the Line A B or C D. The Obliquity of the Teeth is the same in both, with this difference however, that in G, which is a Wheel of the System A A, (*Fig.* 1.) they are cut in from the left to the right, but in H (a Wheel of the System B B) from right to the left. I need not take notice, that for making the Work more durable, the Teeth are not to be cut out into quite sharp Points, but blunted a little, as in the Wheel H. The Nicety of the whole Machine chiefly consists in placing the Center *a* and *b*, or (which amounts to the same thing) after having chosen the Breadth of the Brass-Bottoms, in determining the Diameter of the uppermost Wheel: For if that should prove so large, as that the two Wheels H and G should very near touch one another, the Tooth of Communication will be short; it's Operation will be of a small Force, and the Wheels themselves will Fig. 5.

 require

require a very great Exactneſs, leſt by turning about the Wheel H, and the Tooth of Communication ſtanding in the Poſition as it is repreſented in *Fig.* 5, a Tooth of the Wheel H may touch it, and ſtop the Motion. Whereas, on the other hand, ſuppoſing the Centers at the ſame Diſtance, and the Diameters of both Wheels leſs, the Tooth of Communication will be longer: than ſuch an Exactneſs is not requir'd in the Wheel, yet more Force is neceſſary for making the Tooth of Communication lay hold the better. Furthermore, it will be well for you to make the undermoſt common Tooth-Wheel as large as you can.

From the Conſtruction of this firſt Syſtem, with which the 3d, 5th, 7th, *&c.* entirely agree, one may eaſily imagine the 2d, 4th, 6th, 8th, *&c.* for every thing there alſo is the ſame, except only, that it is inverted; ſo that what in the firſt ſtands on the right-hand, is on the Left in the ſecond.

The Plate for *Multiplication* has on it's hollow Axis, as it is ſaid before, two Shoulders, the lowermoſt of which is very ſmall, the Sum of it's Height, the Thickneſs of the Plate of the Wheel *mm*, and of the Operator muſt amount to as much as anſwers to the Height of the Bridge *ee*. On both Ends of the Braſs-Bottoms, the two Pieces of Braſs ☾☾, of the ſame Height, are rivetted on. This being done, at laſt the Covering-Plates ⊙⊙ is prepared and ſkrewed on the Pieces of Braſs ☾☾. If the Machine be made pretty large, the Covering-Plate muſt be ſkrewed faſt, not only to the Bridge *ee*, but alſo not far from the Wheel of Multiplication. It muſt be provided not only with round Holes, through which are to go the Axis of each uppermoſt Wheel *a*, and the hollow Axis of the Plate *l*; but it muſt alſo have a long Slit, in which the Operator and Determinator may be moved up and down, and laſt of all a ſmall Window over the Plate of *Multiplication*, through which the Figure or Number engraved on the Plate may appear diſtinctly. To the projecting Skrew *l*, of the Plate *l*, is fitted an Handle *ff*, joined to an Index in the Shape of a Scythe. The Skrew in the Syſtem A A is a common Skrew, conſequently the Roundneſs of the Scythe muſt turn from the left to the right; but in the Syſtem BB, where it ought to be inverted, like all the other Parts, the Scythe muſt turn from the right to the left, as in the Figure. The Uſe of this is to ſhew which Way the Wheels are to be turned; and the Skrews are to prevent the Machine's being hurt by unſkilful Hands.

On the Side of the Determinator, *viz.* on that Piece which cannot be preſſed down, is alſo ſkrewed a ſmall Index, which may be directed to ſuch Numbers or Figures as is required. Theſe Figures are to be engraven in the Covering-Plate, according to the Figure, and their Diſtance depends on the Ratchet-Teeth *ee* (*Fig.* 4) in the Braſs-Bottom.

On the Axis of each uppermoſt Wheel *a* (which Axis muſt be made ſquare as far as it projects over the Covering-Plate) is fixed a thin round Silver-Plate *xx* (in the Syſtems BB and CC) or *ad* in *Fig.* 3. yet ſo that it may not rub againſt the Covering-Plate. It has a hollow Axis *bc* (*Fig.*

(*Fig.* 3.) on which is a right or left Skrew, according to the Syſtem it belongs to, and a ſmall Shoulder *c*. To the Skrew is ſkrewed the Handle *ſs* (Syſtem BB and CC, *Fig.* 1.) which is vertically flat on the Extremity, in order to turn by it the Plate and the Wheels. The Plate (as appears by the Figure) is divided by 3 concentrick Circles into two Rings, in the outmoſt of which are engraven the Numbers for *Addition*, in the inmoſt thoſe for *Subſtraction*. I will hereafter call this Plate only the *Silver-Plate*, the firſt Ring the *Addition-Ring*, the ſecond the *Subſtraction-Ring*: Moreover two Indexes *w* and *y* are ſkrewed to the Covering-Plate; *w* ſhews the Numbers of the outmoſt or *Addition Ring*, and *y* thoſe of the *Subſtraction-Ring*. They have Hinges, that they may be lifted up, and the *Silver-Plate* taken out or put in again: Their Curvature ſerves for a Direction, which way the Plates ought to be turned.

A ſkilful Artificer will be able to give them a neater and handſomer Shape, than here in the Draught, where I would not cover the Numbers.

All this being done, there remains now the Figures or Numbers to be engraven, in the manner following: Place each uppermoſt Wheel *a* (Syſtem A A) ſo that the Tooth of Communication be ready to catch (as in G, *Fig.* 5) which may be eaſily felt. Obſerve in the Silver-Plate, where the Index *w* points, and there engrave the Number or Figure 9, lower down in the *Subſtraction-Ring*, where the Index *y* points, engrave the Cypher *o*. After this divide both Rings into 10 equal Parts, one of which is already deſigned for 9 in the *Addition*, and another for *o* in the *Subſtraction-Ring*; then obſerve which way the Wheel turns, if from the right to the left, as in Syſtem B B, then you muſt from the engraven Number 9 in the *Addition-Ring*, towards the right engrave *o* next, then 1, 2, 3, 4, *&c.* and in the *Subſtraction-Ring* towards the right alſo, from the already engraved *o*, firſt engrave 9, then 8, 7, 6, *&c. ordine inverſo*. But if the Wheel turns from the left to the right, as in the Syſtems A A and C C, you engrave the Numbers or Figures in the ſame Order, but from the right to the left. (See in *Fig.* 1. the *Syſtems* BB and CC.)

In the *Multiplication-Wheels mm* you muſt conduct the Index *ff* exactly to the Window, as it is drawn in the Syſtem BB; mark the Place on the round *Multiplication-Plate* under the Window, and engrave upon it the Cypher or *o*; Then make, by two concentrick Circles, a Ring upon this Plate, and divide this Ring into ten equal Parts, and after the *o* (already engraven) engrave on the Numbers 1, 2, 3, 4, 5, 6, 7, 8, 9, in the ſame Order as it was done in the *Addition-Ring* of the Silver-Plate of the ſame Syſtem. Laſt of all, if you think fit, you may ſkrew on thin Ivory Plates, to note upon them the Numbers which are to be calculated, particularly a long ſmall one on that Side of the Slit of the Determinator, where there are are no Numbers, and alſo two ſhorter broader ones, one under the Window of *Multiplication*, the other above the Silver-Plate. All this together compoſes a Machine, by the help of which you may perform all the four Arithmetical Rules or Operations. The Way of working it, is as follows:

I. *As to addition:* For inſtance, if you are to add 32 and 59: becauſe the hindmoſt Syſtem AA in the Figure, which ought to repreſent the Place of Unites, is not cover'd, let us take the Syſtem BB for the Place of the Unites, and the Syſtem CC for the Place of the Tens; turn the Silver-Plates *xx* in theſe two Syſtems, that the Indexes *ww* point to the two Numbers 5 and 9; then make the Determinators *ll*, *ll*, point alſo to 3 and 2: next take one of the two Operators, *ex. gr.* in BB, and pull it down as far as you can, and moveit upwards again. This done, the Number 1 of the Silver-Plate in BB will come by this means under the Index *w*, and the Number 6 of the Silver-Plate in the Syſtem CC under it's Index at the ſame time, which is 61, the Sum, 59 and 2. After this move the Operator of the Syſtem CC alſo up and down, when inſtead of 6, 9 will come under the Index; conſequently you have 91 under the Indexes *ww*, which is the Sum requir'd of 59 and 32 added together. The Reaſon of it is plain; for by pulling down the Operator of the Syſtem BB ſo far, the Stop *c* of the lowermoſt or common Tooth-Wheel *f* (*vid.* Syſt. AA) will ſlide over two Teeth of the Rachet-middlemoſt Wheel *b*; and by moving the Operator up again, the ſame Stop *c* will turn the two Ratchet-Wheels *a* and *b* together, and cauſe the Stop *r* of the great or uppermoſt Wheel *a* to ſlide alſo over two Teeth; at the ſame time the Tooth of Communication *&* will move forward one Tooth of the uppermoſt Ratchet-Wheel in the Syſtem CC; conſequently on the Silver-Plate in BB, inſtead of 9 the Number 1, and in Syſtem CC, inſtead of 5 the Number 6 muſt appear under their Indexes *ww*; and ſo for the ſame reaſon, having pulled up and down the Operator of the Syſtem CC, the Number 6 pointed to by the Index muſt be at laſt changed into 9.

II. *Subſtraction.* Suppoſe 40 the Sum, from which you are to ſubſtract 24: Here you muſt put your Sum 40 in the *Subſtraction-Rings*; that is to ſay, turn the Cypher *o* in the Syſtem BB, and the Number 4 in the Syſtem CC, under the Indexes *yy*, as the Figure ſhews: Set the Determinators at 24, as in *Addition*; move alſo the Operators only once up and down, the Remainder 16 will appear under the Indexes *yy*. As for the Reaſon of this Operation, when you conſider, that the Numbers in the *Subſtraction-Rings* are engraven *inverſo ordine*, as it is ſaid before, you will find that it is the ſame as in *Addition*.

III. *Multiplication.* For inſtance; if your are to multiply 43 by 3, bring the *o* in all your *Addition-Rings* to the Indexes, as alſo in all your *Multiplication-Plates* in the Windows. Write down (which is more particularly neceſſary if the Numbers are larger than here) the Multiplicand 43 upon the Ivory-Plates near the two Determinators in the two Syſtems BB and CC: But the Multiplicator 3, you may write only on the Ivory-Plate under the Window of the Syſtem BB. Set the Determinators at 43; then move your Operators ſucceſſively as often up and down, till there appears in both Windows the Number 3;

then

then you will see on your *Addition-Rings* under the Indexes, the Product 129.

It is easy to understand, that as the *Multiplication* is nothing else than a repeated *Addition*, the Machine does also perform it's Operation by a repeated *Addition* only: For the Number 3, which appears in the Window of the System B B, shews how many Times you have added the Number 3, pointed by the Determinator to itself, which when done 3 Times, is 9. And so the same Number 3, which appears in the Window of the System C C, after your Operation, shews how many Times you have added the Number 4 to itself. I need not to make you observe, that besides the two Systems B B and C C, there must be supposed another, not express'd in the Figure, which will shew the Number 1 of the Product 129.

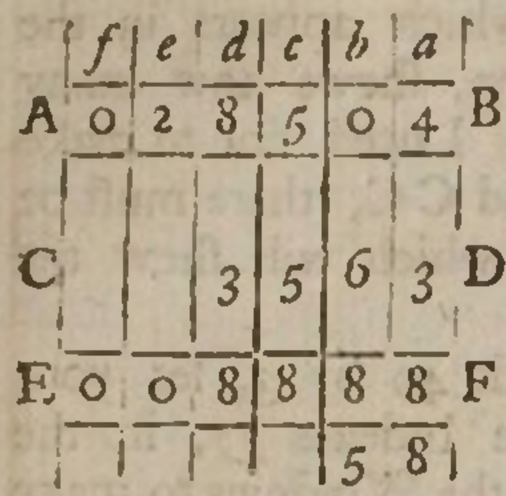
IV. *Division.* If you are, for instance, to divide 40 by 3, set your Dividend 40 in the *Substraction-Rings* under the Indexes *yy*, in the System B B and C C; turn the Indexes *ff*, *ff*, near the Windows to make *o* appear; write your Divisor near the Determinator of the System C C, and set the Determinator at 3; pull the Operator up and down, then you will have 1 under the Index *y*, and 1 likewise in the Window. By this you see, that you cannot work further in this System C C, because you cannot substract 3 from 1: You must therefore go on, to the other Figure of the Dividend, *viz.* 0, and in the System B B set the Determinator again at 3. This being done, the first pulling of the Operator up and down will produce 1 in the Window, and 7 in the *Substraction-Ring* under the Index, and the Number 1 which remained before in the System C C will be changed into *o*. Now as 7 is more than 3, you must work on accordingly; having done it twice more, you will find that there remains under the Index *y* but 1, (which is the Numerator of your Fraction) and below in the two Windows the Quotient 13. When you consider that *Division* is nothing else but a repeated *Substraction*, you will also easily understand the Reason of this Operation.

Those that understand the Matter ever so little, may now easily conceive how they are to proceed with this Machine in larger *Examples*: However, for greater Clearness, I will explain it by two *Examples*.

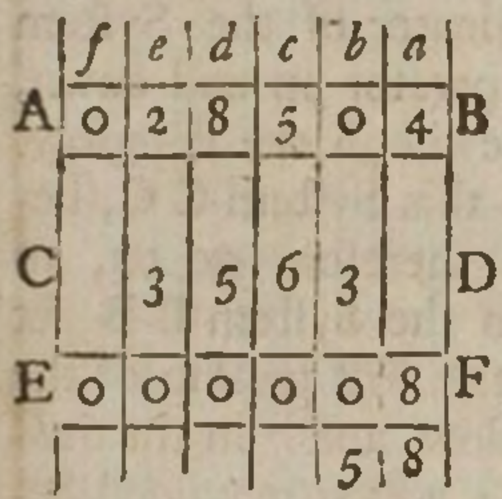
Supposing there are six Systems, *a*, *b*, *c*, *d*, *e*, *f*; Let all the Numbers pointed to by the Indexes *ww* be in A B; those which are to be pointed to by the Determinators in C D; and those which are seen in the Windows, in E F. First of all, you must turn all your *Addition-Rings* of the *Silver-Plates* and your *Multiplication-Plates* to *o*; *viz.* that under all the Indexes, *ww*, and in the Windows nothing may appear but *o*. Write the Number 3563 near the Determinator, in the Systems *a*, *b*, *c*, *d*, and direct them accordingly:

	f	*e*	*d*	*c*	*b*	*a*	
A	o	o	o	o	o	o	B
C			3	5	6	3	D
E	o	o	o	o	o	o	F

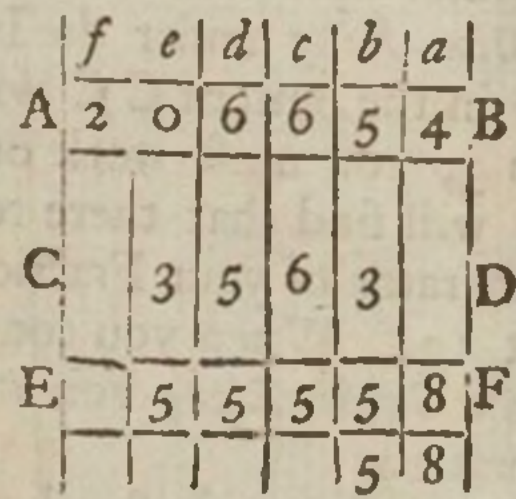
cordingly: The other Number 58, you must write down likewise, but under the Windows in System *a* and *b*, as you see in this Scheme. Move the several Operators, which are moveable, successively as often up and down, till 8 appears below in the Windows, and you will have under the Indexes above 28504, the Product of 3563 × 8. And so the Numbers of the Machine will appear thus.

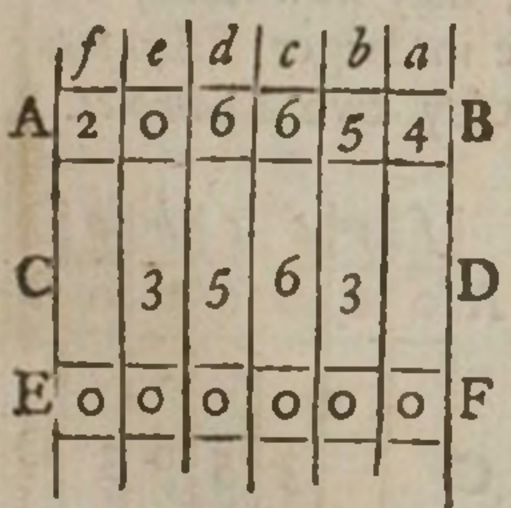


Next advance your Multiplicand 3563, from the Right to the Left; that is to say, place the Determinator in the System *b* at 3, in *c* at 6, in *d* at 5, in *e* at 3, and reduce every Number in the Windows to 0, except in the System *a*. See the *Scheme* following.



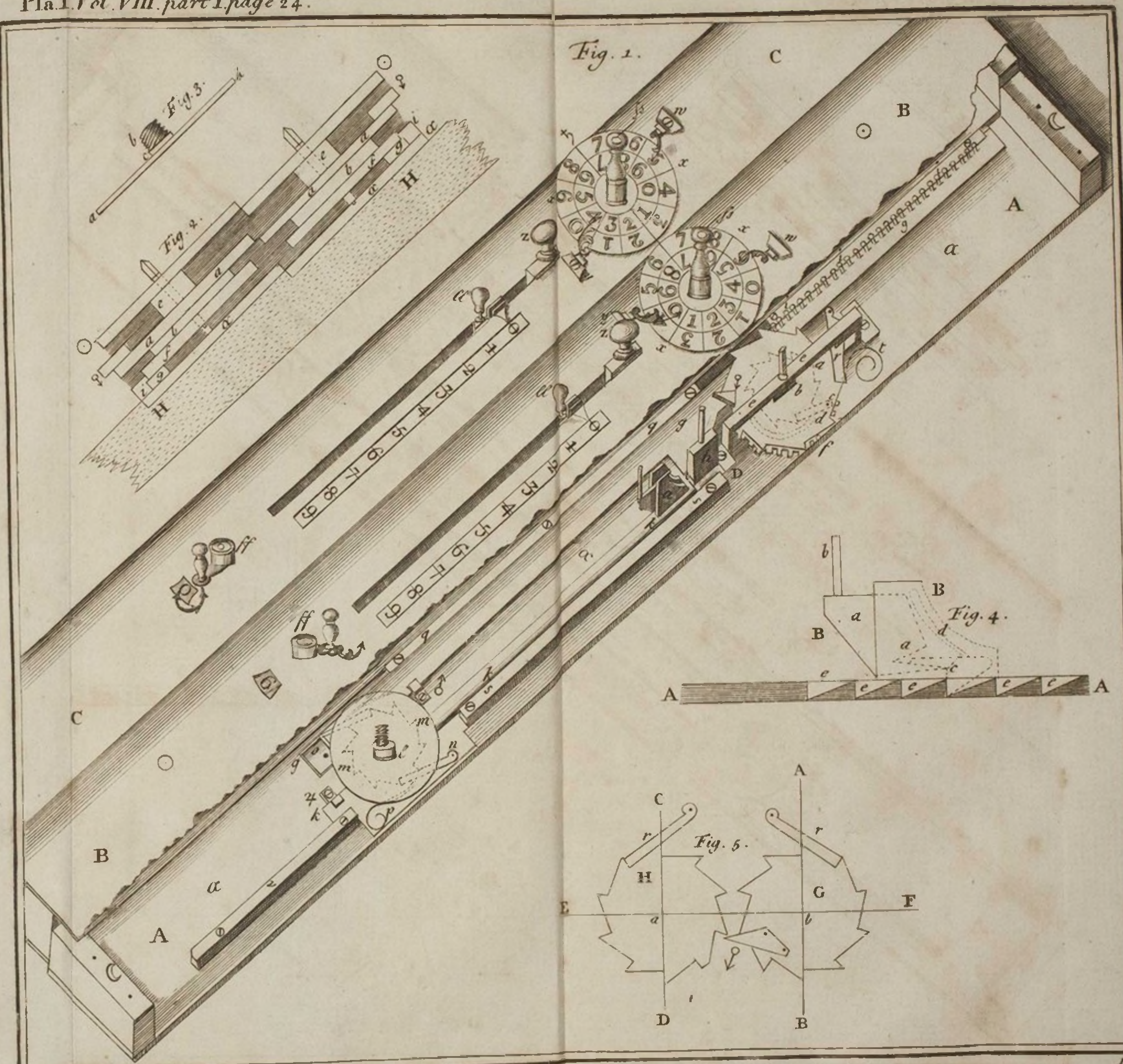
Then pull all the Operators again successively in *b*, *c*, *d*, and *e*, up and down, till 5 appears in the Windows below, and you will find at last under the Indexes 206654, the Product of 3563 × 58.





But if you are to divide again 206654 by 3563, you must place the Dividend above in the *Substraction-Rings* under the Indexes. In the Windows below, every Figure must be 0, likewise as in the *Multiplication*; and write the Divisor under the Dividend, according to *Vulgar Arithmetick*, and as in the *Figure* here annexed.

If you direct the Determinators in *e*, *d*, *c*, *b*, to their Numbers, and substract this Divisor by pulling up and down the Operators as often as you can, you will have in the Windows in *e*, *d*, *c*, *b*, every where 5; but on the *Silver-Plates* there will remain 28504. Now advancing



vancing your Diviſor from the Left to the Right, bringing to the Windows in *d*, *c*, *b*, all the Cyphers 0, and operating as before, there will at laſt appear on the *Silver-Plates* nothing at all, but below in the Windows 5888. See the *Figure* following:

	f	*e*	*d*	*c*	*b*	*a*	
A	0	0	0	0	0	0	B
C			3	5	6	5	D
E		5	8	8	8	8	F

And here you have only this to obſerve, that in ſuch Caſes, you cut off all the hindermoſt Figures or Numbers in EF, except that which ſtands under the firſt Figure of the Diviſor; what remains is your Quotient.

As for what remains, if it be objected that this Machine cannot be fitted for ſo many and long Numbers, as one would pleaſe, becauſe the Multiplication of ſo many Syſtems would require too great a Force for one Operator to move ſo many Wheels, kept by Springs, ſuppoſing the Caſe that all the Teeth of Communication ſhould duly catch; I own that this Objection is but too well grounded: However, I cannot help obſerving at the ſame Time, that this Defect can hardly be avoided, in any Arithmetical Machine, for performing all thoſe Operations of itſelf, without the Help of the Mind: For there muſt certainly be a particular Syſtem for each Place of Figures, which is to communicate with the next; conſequently, as the Syſtems increaſe in Number, the Force muſt increaſe alſo which is required for moving them all. Beſides, it ought to be conſidered, of what Size ſuch a Machine ought to be, which might ſerve for common Uſe. I think few Calculations could be required, for which 14 or 16 Syſtems might not ſuffice. That which I made was of 7 Syſtems, as I have already mentioned. The Diſpoſition of it was neither ſo well contrived as I have explained it here, nor were it's ſeveral Parts ſo well wrought, as a good Artificer, who makes Profeſſion of ſuch Work, might have performed it; yet thoſe 7 Syſtems were very eaſily put in Motion; and if in a Machine for 14 Figures made by a ſkilful Hand, it could not be ſo eaſily practicable, this Defect, I believe, might be eaſily remedied, by applying the other Hand in the fifth or ſixth Syſtem to the Handle *ſs*, in order to eaſe and aſſiſt the Operator.

A brief Account by Mr. John Eames, *F. R. S. of a Work entitled,* The Method of Fluxions and Infinite Series, with it's Application to the

IV. This Text may very well be divided into three Parts: An Introduction, containing the Method of Infinite Series; the Method of Fluxions and Fluents; and laſtly, the Application of both to the moſt conſiderable Problems of the higher Geometry. The Comment conſiſts of very valuable and curious Annotations, Illuſtrations, and Supplements, in order to make the whole a *compleat Inſtitution* for the Uſe of Learners. I ſhall take a kind of comparative View of the Text and Comment together.

 The

Geometry of Curve Lines, *by the Inventor Sir* I. Newton, *Kt. &c. Translated from the Author's Latin Original not yet made publick. To which is subjoined a perpetual Comment upon the whole, &c. by* John Colson, *M. A.* and *F. R. S.* No. 443. *p.* 320. *Oct.* 1736.

The great Author, in what is called the Introduction, teaches the Rudiments of his Method of Infinite Converging Series, which is preparatory to that of Fluxions. In this he shews how all Compound Algebraical Quantities may be resolved into Series of simple Terms, which will converge to those compound Quantities, or rather to their Roots; just as in common Decimal Arithmetick, any complicate Number whatever, rational or surd, may be prosecuted and exhibited to what Degree of Accuracy we please, by decimal Parts continued *in infinitum.* And this general Arithmetick is here applied to the finding of the Roots of all kinds of Algebraical Equations, whether pure or affected.

And this Doctrine is carried on still farther by Mr *Colson* in his Comment. He pursues the Author's Hint, that vulgar Arithmetick and Algebra, decimal Fractions and infinite Series, have the same common Foundation, and compose together but one uniform Science of Computation. For, as in our vulgar Arithmetick, when rightly explained, we express and compute all Numbers by the Root *Ten*, and it's several Powers and their Reciprocals, together with a Set of certain known and small Coëfficients; so in this more universal Arithmetick of infinite Series, we do the same thing in effect, by means of any Root assumed at Pleasure, it's Powers and their Reciprocals, disposed in a regular descending Order, together with any Coëfficients, as it may happen. And when these Series duly converge, they will as truly exhibit by their Aggregate the Quantity required, as a Decimal Fraction infinitely continued will approximate to it's proper *Quæsitum.* This gives him Occasion to expatiate largely upon the Nature and Construction of Arithmetical Scales, particular and general; and to inquire into the Nature and Formation of Infinite Series, and their Circumstances of Convergency and Divergency. To explain which he shews, that in every Series there is always a Supplement to be understood, when it is not exhibited. This Supplement sums up the Series, and makes it stop at a finite Number of Terms, in Series that either converge or diverge. Whence in diverging Series it must necessarily be found and admitted, or otherwise the Conclusion will not be true; but in converging Series, where it can seldom be known, it may safely be omitted, because it continually diminishes with the Terms of the Series, and finally becomes less than any assignable Quantity.

The Nature of infinite Series being thus displayed, he applies them to the Resolution of all kinds of Algebraical Equations. He explains in a very general Manner, the Author's famous Artifice, for finding the *Forms of the Series* for the Roots, and their initial Approximations, by means of a Parallelogram and Ruler, and shews it's Application in all Cases. Then he invents many ways of Analysis, by which the Roots are further prosecuted, and may be produced to any Degree of Accuracy required. Also many other Speculations are added, to compleat the Doctrine of Series; particularly a very general and useful Theorem, for the Solution of all affected Equations in Numbers.

From

From the Refolution of Equations, and the Doctrine of Infinite Series, which finifhes the firft Part of this Work, Sir *I. Newton* proceeds to lay down the Principles of his Method of Fluxions, which is the chief Defign of the prefent Treatife. This Method he founds upon the abftract or rational Mechanicks, by fuppofing all Mathematical Quantities to be generated, as it were, by local Motion, and therefore to have relative Velocities of Increafe or Decreafe, which Velocities he calls *Fluxions*. And the Quantities fo generated by a continual Flux he calls *Fluents* or flowing Quantities; the Relation of which Fluents is always expreffed by fome Algebraical Equation, either given or required. If this Equation be given, and the Relation of the Fluxions is required, it conftitutes the *direct Method of Fluxions*; but when the contrary, 'tis the *inverfe Method of Fluxions.*

Sir *Ifaac*, in his firft Problem, which takes in the direct Method of Fluxions, fhews how to find the Relation of the Fluxions in a very general Manner, and by a great Variety of Solutions. This way of refolving the Problem is peculiar to this Work. He likewife extends it to Equations involving feveral Fluents, which accommodates it to thofe Cafes, wherein any complex or irrational Quantities may be found, or Quantities that are geometrically irreductible. Then he demonftrates the Principles of his Method, or the Precepts of Solution, from the Nature of Moments or vanifhing Quantities, and from the obvious Properties of Equations, which involve indeterminate Quantities.

The Commentator much enlarges upon this whole Doctrine; he enters into the Reafon and Ufe of this Multiplicity of Solutions, and fhews it is a neceffary Refult from the different Forms the fame given Equation may acquire. But efpecially he takes the Author's Demonftration into ftrict Examination, endeavours farther to illuftrate and enforce it's Evidence, and to clear it from all the Objections that either have or may be urged againft it. He even contends, that though the Moments and vanifhing Quantities of the Author could be proved to be impoffible, as has been fuggefted by fome Mathematicians, yet even then they would be fufficient for all the Purpofes of Fluxions, and he produces Inftances of a like Nature from other Parts of Mathematicks. And though the Author, Sir *I. Newton*, in his prefent Treatife, does not directly mention fecond Fluxions, or thofe of higher Orders; yet the ingenious Commentator thinks proper to extend his Enquiries to thefe Orders of Fluxions, demonftrates their Theory, gives Rules and Examples for deriving their Equations, proves their relative Nature, and even exhibits them to View by Geometrical Figures. This laft he does chiefly in what he calls the Geometrical and Mechanical Elements of Fluxions; and he contrives a very general Method, by means of Curve-lines and their Tangents, to make Fluxions and Fluents the Objects of Senfe and ocular Infpection; and thereby he illuftrates and verifies the received Methods of deriving their Equations in all Cafes.

In the Author's fecond Problem, or the Relation of the Fluxions being given to determine the Relation of the Fluents, which includes the inverfe Method of Fluxions, he begins with a particular Solution of it. He calls this Solution particular, becaufe it extends only to fuch Cafes, wherein the given Fluxional Equation either has been, or might have been, derived from fome previous finite Algebraical Equation. Then he fhews how we may return directly to this Equation. But this is feldom the Cafe of fuch Fluxional Equations, whofe Fluents or Roots are propofed to be found. For they have commonly Terms either redundant or deficient, by which they cannot be brought under this particular Solution. Therefore to anfwer this Cafe alfo, he gives us a general Solution, in which he extracts the Roots of any propofed Fluxional Equation, by feveral ingenious Methods of Analyfis. And here it is chiefly, that he calls his Method of Infinite Series to his Affiftance; for the Fluent, or Root, will here always be exhibited by a Series. And to find the Fluent in finite Terms, when it can be done, requires particular Expedients, as we fhall fee afterwards.

Mr *Colfon*, in his Comment upon this Part of the Work, is very full and explicit. He explains and applies the Author's particular Solution; but is much more copious in explaining the Examples, and clearing up the Difficulties and Anomalies of the general Solution. This is chiefly performed by introducing feveral new and fimple Methods of Analyfis, or Proceffes of Refolution; and by applying the Author's Artifice of the Ruler and Parallelogram mentioned before, to thefe Fluxional Equations: By which means not only the Forms of the Series are determined, and their initial Approximations, as has been obferved above; but likewife all the Series may be found, that can be derived from the fame Fluxional Equation. The Commentator concludes by giving us a very general Method for refolving all Equations, whether Algebraical or Fluxional; which Method requires no foreign Affiftance, or no fubfidiary Operations, which all other Methods do. It is founded upon the Ufe and Admiffion of the higher Orders of Fluxions, and is exemplified by the Solution of feveral ufeful Problems. Here the Comment leaves us, but we will go on with our Author.

Having thus taught us the Method of Fluxions both direct and inverfe, he proceeds to apply this Method to fome very curious and general Problems, chiefly in the Geometry of Curve-lines. As firft, he determines the *maxima* and *minima* of Quantities in all Cafes, and propofes fome elegant Problems to illuftrate this Doctrine. Then he teaches us to draw Tangents to Curves, whether Geometrical or Mechanical, and that after a great Variety of Ways, or however the Nature of the Curve may be defined. Here likewife he propofes fome Queftions, to exercife and improve the Learner: Then is very particular upon finding the Quantity of Curvature, at any Point of a given Curve,

Curve, whether Geometrical or Mechanical, or in determining the Centre and the Radius of Curvature: To which ſeveral other curious Speculations are ſubjoined of a like Nature. Here he communicates a very elegant and entirely new Problem, for determining the Quality of the Curvature, at any Point of a given Curve; or how the Curvature proceeds in reſpect of it's greater or leſs Inequability.

Afterwards he goes on to the Quadrature of Curves, which chiefly gives Occaſion to apply the inverſe Method of Fluxions; and firſt he ſhews how, by the direct Method, to find as many Curves as you pleaſe, (or to determine their Equations) the Areas of which ſhall be capable of an exact Quadrature. Then he ſhews how to find as many Curves as you pleaſe, which, though not capable of a juſt Quadrature, yet their Areas may be compared to thoſe of the Conic Sections, or of ſuch other Curves as ſhall be aſſigned. Laſtly, He ſhews how to determine in general the Area of any Curve that ſhall be propoſed, chiefly by the Method of Infinite Series; where many curious and uſeful Speculations are occaſionally introduced and inſerted: As how to aſcertain the Limits of an Area, when thus found analytically; how commodiouſly to ſquare the Circle, the Ellipſis, or Hyperbola, and how to apply the Quadrature of this laſt to the computing a Canon of Logarithms; the Conſtruction of Tables for the ready finding of Quadratures, or the Compariſon of Areas, and how to apply them to the ſolving of other like Problems; the forming of Conſtructions, and demonſtrating Theorems by Fluxions; the approximating to Areas mechanically, and ſuch like.

From finding of Areas he proceeds to the *Rectification of Curves*; and firſt he ſhews how to find as many Curves as you pleaſe, whoſe Curve-lines are capable of an exact Rectification. Then he teaches us to find as many Curves as we pleaſe, whoſe Curve-lines, though not capable of a juſt Rectification, yet may be compared with the Lengths of any Curve-lines aſſigned, or with the Areas of any Curve, when reduced to the Order of Lines. Laſtly, he determines the Lengths of any Curve in general, and gives ſeveral proper Examples of it. All which elegant Speculations are managed with admirable Skill, great Subtility, and fine Contrivance.

An Account by Mr John Eames, *F.R S. of a Book entituled,* A Mathematical Treatiſe, containing a Syſtem of Conic-Sections, with the Doctrine of Fluxions and Fluents, applied to va-

V. In the firſt Book, he conſiders the Properties of the three Sections of a Cone, as well in, as out of the Cone. And to make this Part of the Work of more Service to the Reader, he has not only ſelected the moſt conſiderable Properties of theſe Curves that are to be met with in other Writers, both antient and modern, but has added ſeveral new ones, which, as he informs us, are inſerted in their proper Places. And that ſuch Gentlemen as are deſirous to read Sir *I. Newton*'s *Principia*, but are a Loſs for want of a ſufficient Acquaintance with Conic-Sections, may be the more obliged, he has taken particular Care to demonſtrate ſuch Properties as Sir *Iſaac* preſuppoſes his Reader to be acquainted withal. Accordingly, he has prefixed a Table of ſuch Proporious Subjects. By John Muller. No. 446. p. 87. *July*, &c. 1737.

Propositions, informing him as well where they are to be met with in this Book, as in Sir *I. Newton*'s *Principia Mathematica.*

The Proofs made use of in his Demonstrations, are sometimes Algebraical, at other Times Geometrical, according as he finds the one to be plainer and shorter than the other.

Book II.

The second Book treats of the direct Method of Fluxions. And here he hopes the first Principles of this Method are laid down, not only in a new, but very plain and concise Manner. He proceeds to shew the Use of Fluxions in the Solution of the common Problems of finding the *Maxima* and *Minima* of Quantities, the *Radii* of the Evolution of Curves, and the *Radii* of Refraction and Reflection. Under the first of these Heads he tells us, particular Care has been taken to distinguish the *Maximums* from the *Minimums*, a Thing which has not been taken Notice of so much as it ought to have been. And whereas some Mathematicians having made use of what they call infinitely small Quantities, are forced to reject something out of the Equation, for finding the Fluxion of a Rectangle, whose Sides are varying Quantities, Mr *Muller* uses only finite Quantities; and finds the Fluxion of such a Rectangle after a new Manner, without rejecting any Quantity for it's Smallness. He does the same in finding the Fluxion of a Power. And to avoid the Use of infinitely small Quantities, introduces a new Principle, *viz.* That a Curve-Line may be considered as generated by the Motion of a Point carried along by two Forces or Motions, one in a Direction always parallel to the Abscis, and the other in a Direction always parallel to the Ordinate. Hence he infers, that the Fluxion of the Ordinate is to the Fluxion of the Abscis, as the Ordinate is to the Subtangent of the Curve.

Having likewise proved from the first Supposition, that if the describing Point, when arrived at any Place given, should continue to move onwards, with the Velocity it has there, it would proceed in a right Line, which would touch the Curve in that Point; he concludes that the Direction of the Force in that Place is in the Tangent to the Curve: Consequently, the three Directions being known in each Place, the Proportion between the Velocities of the urging Forces will be likewise known. So that the Nature of the Curve being given, the Law observed by these Velocities may be found; and if the Law of the Velocities be given, the Nature of the Curve may likewise be given.

Book III.

In the third and last Book, we have the inverse Method of Fluxions, with it's Application to the several Problems solvable by it; such as the superficial and solid Contents of curvilineal Figures, the Rectification of Curve-Lines, Centers of Gravity, Oscillation and Percussion. Here also Mr *Cotes*'s Table of Fluents are explained and illustrated by Examples.

He finishes this Book with a great Variety of Problems, that are of a Physico-Mathematical Nature, several of which are new, and proposed to him by Mr *Belidor.* Some, indeed, are not so, having been solved

ſolved by Meſſieurs *Varignon* and *Parent* ; but then he has ſolved them after a different, and, as he hopes, a more agreeable, Manner, the Conſtruction being more ſimple, and the Proceſs much ſhorter.

V. The Author's firſt Deſign in compoſing this Treatiſe, was to eſtabliſh the Method of Fluxions on Principles equally evident and unexceptionable with thoſe of the antient Geometricians, by Demonſtrations deduced after their Manner, in the moſt rigid Form, and by illuſtrating the more abſtruſe Parts of the Doctrine, to vindicate it from the Imputation of Uncertainty or Obſcurity. But he has likewiſe comprehended in this Work the Application of Fluxions to the moſt important geometrical and philoſophical Enquiries. It conſiſts of an Introduction, and two Books. In the Introduction he gives an Abſtract of the Diſcoveries of the Antients in the higher Parts of Geometry, with Obſervations on their Method, and thoſe that firſt ſucceeded to it. The firſt Book treats of Fluxions in a geometrical Method, and the ſecond treats of the Computations.

An Account of a Book entitled, A Treatiſe of Fluxions, in Two Books, *by* Colin M'Laurin, *A. M. Prof. Math.* Edinb. *F. R. S.* 4to. *in 2 Vol. pag.* 763. No. 468. p. 325. *Preſented* Jan. 27, 1742 3.

In the Introduction we have an Abſtract not only of the Diſcoveries of the Antients in the higher Parts of Geometry, but likewiſe of their Demonſtrations. After an Account of the Propoſitions of this Kind, that are to be found in the 12th Book of *Euclid*, there follows a Summary of what is moſt material in the Treatiſes of *Archimedes*, concerning the Sphere and Cylinder, Conoids and Spheroids, the Quadrature of the *Parabola* and the ſpiral Lines. The Demonſtrations are not preciſely in the ſame Form as thoſe of *Archimedes*, but are often illuſtrated from the elementary Propoſitions concerning the Cone, or Corollaries from them, after the Example of *Pappus*, from whom the Propoſition is demonſtrated, and rendered more general, concerning the Area of the Spiral that is generated on a ſpherical Surface by the Compoſition of two uniform Motions analogous to thoſe by which the Spiral of *Archimedes* is deſcribed on a Plane. This Area, though a Portion of a curve Surface, is found to admit of a perfect Quadrature, and this Propoſition concludes the Abſtract. He takes Occaſion from theſe Theorems to demonſtrate ſome Properties of the Conic Sections, that are not mentioned by the Writers on that Subject ; and there are more of this Kind deſcribed in Chap. 11 and 14 of Book I.

Coll. Math. Prop. 21. Lib. 4.

It is known, that if a Parallelogram, circumſcribed about a given Ellipſe, have it's Sides parallel to the conjugate Diameters, then ſhall it's Area be of an invariable or given Magnitude, and equal to the Rectangle contained by the Axis of the Figure ; but this is only a Caſe of a more general Propoſition. For if, upon any Diameter produced without the Ellipſe, you take two Points, one on each Side of the Center at equal Diſtances from it, and the Four Tangents be drawn from theſe Points to the Ellipſe, thoſe Tangents ſhall form a Parallelogram, which is always of a given or invariable Magnitude, when the Ellipſe is given, if the *Ratio* of thoſe Diſtances to the Diameter be given ; and when the *Ratio* of thoſe Diſtances to the Semidiameter is that of the Diagonal

of

of a Square to the Side, (or of $\sqrt{2 \text{ to } 1}$) the Parallelogram has it's Sides parallel to conjugate Diameters. It is likewiſe ſhown here, how the Triangles, *Trapezia*, or Polygons of any Kind are determined, which circumſcribed about a given Ellipſe, are always of a given Magnitude.

There is alſo a general Theorem concerning the *Fruſtum* of a Sphere, Cone, Spheroid, or Conoid, terminated by parallel Planes, when compared with a Cylinder of the ſame Altitude on a Baſe equal to the middle Section of the *Fruſtum* made by a Parallel Plane. The Difference betwixt the *Fruſtum* and the Cylinder is always the ſame in different Parts of the ſame, or of ſimilar Solids, when the Inclination of the Planes to the Axis, and the Altitude of the *Fruſtum*, are given. This Difference vaniſhes in the parabolic Conoid. It is the ſame in all Spheres; being equal to half the Content of a Sphere of a Diameter equal to the Altitude of the *Fruſtum*. In the Cone it is one Fourth of the Content of a ſimilar Cone of the ſame Height with the *Fruſtum*; and in other Figures it is reduced to the Difference in the Cone.

In the Remarks on the Method of the Antients, the Author obſerves, that they eſtabliſhed the higher Parts of their Geometry on the ſame Principles as the Elements of the Science, by Demonſtrations of the ſame Kind; that they ſeem to have been careful not to ſuppoſe any thing to be done, till by a previous Problem they had ſhewn how it was to be performed: Far leſs did they ſuppoſe any thing to be done, that cannot be conceived to be poſſible, as a Line or Series to be actually continued to Infinity, or a Magnitude to be diminiſhed till it become infinitely leſs than it was. The Elements into which the reſolved Magnitudes were always finite, and ſuch as might be conceived to be real. Unbounded Liberties have been introduced of late, by which Geometry (wherein every thing ought to be clear) is filled with Myſteries, and Philoſophy is likewiſe perplexed. Several Inſtances of this Kind are mentioned. The Series 1, 2, 3, 4, 5, 6, 7, *&c.* is ſuppoſed by ſome to be actually continued to Infinity; and, after ſuch a Suppoſition, we are puzzled with the Queſtion, Whether the Number of finite Terms in ſuch a Series is finite or infinite. In order to avoid ſuch Suppoſitions, and their Conſequences, the Author choſe to follow the Antients in their Method of Demonſtration as much as poſſible. Geometry as been always conſidered as our ſureſt Bulwark againſt the Subtleties of the Scepticks, who are ready to make uſe of any Advantages that may be given them againſt it *; and it is important, not only that the Concluſions in Geometry be true, but likewiſe that their Evidence be unexceptionable. However, he is far from affirming, that the Method of Infiniteſimals is

* See *Bayle*'s Dictionary, Article *Zeno.*

without

without Foundation, and afterwards endeavours to juſtify a proper Application of it.

The Grounds of the Method of Fluxions are deſcribed in Chap. 1. Book I. and again Chap. 1. Book II. In the former, Magnitudes are conceived to be generated by Motion, and the Velocity of the generating Motion is the Fluxion of the Magnitude. Lines are ſuppoſed to be generated by the Motion of Points. The Velocity of the Point that deſcribes the Line is it's Fluxion, and meaſures the Rate of it's Increaſe or Decreaſe. Other Magnitudes may be repreſented by Lines that increaſe or decreaſe in the ſame Proportion with them; and their Fluxions will be in the ſame Proportion as the Fluxions of thoſe Lines, or the Velocities of the Points that deſcribe them. When the Motion of a Point is uniform, it's Velocity is conſtant, and is meaſured by the Space which is deſcribed by it in a given Time. When the Motion varies, the Velocity at any Term of the Time is meaſured by the Space which would be deſcribed in a given Time, if the Motion was to be continued uniformly from that Term without any Variation. In order to determine that Space, and conſequently the Velocity which is meaſured by it, four Axioms are propoſed concerning variable Motions, two concerning Motions that are accelerated, and two concerning ſuch as are retarded. The firſt is, That the Space deſcribed by an accelerated Motion is greater than the Space which would have been deſcribed in the ſame Time, if it had not been accelerated, but had continued uniform from the Beginning of the Time. The ſecond is, That the Space which is deſcribed by an accelerated Motion, is leſs than the Space which is deſcribed in an equal Time by the Motion which is acquired by that Acceleration continued afterwards uniformly. By theſe, and two ſimilar Axioms concerning retarded Motions, the Theory of Motion is rendered applicable to this Doctrine with the greateſt Evidence, without ſuppoſing Quantities infinitely little, or having Recourſe to prime or ultimate *Ratios*. The Author firſt demonſtrates from them all the general Theorems concerning Motion, that are of Uſe in this Doctrine; as that when the Spaces deſcribed by two variable Motions are always equal, or in a given *Ratio*, the Velocities are always equal, or in the ſame given *Ratio*; and converſely, when the Velocities of two Motions are always equal to each other, or in a given *Ratio*, the Spaces deſcribed by thoſe Motions in the ſame Time are always equal, or in that given *Ratio*; that when a Space is always equal to the Sum or Difference of the Spaces deſcribed by two other Motions, the Velocity of the firſt Motion is always equal to the Sum or Difference of the Velocities of the other Motions; and converſely, that when a Velocity is always equal to the Sum or Difference of two other Velocities, the Space deſcribed by the firſt Motion is always equal to the Sum or Difference of the Spaces deſcribed by theſe two other Motions. In comparing Motions in this Doctrine, it is convenient and uſual

usual to suppose one of them uniform; and it is here demonstrated, that if the Relation of the Quantities be always determined by the same Rule or Equation, the *Ratio* of the Motions is determined in the same Manner, when both are supposed variable. These Propositions are demonstrated strictly by the same Method which is carried on in the ensuing Chapters for determining the Fluxions of the Figures.

In Chap. II. a Triangle that has two of it's Sides given in Position, is supposed to be generated by an Ordinate moving parallel to itself along the Base. When the Base increases uniformly, the Triangle increases with an accelerated Motion, because it's successive Increments are *Trapezia*, that continually increase. Therefore, if the Motion with which the Triangle flows, was continued uniformly from any Term for a given Time, a less Space would be described by it than the Increment of the Triangle, which is actually generated in that Time by Axiom I. but a greater Space than the Increment which was actually generated in an equal Time preceding that Term, by Axiom II. and hence it is demonstrated, that the Fluxion of the Triangle is accurately measured by the Rectangle contained by the corresponding Ordinate of the Triangle, and the right Line which measures the Fluxion of the Base. The Increment which the Triangle acquires in any Time, is resolved into two Parts; that which is generated in consequence of the Motion with which the Triangle flows at the Beginning of the Time, and that which is generated in consequence of the Acceleration of this Motion for the same Time. The latter is justly neglected in measuring that Motion (or the Fluxion of the Triangle at that Term) but may serve for measuring it's Acceleration, or the second Fluxion of the Triangle. The Motion with which the Triangle flows, is similar to that of a Body descending in free Spaces by an uniform Gravity, the Velocity of which, at any Term of the Time, is not to be measured by the Space described by the Body in a given Time, either before or after that Term, because the Motion continually increases, but by a Mean between these Spaces.

When the Sides of a Rectangle increase or decrease with uniform Motions, it may be always considered as the Sum or Difference of a Triangle and *Trapezium*; and it's Fluxion is derived from the last Proposition. If the Sides increase with uniform Motions, the Rectangle increases with an accelerated Motion; and in measuring this Motion at any Term of the Time, a Part of the Increment of the Rectangle, that is here determined, is rejected, as generated in consequence of the Acceleration of that Motion.

The Fluxions of a curvilineal Area (whether it be generated by an Ordinate moving parallel to itself, or by a Ray revolving about a given Center) and of the Solid, generated by the Area revolving about the Base, are determined by Demonstrations of the same Kind; and when the Ordinates of the Figure increase, the Increment of the Area is resolved in like manner into two Parts, one of which is only to be re-

tained

tained in meaſuring the Fluxion of the Area, the other being rejected as generated in conſequence of the Acceleration of the Motion with which the Figure flows. An Illuſtration of ſecond and third Fluxions is given by reſolving the Increment of a Pyramid or Cone into the ſeveral reſpective Parts that are conceived to be generated in conſequence of the firſt, ſecond, and third Fluxions of the Solid, when the Axis is ſuppoſed to flow uniformly.

In Chap. V. a Series of Lines in Geometrical Progreſſion are repreſented by an eaſy Conſtruction. The firſt Term being ſuppoſed invariable, and the ſecond to increaſe uniformly, all the ſubſequent Terms increaſe with accelerated Motions. The Velocities of the Points that deſcribe thoſe Lines being compared, it is demonſtrated from the Axioms by common Geometry, that the Fluxions of any two Terms are in a *Ratio* compounded of the *Ratio* of the Terms, and of the *Ratio* of the Numbers that expreſs how many Terms precede them in the Progreſſion.

In Chap. VI. the Nature and Properties of Logarithms are deſcribed after the celebrated Inventor; and it is obſerved, that he made uſe of the very Terms *Fluxus* and *Fluat* on this Occaſion. A Line is ſaid to increaſe or decreaſe *proportionally*, when the Velocity of the Point, that deſcribes it, is always as it's Diſtance from a certain Term of the Line; and if in the mean time another Point deſcribes a Line with a certain uniform Motion, the Space deſcribed by the latter Point is always the Logarithm of the Diſtance of the former from the given Term. Hence the Fluxion of this Diſtance is to the Fluxion of it's Logarithm as that Diſtance is to an invariable Line; and the Fluxions of the Quantities that have their Logarithms in an invariable *Ratio*, are to each other in a *Ratio* compounded of this invariable *Ratio*, and of the *Ratio* of the Quantities themſelves. Some Propoſitions are demonſtrated, that relate to the Computation of Logarithms; but this Subject is proſecuted farther in the ſecond Book. The Logarithmick Curve is here deſcribed, with the Analogy betwixt Logarithms, and Hyperbolic *Ratios*.

In Chap. VII. after a general Definition of Tangents, it is demonſtrated, that the Fluxions of the Baſe, Ordinate, and Curve, are in the ſame Proportion to each other, as the Sides of a Triangle reſpectively parallel to the Baſe, Ordinate, and Tangent. When the Baſe is ſuppoſed to flow uniformly, if the Curve be convex towards the Baſe, the Ordinate and Curve increaſe with accelerated Motions; but their Fluxions at any Term are the ſame as if the Point which deſcribes the Curve had proceeded uniformly from that Term in the Tangent there. Any further Increment which the Ordinate or Curve acquires, is to be imputed to the Acceleration of the Motions with which they flow. A Ray that revolves about a given Center, being ſuppoſed to meet any Curve and an Arc of a Circle, deſcribed from the ſame Center, the Fluxions of the Ray, Curve, and circular Arc, are compared together; and ſeveral other Propoſitions concerning Tangents are demonſtrated from the

 Axioms.

Axioms. The next Chapter treats of the Fluxions of curve Surfaces in a ſimilar Manner.

Chap. IX. treats chiefly of the greateſt and leaſt Ordinates of Figures, and of the Points of contrary Flexure and Cuſpids. The Fluxion of the Baſe being given, when the Fluxion of the Ordinate vaniſhes, the Tangent becomes parallel to the Baſe, and the Ordinate moſt commonly is a *Maximum* or *Minimum*, according to the Rule given by Authors upon this Subject. But if the 2d Fluxion of the Ordinate vaniſh at the ſame Time, and the 3d Fluxion be real, this Rule does not hold, for the Ordinate is in that Caſe neither a *Maximum* nor *Minimum*. If the 1ſt, 2d, and 3d Fluxions vaniſh, and the 4th Fluxion be real, the Ordinate is a *Maximum* or *Minimum*. The general Rule demonſtrated in this Chapter, and again in the laſt Chapter of Book II. is, that when the 1ſt Fluxion of the Ordinate, with it's Fluxions of any ſubſequent ſucceſſive Orders, vaniſh, and the Number of all theſe Fluxions that vaniſh is odd, then the Ordinate is a *Maximum* or *Minimum*, according as the Fluxion of the next Order to theſe is negative or poſitive. The Ordinate paſſes through a Point of contrary Flexure, when it's Fluxion becomes a *Maximum* or *Minimum*, ſuppoſing the Curve to be continued on both Sides of the Ordinate. Hence the common Rule for finding the Points of contrary Flexure is corrected in a ſimilar Manner. Such a Point is not always formed when the 2d Fluxion of the Ordinate vaniſhes; for if it's 3d Fluxion likewiſe vaniſhes, and it's 4th Fluxion be real, the Curve may have it's Cavity turned all one Way. The ſame is to be ſaid, when it's Fluxions of the ſubſequent ſucceſſive Orders vaniſh, if the Number of all thoſe that vaniſh be even. Other Theorems are ſubjoined relating to this Subject.

Chap. X. treats of the Aſymptotes of Lines, the Areas bounded by them and the Curves, the Solids generated by theſe Areas of ſpiral Lines, and the Limits of the Sums of Progreſſions. The Analogy there is betwixt theſe Subjects, induced the Author to treat of them in one Chapter, and illuſtrate them by one another. He begins with three of the moſt ſimple Inſtances of Figures that have Aſymptotes. In the common Hyperbola, the Ordinate is reciprocally as the Baſe, and therefore decreaſes while the Baſe increaſes, but never vaniſhes, becauſe the Rectangle contained by it and the Baſe is always a given Area, and it is aſſignable at any aſſignable Diſtance, how great ſoever. The Points of the Conchoid are determined by drawing right Lines from a given Center, and upon theſe produced from the Aſymptote, taking always a given right Line; ſo that the Curve never meets the Aſymptote, but continually approaches to it, becauſe of the greater and greater Obliquity of this right Line. The 3d is the Logarithmic Curve, wherein the Ordinates, at equal Diſtances, decreaſe in Geometrical Proportion, but never vaniſh, becauſe each Ordinate is in a given *Ratio* to the preceding Ordinate. Geometrical Magnitude is always underſtood to conſiſt of

Parts;

Parts; and to have no Parts, or to have no Magnitude, are confidered as equivalent in this Science *. There is, however, no Neceffity for confidering Magnitude as made up of an infinite Number of fmall Parts; it is fufficient, that no Quantity can be fuppofed to be fo fmall, but it may be conceived to be diminifhed further; and it is obvious, that we are not to eftimate the Number of Parts that may be conceived in a given Magnitude, by thofe which in particular determinate Circumftances may be actually perceived in it by Senfe; fince a greater Number of Parts become vifible in it by varying the Circumftances in which it is perceived.

It is hardly poffible to give a tolerable Extract of this or the following Chapters, without Diagrams and Computations: We fhall therefore obferve only, that after giving fome plain and obvious Inftances, wherein a Quantity is always increafing, and yet never amounts to a certain finite Magnitude (as, while the Tangent increafes, the Arc increafes, but never amounts to a Quadrant); this is applied fucceffively to the feveral Subjects mentioned in the Title of the Chapter. Let the Figure be concave towards the the Bafe, and fuppofe it to have an Afymptote parallel to the Bafe; in this Cafe the Ordinate always increafes while the Bafe is produced, but never amounts to the Diftance between the Afymptote and the Bafe. In like manner a curvilineal Area, in a fecond Figure, may increafe, while the Bafe is produced, and approach continually to a certain finite Space, but never amount to it: This is always the Cafe, when the Ordinate of this latter Figure is to a given right Line, as the Fluxion of the Ordinate of the former is to the Fluxion of the Bafe; and of this various Examples are given. A Solid may increafe in the fame Manner, and yet never amount to a given Cube or Cylinder, when the Square of the Ordinate of the latter Figure is to a given Square, as the Fluxion of the Ordinate of the firft Figure is to the Fluxion of the Bafe. A Spiral may in like manner approach to a Point continually, and yet in any Number of Revolutions never arrive at it; and there are Progreffions of Fractions that may be continued at Pleafure, and yet the Sum of the Terms may be always lefs than a given Number. Various Rules are demonftrated, and illuftrated by Examples, for determining when a Figure has an Afymptote parallel or oblique to the Bafe; when the Area terminated by the Curve and the Afymptote has a Limit which it never exceeds, or may be produced till it furpafs any affignable Space; when the Solid generated by that Area, the Surface generated by the Perimeter of the Curve, the fpiral Area generated by the revolving Ray, the fpiral Line itfelf, or the Sum of the Terms of a Progreffion, have fuch Limits or not; and for meafuring thofe Limits. The Author infifts on thefe Subjects, the rather that they are commonly defcribed in very myfterious Terms, and have been the moft fertile of Paradoxes of any

* See *Euclid's* Elements, Def. I. Lib. I.

Parts

Parts of the higher Geometry. These Pradoxes, however, amount to no more than this: That a Line or Number may be continually acquiring Increments, and those Increments may decrease in such a Manner, that the whole Line or Number shall never amount to a given Line or Number. The Necessity of admiting this is obvious enough, and is here shewn from the Nature of the most common geometrical Figures in Art. 292, 293, *&c.* and from any Series of Fractions that decrease continually, in Art. 354, 355, *&c.*

Chap. XI. treats of the Curvature of Lines, it's Variation, the Degrees of Contact of the Curve and Circle of Curvature, and of various Problems that depend on the Curvature of Lines. This Subject is treated fully, because of it's extensive Usefulness, and because in this consists one of the greatest Advantages of the modern Geometry above that of the Antients. The Author on this, as former Occasions, begins by premising the necessary Definitions. Curve Lines touch each other in a Point, when the same right Line is their common Tangent at that Point; and that which has the closest Contact with the Tangent, or passes betwixt it and the other Curve through the Angle of Contact formed by them, being less inflected from the Tangent, is therefore less curve. Thus a greater Circle has a less Curvature than a lesser Circle; and since the Curvature of Circles may be varied indefinitely, by inlarging or diminishing their Diameters, they afford a Scale by which the Curvature of other Lines may be measured. As the Tangent is the right Line which touches the Arc so closely, that no other right Line can be drawn between them; so the Circle of Curvature is that which touches the Curve so closely, that no other Circle can be drawn through the Point of Contact between them. As the Curve is separated from it's Tangent in consequence of it's Flexure or Curvature, so it is separated from the Circle of Curvature in consequence of the Variation of it's Curvature; which is greater or less, according as it's Flexure from that Circle is greater or less.

The Tangent of the Figure being considered as the Base, a new Figure is imagined, whose Ordinate is a third Proportional to the Ordinate and Base of the first. This new Figure determines the Chord of the Circle of Curvature, by it's Intersection with the Ordinate at the Point of Contact, and by the Tangent of the Angle in which it cuts that Circle, measures the Variation of Curvature. The less this Angle is, the closer is the Contact of the Curve and Circle of Curvature, of which there may be indefinite Degrees. When the Figure proposed is a conic Section, the new Figure is likewise a conic Section; and it is a right Line when the first Figure is a *Parabola*, and the Ordinates are parallel to the Axis; or when the first Figure is an *Hyperbola*, and the Ordinates are parallel to either Asymptote. Hence the Curvature and it's Variation in a conic Section are determined by several Constructions; and, amongst other Theorems, it is shewn, that the Variation of Curvature at any Point of a conic Section is as the Tangent of the

Angle

Angle contained by the Diameter which paſſes through that Point, and by the Perpendicular to the Curve.

When the Ordinate at the Point of Contact is an Aſymptote to the new Figure, the Curvature is leſs than in any Circle; and this is the Caſe in which it is ſaid to be infinitely little, or the Ray of Curvature is ſaid to be infinitely great. Of this Kind is the Curvature at the Points of contrary Flexure in the Lines of the third Order. When the new Figure paſſes through the Point of Contact, the Curvature is greater than in any Circle, or the Ray of Curvature vaniſhes; and in this Caſe the Curvature is ſaid to be infinitely great. Of this Kind is the Curvature at the Cuſpids of the Lines of the third Order.

As Lines which paſs through the ſame Point have the ſame Tangent when the firſt Fluxions of the Ordinate are equal, ſo they have the ſame Curvature when the ſecond Fluxions of the Ordinate are likewiſe equal; and half the Chord of the Circle of Curvature that is intercepted between the Points wherein it interſects the Ordinate, is a third Proportional to the right Lines that meaſure the ſecond Fluxion of the Ordinate and firſt Fluxion of the Curve, the Baſe being ſuppoſed to flow uniformly. When a Ray revolving about a given Point, and terminated by the Curve, becomes perpendicular to it, the firſt Fluxion of the Ray vaniſhes; and if it's ſecond Fluxion vaniſhes at the ſame time, that Point muſt be the Center of Curvature. The ſame is to be ſaid when the angular Motion of the Ray about that Point is equal to the angular Motion of the Tangent of the Curve; as the angular Motion of the *Radius* of a Circle about it's Center is always equal to the angular Motion of the Tangent of the Circle. Thus the various Properties of the Circle ſuggeſt various Theorems for determining the Center of the Curvature.

Becauſe Figures are often ſuppoſed to be deſcribed by the Interſections of right Lines revolving about given Poles, three Theorems are given in Prop. 18. 26. and 35. for determining the Tangents, Aſymptotes, and Curvature of ſuch Lines, from the Deſcription, which are illuſtrated by Examples. A new Property of Lines of the third Order is ſubjoined to Prop. 35. The Evolution of Lines is conſidered in Prop. 36. The Tangents of the *Evoluta* are the Rays of Curvature of the Line which is deſcribed by it's Evolution; and the Variation of Curvature in the latter, is meaſured by the *Ratio* of the Ray of Curvature of the former to the Ray of Curvature of the latter.

Sir *I. Newton*, in a Treatiſe lately publiſhed, meaſures the Variation of the Curvature by the *Ratio* of the Fluxion of the Ray of Curvature to the Fluxion of the Curve; and is followed by the Author, to avoid the Perplexity which a Difference in Definitions occaſions to Readers, though he hints (in Art. 386.) that this *Ratio* gives rather the Variation of the Ray of Curvature, and that it might have been proper to have meaſured the Variation of Curvature rather by the *Ratio* of the Fluxion of the Curvature itſelf to the Fluxion of the Curve; ſo that

that the Curvature being inverſely as the Ray of Curvature, and conſequently it's Fluxion as the Fluxion of the Ray itſelf directly, and the Square of the Ray inverſely, it's Variation would have been directly as the Meaſure of it, according to Sir *I. Newton*'s Definition, and inverſely as the Square of the Ray of Curvature: According to this Explication, it would have been meaſured by the Angle of Contact contained by the Curve and Circle of Curvature, in the ſame Manner as the Curvature itſelf is meaſured by the Angle of Contact contained by the Curve and Tangent. The Ground of this Remark will better appear from an Example: According to Sir *I. Newton*'s Explication, the Variation of Curvature is uniform in the Logarithmic Spiral, the Fluxion of the Ray of Curvature in this Figure being always in the ſame *Ratio* to the Fluxion of the Curve; and yet while the Spiral is produced, though it's Curvature decreaſes, it never vaniſhes; which muſt appear ſtrange to ſuch as do not attend to the Import of his Definition.——It is eaſy, however, to derive one of theſe Meaſures of this Variation from the other, and becauſe Sir *I. Newton*'s is (generally ſpeaking) aſſigned by more ſimple Expreſſions, the Author has the rather conformed to it in this Treatiſe, but thought it neceſſary to give the Caution we have mentioned.

The greateſt Part of this Chapter is employed in treating of uſeful Problems, that have a Dependance on the Curvature of Lines. Firſt, the Properties of the Cycloid are briefly demonſtrated, with the Application of this Doctrine to the Motion of Pendulums, by ſhewing that when the Motion of the generating Circle along the Baſe is uniform, and therefore may meaſure the Time, the Motion of the Point that deſcribes the Cycloid, is ſuch as would be acquired by a heavy Body deſcending along the cycloidal Arc, the Axis of the Figure being ſuppoſed perpendicular to the Horizon. In the next place, the Cauſtics, by Reflexion and Refraction, are determined. If Perpendiculars be always drawn from the radiating Point to the Tangents of the Curve, and a new Curve be ſuppoſed to be the *Locus* of the Interſections of the Perpendiculars and Tangents, then the Line, by the Evolution of which that new Curve can be deſcribed, is ſimilar and ſimilarly ſituated to the Cauſtic by Reflection. The Doctrine of centripetal Forces is treated at Length from Art. 416. to 493.

Firſt, a Body is ſuppoſed to deſcend freely by it's Gravity in a vertical Line; and becauſe the Gravity is the Power which accelerates the Motion of the Body, it muſt be meaſured by the Fluxion of it's Velocity, or the ſecond Fluxion of the Space deſcribed by it. When the vertical Line is ſuppoſed to move parallel to itſelf with an uniform Motion, the Body will deſcend in it in the ſame Manner as before; and the Gravity will be ſtill meaſured by the ſecond Fluxion of the Deſcent, or the ſecond Fluxion of the Ordinate of the Curve that is traced in this Caſe by the Body on an immoveable Plain, and therefore is as the Square of the Velocity (which is meaſured by the Fluxion of the

Curve)

Curve) directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely, by a Proposition mentioned above. When the Gravity acts uniformly, and in parallel Lines, the Projectile, in describing any Arc, falls below the Tangent drawn at the Beginning of the Arc, as much as if it had fallen perpendicularly in the Vertical; and the Time being given, the Gravity may be measured by the Space which is the Subtense of the Angle of Contact. In other Cases, when the Gravity varies, or it's Direction changes, it may be measured at any Point by the Subtense of the Angle of Contact, that would have been generated in a given Time, if the Gravity had continued to act uniformly in parallel Lines from that Term, that is, by the Subtense of the Angle of Contact in the Parabola that has it's Diameter in the Direction of the Force, and has the closest Contact with the Curve; which leads us to the same Theorem as before.

In general, let the Gravity (that results from the Composition of any Number of centripetal Forces, which are supposed to act on the Body in one Plane) be resolved into a Force parallel to the Ordinates, and a Force parallel to the Base; then the former shall be measured by the second Fluxion of the Ordinate, and the latter by the second Fluxion of the Base, the Time being supposed to flow uniformly, so that the Velocity of the Body may be measured by the Fluxion of the Curve. When the Trajectory is not in one Plane, the Force is resolved in a similar Manner into three Forces, which are measured by three second Fluxions analogous to them.

Whether the Body move in a Void, or in a Medium that resists it's Motion; the Gravity that results from the Composition of the centripetal Forces which act upon the Body, is always as the Square of it's Velocity directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely.

When a Body describes any Trajectory in a Void or in a Medium, by a Force directed to one given Center, the Velocity at any Point of the Trajectory is to the Velocity by which a Circle could be described in a Void about the same Center, at the same Distance, by the same Gravity, in the subduplicate *Ratio* of the angular Motion of the Ray drawn always from the Body to the Center, to the angular Motion of the Tangent of the Trajectory: And, if there be no Resistance, the Velocity in the Trajectory at any Point, is the same that would be acquired by the Body, if it was to fall from that Point through one fourth of the Chord of the Circle of Curvature that is in the Direction of the Gravity, and the Gravity at that Point was to be continued uniformly during it's Descent.

If the centripetal Force be inversely as any Power of the Distance whose Exponent is any Number m greater than Unit, there is a certain Velocity (*viz.* that which is to the Velocity in a Circle at the same Distance as $\sqrt{2}$ to $\sqrt{m-1}$) which would be just sufficient to carry off the Body upwards in a vertical Line, so as that it should continue to aſcend for ever, and never return towards the Center. If the Body be projected in any other Direction with the ſame Velocity, it will deſcribe a Trajectory which is here conſtructed: It is a *Parabola* when $m = 2$, a Logarithmic Spiral when $m = 3$, an Epicycloid when $m = 4$, a Circle that paſſes through the Center of the Forces when $m = 5$, and the *Lemniſcata* when $m = 7$. In general, it is conſtructed by drawing a Perpendicular from the Center of the Forces to a right Line given in Poſition, and any other Ray to the ſame right Line, then increaſing or diminiſhing the Angle contained by this Ray and the Perpendicular in the given *Ratio* of 2 to the Difference between 3 and m, and increaſing or diminiſhing the Logarithm of the Ray in the ſame given *Ratio.* The Trajectories deſcribed in analogous Caſes by centrifugal Forces, are conſtructed in a ſimilar Manner. Theſe are the Figures in which the Perpendicular, from a given Center on the Tangent, is always as ſome Power of the Ray drawn from the ſame Center to the Point of Contact, which are afterwards found to ariſe in the Reſolution of the moſt ſimple Caſes of Problems of various Kinds.

When the Area deſcribed about the Center of an Ellipſe is given, the Subtenſe of the Angle of Contact, drawn through one Extremity of the Arc parallel to the Semidiameter drawn to the other Extremity is in a given *Ratio* to this Semidiameter; and therefore, when an Ellipſe is deſcribed by a Force directed towards the Center, that Force is always as the Diſtance from the Centre. When the Force is directed toward the *Focus*, it is inverſely as the Square of the Diſtance. And theſe two Caſes are conſidered particularly, becauſe of their Uſefulneſs in the true Theory of Gravity. To illuſtrate which, the Laws of centripetal Forces that would cauſe a Body to deſcend continually toward the Center, or aſcend from it, are diſtinguiſhed from thoſe which cauſe the Body to approach towards the Center, and recede from it by Turns. A Body approaches from the higher Apſid toward the Center, when it's Velocity is leſs than what is requiſite to carry it in a Circle; and if it's Velocity increaſe, while it deſcends, in a higher Proportion than the Velocities requiſite to carry Bodies in Circles about the ſame Center, the Velocity in the lower Part of the Curve may exceed the Velocity in a Circle at the ſame Diſtance, and thereby become ſufficient to carry off the Body again. But while the Diſtance decreaſes, if the Velocities in Circles increaſe in the ſame or in a higher Proportion, than the Velocity in a Trajectory can increaſe, the Body muſt either continually approach toward the Center, if it once begin to approach to it, or recede continually from the Center, if it once begin to aſcend from it; and this is the Caſe, when the centripetal Force increaſes as the Cube of the Diſtance decreaſes, or in a higher Proportion. But though, in ſuch Caſes, the Body approach continually towards the Center, we are not to conclude, that it will always approach to it till it fall into it, or come within any given Diſtance; for it is demonſtrated afterwards in Art. 879 and 880, that it may approach to the Center for ever, in a

Spiral that never descends to a given Circle described in the same Plane, and that it may recede from it for ever in a Spiral that never arises to a given Altitude. An Example of each Case is given when the centripetal Force is inversely as the fifth Power of the Distance.

When the Trajectory is described in a *Medium*, let z be to a given Magnitude as the centripetal Force is to the Force by which the same Trajectory could be described in a Void; and if the Area be supposed to flow uniformly, the Resistance will be in the compound *Ratio* of the Fluxion of z, and of the Fluxion of the Curve; and the Density of the Medium (supposing the Resistance to be in the compound *Ratio* of the Density and of the Square of the Velocity) shall be as the Fluxion of the Logarithm of z directly, and the Fluxion of the Curve inversely. Hence, when any Figure that can be described in a Void by a Force that varies according to any Power of the Distance from the Center, is described in a Medium, the Density of the Medium must be inversely as the Tangent of the Figure bounded by a Perpendicular at the Center to the Ray drawn from it to the Point of Contact.

After giving some Properties of the Trajectories that are described by a Body when it gravitates in right Lines perpendicular to a given Surface, and their Application to optical Uses, the Author proceeds to consider the Motion of a Body that gravitates towards several Centers. In such Cases, that Surface is said to be horizontal, which is always perpendicular to the Direction of the Gravity that results from the Composition of the several Forces; and it is shewn, that the Velocity which is acquired by descending from one horizontal Surface to another, is always the same (whether the Body move in right Lines, or in any Curves); the Square of which is measured by the Aggregate of several Areas which have the Distances from the respective Centers for their Bases, and right Lines proportional to the Forces at these Distances for their Ordinates.

The Force which acts upon the Moon is resolved into a Force perpendicular to the Plane of the Ecliptic, and a Force parallel to it. This last is again resolved into that which is parallel to the Line of the *Syzigies*, and that which is parallel to the Line joining the Quadratures. The first measures the second Fluxion of the Distance of the Moon from that Plane, the second and third measure the second Fluxions of her Distances from the Line of the Quadratures, and from the Line of the *Syzigies*, respectively. Hence a Construction is derived of the Trajectory which would be described by the Moon about the Earth, in consequence of their unequal Gravitation towards the Sun, if the Gravity of the Moon towards the Earth was as her Distance from it. From this a Computation is deduced of the Motion of the Nodes of the Moon, and of the Variation of the Inclination of the Plane of her Orbit, which we cannot describe here. It is sufficient to observe, that these Motions are found to agree nearly with those which have been deduced from other Theories, and from Astronomical Observations.

A Fluid being ſuppoſed to gravitate towards two given Centers with equal and invariable Forces, it is ſhewn, that the Figure of the Fluid muſt be that of an oblong Spheroid, and that thoſe two Centers muſt be the *Foci* of the generating Ellipſe. The Nature of the Figure is alſo ſhewn, when the Fluid gravitates towards ſeveral Centers, or when it revolves on it's Axis; but theſe are mentioned briefly, becauſe ſuch Theories are of little or no Uſe for diſcovering the Figures of the Planets.

In Chap. XII, the Author proceeds to conſider the more conciſe Methods, by which the Fluxions of Quantities are uſually determined, and to deduce general Theorems more immediately applicable to the Reſolution of Geometrical and Philoſophical Problems. In the Method of Infiniteſimals, the Element by which any Quantity increaſes or decreaſes, is ſuppoſed to become infinitely ſmall, and is generally expreſſed by two or more Terms, ſome of which become infinitely leſs than the reſt, and therefore being neglected as of no Importance, the remaining Terms form what is called the *Difference* of the Quantity propoſed. The Terms that are neglected in this Manner are the very ſame which ariſe in conſequence of the Acceleration or Retardation of the generating Motion, during the infinitely ſmall Time in which the Element is generated; and therefore theſe Differences are in the ſame *Ratio* to each other as the generating Motions or Fluxions. Hence the Concluſions in this Method are accurately true, without even an infinitely ſmall Error, and agree with thoſe that are deduced by the Method of Fluxions.

It is uſual in this Method to conſider a Curve as a Polygon of an infinite Number of Sides, which, being produced, give the Tangents of the Curve, and, by their Inclination to each other, meaſure it's Curvature. But it is neceſſary in ſome Caſes, if we would avoid Error, to reſolve the Element of the Curve into ſeveral infinitely ſmall Parts, or even ſometimes into Infiniteſimals of the ſecond Order; and Errors that might otherwiſe ariſe in it's Application, may, with due Care, be corrected by a proper Uſe of this Method itſelf, of which ſome Inſtances are given. If we were to ſuppoſe, for Example, the leaſt Arc that can be deſcribed by a Pendulum to coincide with it's Chord, the Time of the Vibration derived from this Suppoſition will be found erroneous; but by reſolving that Arc into more and more infinitely ſmall Parts, we approach to the true Time in which it is deſcribed. By ſuppoſing the Tangent of the Curve to be the Production of the rectilineal Element of the Curve, the Subtenſe of the Angle of Contact is found equal to the ſecond Difference or Fluxion of the Ordinate; but in this Inquiry, the Tangent ought to be ſuppoſed to be equally inclined to the two Elements of the Curve that terminate at the Point of Contact; and then the Subtenſe of the Angle of Contact will be found equal to half the ſecond Difference of the Ordinate, which is it's true Value.

Sir

Sir *I. Newton*, however, inveſtigates the Fluxions of Quantities in a more unexceptionable Manner. He firſt determines the finite ſimultaneous Increments of the Fluents, and, by comparing them, inveſtigates the *Ratio* that is the Limit of the various Proportions which they bear to each other, while he ſuppoſes them to decreaſe together till they vaniſh. When the generating Motions are variable, the *Ratio* of the ſimultaneous Increments that are generated from any Term, is expreſſed by ſeveral Quantities, ſome of which ariſe from the *Ratio* of the generating Motions at that Term, and others from the ſubſequent Acceleration or Retardation of theſe Motions. While the Increments are ſuppoſed to be diminiſhed, the former remain invariable, but the latter decreaſe continually, and vaniſh with the Increments; and hence the Limit of the variable *Ratio* of the Increments (or their ultimate *Ratio*) gives the preciſe *Ratio* of the generating Motions or Fluxions. Moſt of the Propoſitions in the preceding Chapters may be more briefly demonſtrated by this Method, (of which ſeveral Examples are given) and the Author makes always uſe of it in the Sequel of this Book.

It is one of the great Advantages of this Method, that it ſuggeſts general Theorems for the Reſolution of Problems, which may be readily applied as there is Occaſion for them. Our Author proceeds to treat of theſe, and firſt of ſuch as relate to the Center of Gravity and it's Motion. In any Syſtem of Bodies, the Sum of their Motions, eſtimated in a given Direction, is the ſame as if all the Bodies were united in their common Center of Gravity. If the Motion of all the Bodies is uniform and rectilineal, the Center of Gravity is either quieſcent, or it's Motion is uniform and rectilineal. When Action is equal to Reaction, the State of the Center of Gravity is never affected by the Colliſions of the Bodies, or by their attracting or repelling each other mutually. It is not, however, the Sum of the abſolute Motions of the Bodies that is preſerved invariable in conſequence of the Equality of the Action and Reaction, as they ſeem to imagine, who tell us, that this Sum is unalterable by the Colliſions of Bodies, and that this follows ſo evidently from the Equality of Action and Reaction, that to endeavour to demonſtrate it, would ſerve only to render it more obſcure. On this Occaſion the Author illuſtrates an Argument which he had propoſed in a Piece that obtained the Prize propoſed by the *Royal Academy of Sciences* at *Paris* in 1724, againſt the Menſuration of the Forces of Bodies by the Square of the Velocities, ſhewing that if this Doctrine was admitted, the ſame Power or Agent, exerting the ſame Effort, would produce more Force in the ſame Body when in a Space carried uniformly forwards, than if the Space was at Reſt; or that Springs acting equally on two equal Bodies in ſuch a Space, would produce unequal Changes in the Forces of thoſe Bodies.

Various Problems concerning the Colliſion of Bodies are reſolved in a more general Manner than uſual. Mr *Bernouilli* had determined the Motions when the Elaſticity is perfect, and one Body ſtrikes two equal Bodies

Bodies in Directions that form equal Angles with it's Direction; or when there are any Number of Bodies impelled by it on one Side in various Directions, providing equal Bodies be impelled by it on the other Side, in Directions equally inclined to it's own Direction. But the Problem is resolved here without these Limitations; some others of this Kind are subjoined, and this Doctrine is applied for determining the Motions of Bodies that act upon each other while they descend by their Gravity.

The general Principle derived from these Inquiries, is, that if there be no Collision, or sudden Communication of Motion from one Body to another, while they descend together, and in any case, if the Elasticity be perfect, the Sum of the Products, when each Body is multiplied by the Square of the Velocity acquired by it, is the same as if all the Bodies had descended freely from the same respective Altitudes to their several Places; only in collecting that Sum, if any Body is made to ascend, the Product of it multiplied by the Square of it's Velocity is to be subducted: And if the Bodies be supposed to ascend from their Places with the respective Velocities acquired by them, then their common Center of Gravity will rise to the same Level from which it descended. In other Cases, however, the Ascent of the Center of Gravity will be less than it's Descent, but is never greater.

After demonstrating the usual Rule for finding the Center of Oscillation, the Author treats of the Motion of Water issuing from a cylindric Vessel. The Effect of the Gravitation of the whole Mass of Water is considered as threefold. It accelerates, for some time at least, the Motion with which the Water in the Vessel descends; it generates the Excess of the Motion with which the Water issues at the Orifice above the Motion which it had in common with the rest of the Water; and it acts on the Bottom of the Vessel at the same Time. Then supposing the last two Parts of the Force to be in any invariable *Ratio* to each other, when the Diameters of the Base and Orifice are given, he determines by Logarithms the Velocity with which the Water issues at the Orifice; and shews that this Velocity will approach very near to it's utmost Limit, in an exceeding small Time. When the Water is supposed to be supplied in a Cylinder, so as to stand always at the same Altitude above the Orifice, there is an Analogy between the Acceleration of the Motion of the Water that issues at the Orifice, and the Acceleration of a Body that descends by it's Gravity in a Medium which resists in the duplicate *Ratio* of the Velocity. For when the utmost Velocities, or Limits, are equal in those two Cases, the Time in which the issuing Water acquires any lesser Velocity, is to the Time in which the descending Body acquires the same Velocity as the Area of the Orifice to the Area of the Base; and if a cylindric Column be supposed to be erected on the Orifice equal to the Quantity of Water that issues at the Orifice in the former of those Times, the Height of this Column will be to the Space described by the descending Body in the latter Time,

Time, in the ſame *Ratio* as the Orifice to the Area of the Baſe. The *Ratio* of the Force that acts on the Bottom of the Veſſel to the Force that generates the Motion of the Water iſſuing at the Orifice, is deduced from Sir *I. Newton*'s Cataract, and is the ſame that follows from the Principle concerning the Equality of the Aſcent and Deſcent of the Center of Gravity, which was firſt applied to this Inquiry by Mr *Daniel Bernouilli* Comment. *Acad. Petrop. Tom.* 2. But there are ſeveral Precautions to be taken in applying this Doctrine.

After ſome other Theorems concerning the Center of Gravity, and ſeveral Obſervations concerning the Curvature of Lines, and the Angles of Contact; the Author repreſents four general Propoſitions in one View, that the Analogy between them may appear. The 1ſt gives the Property of the Trajectories that are deſcribed by any centripetal Forces, how variable ſoever theſe Forces, or their Directions, may be. The 2d gives a like general Property of the Lines of ſwifteſt Deſcent. The 3d gives the Property of the Line that is deſcribed in leſs Time than any other of an equal-Perimeter. And the 4th gives the Property of the Figure that is aſſumed by a flexible Line or Chain, in conſequence of any ſuch Forces acting upon it. If we ſuppoſe a Body to ſet out from any Point in the Trajectory, or in the Line of ſwifteſt Deſcent, with the Velocity which it has acquired there, and to move in the right Line which is the Direction of the Gravity, that reſults from the Compoſition of the centripetal Forces, then ſhall it's Velocity, and it's Diſtance from the Point where the Perpendicular from the Center of Curvature meets that right Line, flow *proportionally*, *i. e.* the Fluxion of the Velocity (or of the right Line that meaſures it) ſhall be to the Velocity as the Fluxion of that Diſtance is to the Diſtance. When the Velocity and Direction of the Motion is the ſame in the Line of ſwifteſt Deſcent as in the Trajectory, their Curvature is the ſame. Thus in the common Hypotheſis of Gravity, the Curvature in the Cycloid, the Line of ſwifteſt Deſcent, is the ſame as the *Parabola* deſcribed by a Projectile, if the Velocities in thoſe Lines be equal, and their Tangents be equally inclined to the Horizon. In order to find the Nature of the *Catenaria* in any Hypotheſis of Gravity, ſuppoſe the Gravity to be increaſed or diminiſhed in the ſame Proportion as the Thickneſs of the Chain varies, and to have it's Direction changed into the oppoſite Direction; then imagine a Body to ſet out with a juſt Velocity from a given Point in the Chain, and to deſcribe the Curve. The Tenſion of the Chain at any Point will be always as the Square of the Velocity acquired at that Point, and if a Body be projected with this Velocity in the Direction of the Tangent, the Curvature of the Trajectory deſcribed by it will be one half of the Curvature of the Chain at that Point. We muſt refer to the Book for a fuller Account of theſe and of other Theorems.

In Chap. XIII. the Problems concerning the Lines of ſwifteſt Deſcent, the Figures which amongſt all thoſe that have equal Perimeters produce

produce *Maxima* or *Minima*, and the Solid of leaſt Reſiſtance, are reſolved without Computations, from the firſt Fluxions only. There are alſo eaſy ſynthetic Demonſtrations ſubjoined, becauſe this Theory is commonly eſteemed of an abſtruſe Nature, and Miſtakes have been more frequently committed in the Proſecution of it, than of any other relating to Fluxions. To give ſome Idea of the Author's Method, ſuppoſe the Gravity to act in parallel Lines, *a* to denote the Velocity acquired at the lowermoſt Point of the Curve, and *u* the Velocity acquired at any other Point of the Curve. Suppoſe the Element of the Curve to be deſcribed by this Velocity *u*, but the Element of the Baſe to be always deſcribed by the conſtant Velocity *a*. Then it is eaſily demonſtrated without any Computation, that the Element of the Ordinate being given, the Difference of the Times in which the Elements of the Curve and Baſe are thus deſcribed is a *Minimum*, when the *Ratio* of thoſe Elements is that of *a* to *u*; *i. e.* when the Sine of the Angle, in which the Ordinate interſects the Curve, is to the *Radius* in this *Ratio*. Suppoſing therefore this Property to take Place over all the Curve, the Exceſs of the Time in which it is deſcribed by the Body deſcending along it, above the Time in which the Baſe is deſcribed uniformly with the Velocity *a*, muſt be a *Minimum*; and this latter Time being given, it follows that the Time of Deſcent in this Curve is a *Minimum*. When the Gravity tends to a given Center, ſubſtitute an Arc of a Circle deſcribed from that Center through the lowermoſt Point of the Curve in the Place of the Baſe in the former Caſe; and the Property of the Line of ſwifteſt Deſcent will be diſcovered in the ſame Manner. The Nature of the Line that among all thoſe of the ſame Perimeter is deſcribed in the leaſt Time, is diſcovered with great Facility, by determining from the former Caſe the Property of the Figure when the Sum or Difference of the Time in which it is deſcribed by the deſcending Body, and of the Time in which it would be deſcribed by any given uniform Motion, is a *Minimum*; for the latter Time being the ſame in all Curves of the ſame Length, it follows that the Figure, which has this Property, muſt be deſcribed in leſs Time than any of an equal Perimeter. The general Iſoperimetrical Problems are reſolved, and the Solutions are rendered more general, with like Facility by the ſame Method; which is alſo applied for determining the Property of the Solid of leaſt Reſiſtance, and ſerves for reſolving the Problem, when Limitations are added concerning the Capacity of the Solid, or the Surface that bounds it.

The laſt Chapter of the firſt Book treats chiefly of Gravitation towards Spheroids, of the Figure of the Planets, and of the Tides. The Author, having Occaſion in thoſe Inquiries for ſeveral new Properties of the Ellipſe, begins this Chapter by deriving it's Properties from thoſe of the Circle, by conſidering it as the oblique Section of a Cylinder, or as the Projection of the Circle by parallel Rays upon a Plane oblique to the Circle. In this Manner the Properties are briefly transferred

ferred from the one to the other, becaufe by this Projection the Center of the Circle gives the Center of the Ellipfe; Diameters perpendicular to each other in the Circle with their Ordinates, and the circumfcribed Square, give conjugate Diameters of the Ellipfe with their Ordinates, and the circumfcribed Parallelogram; parallel Lines in the Plane of this Circle are projected by Parallels in the Plane of the Ellipfe that are in the fame *Ratio*; any Area in the former is projected by an Area in the latter, which is in an invariable *Ratio* to it; and concentric Circles give fimilar concentric Ellipfes. It is likewife fhewn how Properties of a certain Kind are briefly transferred from the Circle to any conic Section with the fame Facility.

After demonftrating the Properties of the Ellipfe, it is fhewn, that if the Gravity of any Particle of a Spheroid being refolved into two Forces, one perpendicular to the Axis of the Solid, the other perpendicular to the Plane of it's Equator, then all Particles, equally diftant from the Axis, muft tend towards it with equal Forces; and all Particles at equal Diftances from the Plain of the Equator, gravitate equally towards this Plain; but that the Forces with which Particles at different Diftances from the Axis tend towards it, are as the Diftances; and that the fame is to be faid of the Forces with which they tend towards the Plain of the Equator.

From this it is demonftrated, that when the Particles of a fluid Spheroid of an uniform Denfity gravitate towards each other with Forces that are inverfely as the Squares of their Diftances, and at the fame time any other Powers act on the Particles, either in right Lines perpendicular to the Axis, that vary in the fame Proportion as the Diftances from the Axis, or in right Lines perpendicular to the Plain of the Equator, that vary as their Diftances from it, or when any Powers act on the Particles of the Spheroid, that may be refolved into Forces of this Kind; then the Fluid will be every-where *in Æquilibrio*, if the whole Force that acts at the Pole be to the whole Force that acts at the Circumference of the Equator, as the Semidiameter of the Equator to the Semiaxis of the Spheroid; and that the Forces with which equal Particles at the Surface tend towards the Spheroid, will be in the fame Proportion as Perpendiculars to it's Surface, terminated either by the Plane of the Equator, or by the Axis. Becaufe the centrifugal Force with which any Particle of the Spheroid endeavours to recede from it's Axis, in confequence of the diurnal Rotation, is as the Diftance from the Axis, it appears that if the Earth, or any other Planet, was fluid, and of an uniform Denfity, the Figure which it would affume would be accurately that of an oblate Spheroid generated by an Ellipfis revolving about it's fecond Axis.

Afterwards the Gravity towards an oblate Spheroid is accurately meafured by circular Arcs, not only at the Pole, but alfo at the Equator, and in any intermediate Places; and the Gravity towards an oblong Spheroid is meafured by Logarithms. The Gravity at any Diftance in in the Axis of the Spheroid, or in the Plane of the Equator produced, is likewiſe accurately determined by like Meaſures, without any new Computation or Quadrature, by ſhewing that when two Spheroids have the ſame Center and *Focus*, and are of an uniform Denſity, the Gravities towards them at the ſame Point in the Axis or Plane of the Equator produced, are as the Quantities of Matter in the Solids.

This Theory is applied for determining the Figure of the Earth, by comparing the Force of Gravity in any given Latitude, derived from the Length of a *Pendulum* that vibrates there in a Second of Time, with the centrifugal Force at the Equator, deduced from the periodic Time of the diurnal Rotation, and the Amplitude of a Degree of the Meridian; or by comparing the Lengths of *Pendulums* that vibrate in equal Times in given unequal Latitudes; or by comparing different Degrees meaſured upon the Meridian. By the beſt Obſervations it would ſeem, that there is a greater Increaſe of Gravitation, and of the Degrees of the Meridian from the Equator towards the Poles, than ought to ariſe from the Suppoſition of an uniform Denſity. Therefore the Author ſuppoſes the Denſity to vary from the Surface towards the Center; and, in ſeveral Caſes he has conſidered, he finds that a greater Denſity towards the Center would account for a greater Increaſe of Gravitation, towards the Poles, but not for a greater Increaſe of the Degrees of the Meridian; and that the Hypotheſis of a leſs Denſity towards the Center would account for the latter, but not for the former, ſuppoſing (after Sir *I. Newton*) the Columns of the Fluid to extend from the Surface to the Center, and there to ſuſtain each other. On this Account he determines the Gravitation towards the Earth, when it is ſuppoſed to be hollow with a *Nucleus* included, according to the Hypotheſis advanced by Dr *Halley*, with the Difference of the Semidiameters that might ariſe from ſuch a Diſpoſition of the Internal Parts. But in this Caſe, and when the Denſity is ſuppoſed variable, the ſpheroidical Figure is only aſſumed as an Hypotheſis. He adds, that by imagining the Denſity to be greater in the Axis than in the Plain of the Equator at equal Diſtances from the Centre, an Hypotheſis perhaps might be found, that would account for moſt of the *Phænomena*; but that a Series of many exact Obſervations is requiſite, before we can examine with any Certainty the various Suppoſitions that may be imagined concerning the internal Conſtitution of the Earth. This Doctrine is likewiſe applied for determining the Figure of *Jupiter*.

It follows from the ſame Theorem, that if we ſuppoſe the Earth to be fluid, and abſtract from it's Motion upon it's Axis, and the Inclination of the right Lines in which it's Particles gravitate towards the Sun or Moon, the Figure which it would aſſume in conſequence of the unequal Gravitation of it's Particles towards either of thoſe Bodies would be accurately that of an oblong Spheroid having it's Axis directed towards that Body. The Aſcent of the Water, deduced from this Theorem, agrees nearly with that which Sir *I. Newton* found, by computing

puting it briefly from what he had demonſtrated concerning the Figure of the Earth. Several Obſervations are ſubjoined concerning the Tides, and the Cauſes which may contribute to increaſe or diminiſh them, particularly the Inequality of the Velocities with which Bodies revolve about the Axis of the Earth in different Latitudes.

This Chapter concludes by demonſtrating briefly, that if the Attraction of the Particles decreaſed as the Cube of their Diſtance increaſes, or in any higher Proportion, then any Particle would tend toward the leaſt Portion of Matter in Contact with it, with a greater Force than towards the greateſt Body at any Diſtance, how ſmall ſoever from it. The true Law of Gravity is better adapted for holding the Parts of each Body in a proper Union, while it perpetuates the Motions in the great Syſtem about the Sun, and preſerves the Revolutions in the leſſer Syſtems nearly regular; and the Author concludes with obſerving, that a remarkable geometrical Simplicity is often found in the Concluſions that are derived from it.

V. 3. In the ſecond Book, he treats of the Method of Computation, or the Algebraic Part; to the Facility, Conciſeneſs, and great Extent of which, the Improvements that have been made by this Method are in great meaſure to be aſcribed. In order to obtain thoſe Advantages, it was neceſſary to admit various Symbols into the Algebra: But the Number and Complication of thoſe Signs muſt occaſion ſome Obſcurity in this Art, unleſs Care be taken to define their Uſe and Import clearly, with the Nature of the ſeveral Operations. An Example of this is given by an Illuſtration of one of the firſt Rules in Algebra. As it is the Nature of Quantity to be capable of Augmentation and Diminution, ſo Addition and Subſtraction are the primary Operations in the Sciences that treat of it. The poſitive Sign implies an Increment, or a Quantity to be added. The negative Sign implies a Decrement, or Quantity to be ſubſtracted: And theſe ſerve to keep in our View what Elements enter into the Compoſition of Quantities, and in which Manner, whether as Increments or Decrements. It is the ſame Thing to ſubſtract a Decrement as to add an equal Increment. As the Multiplication of a Quantity by a poſitive Number implies a repeated Addition of the Quantity, ſo the Multiplication by a negative Number implies a repeated Subſtraction: And hence to multiply a negative Quantity, or Decrement, by a negative Number, is to ſubſtract the Decrement as often as there are Units in this Number, and therefore is equivalent to adding the equal Increment the ſame Number of Times; or, when a negative Quantity is multiplied by a negative Number, the Product is poſitive. When we inquire into the Proportion of Lines in Geometry, we have no Regard to their Poſition or Form; and there is no Ground for imagining any other Proportion betwixt a poſitive and negative Quantity in Algebra, or betwixt an Increment and a Decrement, than that of the abſolute Quantities or Numbers themſelves. The Algebraic Expreſſions, however, are chiefly uſeful, as they

The ſame continued, No. 469. p. 403. *Preſented March* 10, 1742-3.

ſerve to repreſent the Effects of the Operations; and ſuch Expreſſions are not to be ſuppoſed equal that involve equal Quantities, unleſs the Operations denoted by the Signs are the ſame, or have the ſame Effect. Nor is every Expreſſion to be ſuppoſed to repreſent a certain Quantity; for if the $\sqrt{-1}$ ſhould be ſaid to repreſent a certain Quantity, it muſt be allowed to be imaginary, and yet to have a real Square; a way of ſpeaking which it is better to avoid. It denotes only, that an Operation is ſuppoſed to be performed on the Quantity that is under the radical Sign. The Operation is indeed in this Caſe imaginary, or cannot ſucceed; but the Quantity that is under the radical Sign, is not leſs real on that Account. The Author mentions thoſe Things briefly, becauſe they belong rather to a Treatiſe of Algebra than of Fluxions, wherein the common Algebra is admitted.

In order to avoid the frequent Repetition of figurative Expreſſions in the Algebraic Part, the Fluxions of Quantities are here defined to be any Meaſures of their reſpective Rates of Increaſe or Decreaſe, while they are ſuppoſed to vary (or flow) together. Theſe may be determined by comparing the Velocities of Points that always deſcribe Lines proportional to the Quantities, as in the firſt Book; but they may be likewiſe determined, without having Recourſe to ſuch Suppoſitions, by a juſt Reaſoning from the ſimultaneous Increments or Decrements themſelves. While the Quantity A increaſes by Differences equal to a, $2A$ increaſes by Differences equal to $2a$, and (ſuppoſing m and n to be invariable) $\frac{mA}{n}$ increaſes by Differences equal to $\frac{ma}{n}$ and therefore at a greater or leſs Rate than a, in Proportion as m is greater or leſs than n. Thus a Quantity may be always aſſigned that ſhall increaſe at a greater or leſs Rate than A, (*i. e.* ſhall have it's Fluxion greater or leſs than the Fluxion of A) in any Proportion; and a Scale of Fluxions may be eaſily conceived, by which the Fluxions of any other Quantities of the ſame Kind may be meaſured.

Let B be any other Quantity whoſe Relation to A can be expreſſed by any Algebraic Form; and while A increaſes by equal ſucceſſive Differences, ſuppoſe B to increaſe by Differences that are always varying. In this Caſe, B cannot be ſuppoſed to increaſe at any one conſtant Rate; but it is evident, that if B increaſe by Differences that are always greater than the equal ſucceſſive Differences by which $\frac{mA}{n}$ increaſes at the ſame Time, then B cannot be ſaid to increaſe at a leſs Rate than $\frac{mA}{n}$; or if the Fluxion of A be repreſented by a, the Fluxion of B cannot be leſs than $\frac{ma}{n}$. And if the ſucceſſive Diffe-

rences

rences of B be always leſs than thoſe of $\frac{m\,A}{n}$, then ſurely B cannot be ſaid to increaſe at a greater Rate than $\frac{m\,A}{n}$; or the Fluxion of B cannot be ſaid to be greater in this Caſe than $\frac{m\,a}{n}$.

From thoſe Principles the primary Propoſitions in the Method of Fluxions, and the Rules of the direct Method, with the Fundamental Rules of the inverſe Method, are demonſtrated. We muſt be brief in our Account of the Remainder of this Book. The Rule for finding the Fluxion of a Power is not deduced, as uſually, from the Binomial Theorem, but from one that admits of a much eaſier Demonſtration from the firſt Algebraic Elements, *viz.* That when n is any integer poſitive Number, if the Terms E^{n-1}, $E^{n-2}F$, $E^{n-3}F^2$, $E^{n-4}F^3$, ... F^{n-1}, (wherein the Index of E conſtantly decreaſes, and that of F increaſes by the ſame Difference Unit) be multiplied by $E-F$, the Sum of the Products is E^n-F^n; from which it is obvious, that when E is greater than F, then E^n-F^n is leſs than $nE^{n-1}\times\overline{E-F}$ but greater than $nF^{n-1}\times\overline{E-F}$.

The Rules are ſometimes propoſed in a Form ſomewhat different from the uſual Manner of deſcribing them, with a View to facilitate the Computations both in the direct and inverſe Method. Thus, when a Fraction is propoſed, and the Numerator and Denominator are reſolved into any Factors, it is demonſtrated, that the Fluxion of the Fraction divided by the Fraction is equal to the Sum of the Quotients, when the Fluxion of each Factor of the Numerator is divided by the Factor itſelf, diminiſhed by the Quotients that ariſe by dividing in like Manner the Fluxion of each Factor of the Denominator by the Factor.

The Notation of Fluxions is deſcribed in Chap. 2. with the Rules of the direct Method, and the fundamental Rules of the inverſe Method. The latter are comprehended in Seven Propoſitions, Six of which relate to Fluents that are aſſignable in finite Algebraic Terms, and the Seventh to ſuch as are aſſigned by infinite Series. It is in this Place the the Author treats of the Binomial and Multinomial Theorems (becauſe of their Uſe on this Occaſion), and they are inveſtigated by the direct Method of Fluxions. The ſame Method is applied for demonſtrating other Theorems, by which an Ordinate of a Figure being given, and it's Fluxions determined, any other Ordinate and *Area* of the Figure may be computed. The moſt uſeful Examples are deſcribed in this Chapter, by computing the Series's that ſerve for determining the Arc from it's Sine or Tangent, and the Logarithm from it's Number, and converſely the Sine, Tangent, or Secant, from the Arc, and the Number from it's Logarithm.

The inverſe Method is proſecuted farther in Chapter III. by reducing Fluents to others of a more ſimple Form, when they are not aſſignable by a finite Number of Algebraic Terms. When a Fluent can be aſſigned by the Quadrature of the Conic Sections, (and conſequently by circular Arcs or Logarithms) this is conſidered as the ſecond Degree of Reſolution; and this Subject is treated at Length. An Illuſtration is premiſed of the Analogy betwixt Elliptic and Hyperbolic Sectors formed by Rays drawn from the Centers of the Figures: The Properties of the latter are ſometimes more eaſily diſcovered becauſe of their Relation to Logarithms, and lead us in a brief Manner to the analogous Properties of elliptic Sectors, and particularly to ſome general Theorems concerning the Multiplication and Diviſion of circular Sectors or Arcs. When two Points are aſſumed in an Hyperbola, and alſo in an Ellipſis, ſo that the Sectors terminated by the Semi-axis; and the two Semi-diameters, belonging to thoſe Points, are in the ſame given *Ratio* in both Figures, then the Relation betwixt the Semi-axis and the two Ordinates drawn from thoſe Points to the other Axis, is always defined by the ſame, or by a ſimilar Equation in both Figures. This Propoſition ſerves for demonſtrating Mr *Cotes*'s celebrated Theorem, as it is extended by M. *De Moivre*, by which a Binomial or Trinomial is reſolved into it's quadratic Diviſors, and various Fluents are reduced to circular Arcs and Logarithms. The Demonſtrations are alſo rendred more eaſy of the Theorems concerning the Reſolution of a Fraction, that has a multinomial Denominator, into Fractions that have the ſimple or quadratic Diviſors of the Multinomial for their ſeveral Denominators. Theſe Demonſtrations are derived from the Method of Fluxions itſelf, without any foreign Aid; the invariable Coefficients being determined by ſuppoſing the variable Quantity or it's Fluxions to vaniſh.

When a Fluent cannot be aſſigned by the Areas of Conic Sections, it may however be meaſured by their Arcs in ſome Caſes; and this may be conſidered as the third Degree of Reſolution, or the Fluents may be called of the third Order. On this Occaſion, ſome Fluents are found to depend on the Rectification of the Hyperbola and Ellipſis, which have been formerly eſteemed of an higher Kind. The Conſtruction of the elaſtic Curve, with it's Rectification, and the Meaſure of the Time of Deſcent in an Arch of a Circle, are derived from hyperbolic and elliptic Arcs; and the Fluents of this Kind are compared with thoſe of the firſt or ſecond Order by infinite Series. Becauſe there are Fluents of higher Kinds than theſe, the Trajectories abovementioned, which are deſcribed by a centripetal Force, that is, as ſome Power of the Diſtance from a given Centre, when the Velocity of the Projection is that which would be acquired by an infinite Deſcent, or by ſuch a centrifugal Force, and the Velocity is ſuch as would be acquired by flying from the Centre, are employed for repreſenting them. A ſimple Conſtruction of theſe Trajectories had been given above, by drawing Rays

Rays from the Centre to a Right Line given in Poſition, increaſing or diminiſhing the Logarithms of thoſe Rays always in a given *Ratio*, and increaſing or diminiſhing the Angles contained by them and the Perpendicular in the ſame *Ratio*. From any Figure of this Kind, a Series of Figures is derived by determining the Interſections of the Tangents of the Figure with the Perpendiculars from the Centre. Every Series of this Kind gives two diſtinct Sort of Fluents; and any one Fluent being given, all the other Fluents taken alternately from it in the Series depend upon it, or are meaſured by it; but it does not appear, that the Fluents of one Sort can be compared with thoſe of the other Sort, or with thoſe of any different Series of this Kind.

The inverſe Method is proſecuted farther in the 4th Chapter, by various Theorems concerning the Area when the Ordinate is expreſſed by a Fluent, or when the Ordinate and Baſe are both expreſſed by Fluents. The firſt is the XIth Prop. of Sir *I. Newton*'s Treatiſe of Quadratures. In Art. 819, 820, *&c.* the Author ſuppoſes the Ordinate and Baſe to be both expreſſed by Fluents, and ſhews, in many Caſes, that the Area may be aſſigned by the Product of two ſimple Fluents, as of two circular Arcs, or of a circular Arc and a Logarithm. This Subject deſerves to be proſecuted, becauſe the Reſolution of Problems is rendered more accurate and ſimple, by reducing Fluents to the Products of Fluents already known, than by having immediately Recourſe to infinite Series. One of the Examples in Art. 822, may be eaſily applied for demonſtrating, that the Sum of the Fractions which have Unit for their common Numerator, and the Squares of the Numbers 1, 2, 3, 4, 5, 6, *&c.* in their natural Order, for their ſucceſſive Denominators, is one ſixth Part of the Number, which expreſſes the *Ratio* of the Square of the Periphery of a Circle to the Square of it's Diameter; which is deduced by Mr *Euler*, *Comment. Petropol.* Tom. 7. in a different Manner; and other Theorems of this Kind may be demonſtrated from the ſame or like Principles.

The Series that is deduced by the uſual Methods for computing the Area or Fluent, converge in ſome Caſes at ſo ſlow a Rate, as to be of little or no Uſe without ſome farther Artifice. For Example: The Sum of the firſt Thouſand Terms of Lord *Brounker*'s Series for the Logarithm of 2, is deficient in the fifth Decimal. In order therefore to render the Account of the inverſe Method more complete, the Author ſhews how this may be remedied in many Caſes, by Theorems derived from the Method of Fluxions itſelf, which likewiſe ſerve for approximating readily to the Values of Progreſſions, and for reſolving Problems that are commonly referred to other Methods. Thoſe Theorems had been deſcribed in Book I. Art. 352, *&c.* but the Demonſtration and Examples were referred to this Place, as requiring a good deal of Computation. The Baſe being ſuppoſed equal to Unit, and it's Fluxion alſo equal to Unit, let half the Sum of the extreme Ordinates be repreſented by *a*, the Difference of the firſt Fluxions of theſe Ordinates by b, the Difference of their third, fifth, feventh, and higher alternate Fluxions by c, d, e, &c. then the Area fhall be equal to $a - \frac{b}{12} + \frac{c}{720} - \frac{d}{30240} + \frac{e}{1209600} -$, &c. which is the firft Theo- for finding the Area. The reft remaining, let a now reprefent the middle Ordinate, and the Area fhall be equal $a + \frac{b}{24} - \frac{7c}{5760} + \frac{31d}{967680} - \frac{127e}{154828800} +$, &c. And this is the Theorem which the Author makes moft Ufe of. When the feveral intermediate Ordinates reprefent the Terms of a Progreffion, the Area is computed from their Sum, or converfely their Sum is derived from the Area, by Theorems that eafily flow from thefe.

Thefe general Theorems are afterwards applied for finding the Sums of the Powers of any Terms in Arithmetical Progreffion, whether the Exponents of the Powers be pofitive or negative, and for finding the Sums of their Logarithm, and thereby determining the *Ratio* of the *Uncia* of the middle Term of a Binomial of a very high Power to the Sum of all the *Unciæ*. This laft Problem was celebrated amongft Mathematicians fome Years ago, and by endeavouring to refolve it by the Method of Fluxions the Author found thofe Theorems, which give the fame Conclufions that are derived from other Methods. They are likewife applied for computing *Areas* nearly from a few equidiftant Ordinates, and for interpolating the intermediate Terms of a Series, when the Nature of the Figure can be determined, whofe Ordinates are as the Differences of the Terms.

In the laft Chapter, the general Rules derived from the Method of Fluxions for the Refolution of Problems, are defcribed and illuftrated by Examples. After the common Theorems concerning Tangents, the Rules for determining the greateft and leaft Ordinates, with the Points of contrary Flexure, and the Precautions that are neceffary to render them accurate and general, (which were defcribed above) are again demonftrated. Next follow the Algebraic Rules for finding the Center of Curvature, and determining the Cauftics by Reflexion and Refraction, and the centripetal Forces. The Conftruction of the Trajectory is given, which is defcribed by a Force that is inverfely as the 5th Power of the Diftance from the Center, becaufe this Conftruction requires Hyperbolic and Elliptic Arcs, and becaufe a remarkable Circumftance takes Place in this Cafe, (and indeed in an Infinity of other Cafes) which could not obtain in thofe that have been already conftructed by others, *viz.* That a Body may continually defcend in a fpiral Line towards the Center, and yet never approach fo near to it as to defcend to a Circle of a certain *Radius*; and a Body may recede for ever from the Center, and yet never arife to a certain finite Altitude. The Conftruction of the Cafes wherein this obtains is performed

formed by Logarithms or Hyberbolic Areas, the Angles defcribed about the Center being always proportional to the Hyperbolic Sectors, while the Diftances from the Center are directly or inverfely as the Tangents of the Hyperbola at it's Vertex. The Circle is an Afymptote to the Spiral; and this can never be, unlefs the Velocities requifite to carry Bodies in Circles increafe while the Diftances decreafe, (or decreafe while the Diftances increafe) in a higher Proportion than the Velocity in the Trajectory; that is, unlefs the Force be inverfely as a higher Power of the Diftance than the Cube. Next follow Theorems for computing the Time of Defcent in any Arc of a Curve, for finding the Refiftance and Denfity of the Medium when the Trajectory and centripetal Force are given, and for defining the *Catenaria* and Line of fwifteft Defcent in any Hypothefis of Gravity.

Then the ufual Rules are derived from the inverfe Method for computing the Area, the Solid generated by it, the Arc of the Curve, and the Surface defcribed by it revolving about a given Axis. The meridional Parts in a Sphere, and any Spheroid, are determined with the fame Accuracy, and almoft equal Facility. The Attraction of a Spheroid at the Equator, as well as at the Poles, is determined in a more general Manner than in the firft Book, or in a Piece of the Author's publifhed at *Paris* in 1740, which obtained a Part of the Prize propofed by the *Royal Academy of Sciences* for that Year. Several Mechanical Problems are refolved, concerning the Proportion the Power ought to bear to the Weight, that the Engine may produce the greateft Effect in a given Time; and concerning the moft advantageous Pofition of a Plane which moves parallel to itfelf, that a Stream of Air or Water may impel it with the greateft Force, having Regard to the Velocity which the Plane may have already acquired. On this Occafion, it is fhewn, that the Wind ought to ftrike the Sails of a Wind-mill in a greater Angle than that of $54^\circ\ 44'$, againft what has been deduced from the fame Principles by a learned Author. The fame Theory is applied to the Motion of Ships, abftracting from the Lee-way, but having Regard to the Velocity of the Ship; and amongft other Conclufions it appears, that the Velocity of a Veffel of one Sail may be greater with a Side-wind, than when fhe fails directly before the Wind; which, perhaps, may be the Cafe of thofe feen by Captain *Dampier* in the *Ladrone Iflands*, that failed at the Rate of 12 Miles in half an Hour with a Side-wind.

The Remainder of this Chapter is employed in reducing Equations from fecond to firft Fluxions; conftructing the elaftic Curve by the Rectification of the equilateral Hyperbola; determining the Vibrations of Mufical Chords; refolving Problems concerning the *Maxima* and *Minima*, that are propofed with Limitations, relating to the Perimeter of the Figure, it's Area, the Solid generated by this Area, *&c.* with Examples of this Kind concerning the Solid of leaft Refiftance; and concludes with an Inftance of the Theorems by which the Value of the

Ordinate may be determined from the Value of the Area, by common Algebra, and by obſerving, that it is not abſolute, but relative Space and Motion, that is ſuppoſed in the Method of Fluxions.

A general Method of deſcribing Curves, by the Interſection of right Lines moving about Points in a given Plane, by the Rev. Mr William Braikenridge, No. 436. p. 25. *Jan.* &c. 1735.

VI. 1. You have here a general Method of deſcribing Lines of any Order, by means of the Interſection of right Lines about Poles; which is much more ſimple than that of Sir *I. Newton*, and will give a Solution of many very difficult Problems; and I queſtion whether they can be found by any other Principles. I gave only one particular Caſe of this in a geometrical Exercitation printed at *London* in 1733, not thinking it convenient to explain the whole Affair at that Time, tho' I was well acquainted with the Method. It is now three Years ago, that I fell upon the general Theorem, but I had many Reaſons for concealing it; and I was determined to let two Years at leaſt paſs after the Publication of that Exercitation, before this general Method ſhould come into the World. For I did not doubt, but that if any others were in Poſſeſſion of this Invention, they would, upon the Publication of a particular Caſe, eſpecially as they were provoked to it, lay hold of the Opportunity to publiſh their general Method, if they had really diſcovered any.

Fig. 6.

About three given Points A, B, C, as Poles, in any Plane, let there be moved three right Lines, A N S, B O S, C N O, which may interſect one another in the Points S, N, O, and let the two Points of interſection S and N be drawn through the right Lines D K S, R N K given by Poſition; the reſt O will deſcribe a Conic Section. If through the Points, A, B, C, are drawn the right Lines A B, A C, meeting each other in A, and the right Lines R K, D K, given by Poſition in R and M; the Figure deſcribed will paſs through the five Points B, C, K, M, R. And hence appears a new Method of deſcribing a Conic Section through five given Points, much more eaſy than any that have been hitherto invented.

Demonſtrated in Exerc. Geom. *Prop.* 1.

Vid. Exerc. Geom. *Prop.* 3.

Fig. 7.

Let there be moved about four Points A, B, C, D, as Poles, in any Plane, as many right Lines, A N S, B O S, C N O, D P O, three of which A N S, B O S, C N O, may interſect each other in three Points, S, N, O, and let the two Points of Interſection S, N, be drawn through the right Lines d K, R K, given by Poſition, and in the mean time let the right Line D P O, drawn from the fourth Pole O, paſs through the Remainder O, and cut the right Line A N S in P, and that Point P will deſcribe a Line of the third Order.

Demonſtrated in Exerc. Geom. *Prop.* 11.

Through the Poles A, B, D, let the right Lines A B R, D B H, be drawn, meeting each other in B, and the right Lines K R, K d, given by Poſition in R H; the Figure deſcribed by the Motion of the Point P will paſs through the five Points, A, D, H, K, R, of which A will be double. Hence is deduced the Method of deſcribing a Line of the 3d Order through ſeven given Points, one of which is double. For let A, D, H, K, P, M, R, be given, and one of them A muſt be double. Through the two Points H, R, and another K, let the right

Fig. 8.

Lines

Lines H K, R K, be moved, and let the Points A R, and H D, be joined, and let the right Lines A R, H D, be produced, meeting one another in B. Then the right Lines A P N S, A M n s, cutting the right Line K R in N, n, and the right Line H K, in S, s, being drawn through A, and the Points P, M; let the right Lines B S, B s, be drawn through those Points S, s, to B; and through D, to the Points P, M, move the right Lines D P O, D M T, meeting the right Lines B S, B s, in O, T. Let the Points O, N, and T, n be joined, and let the right Lines O N, T n be produced, meeting together in C. Then about the Points A, B, C, D, as Poles, let the right Lines A S, B O, C O, DO, revolve, of which let three A S, B O, C O, intersect each other in the Points S N O, and let two S, N be drawn through the right Lines H K, K R, and in the mean time let the right Line D O always pass through the Remainder O, and cut the right Line A N S in P, and this Intersection P of the right Lines A S, D O, will describe a Line of the third Order passing through seven given Points, A, D, H, K, M, P, R, and doubly through the given A.

Lines also of the third Order are more generally, but less commodiously described after this Manner, which also comprehends the first. About five given Points A, B, C, D, E, as Poles, let as many right Lines A N S, B O S, C N O, D P O E P S revolve, of which let three A N S, B O S, C N O intersect one another in the Points N S O; let two S, N, be drawn through the right Lines given by Position d K, K R; and one S of the two S, N, and the Remainder O, let the right Lines E P S, D P O pass, being drawn through the Poles E, D, and meeting in P: let that Point P describe a Line of the third Order, with a double Point in the Pole E. Fig. 9.

In like Manner may Lines of the fourth order be described. About five given Points joined A, B, C, D, E, as Poles, in any Plane; let as many right Lines, A N S, B Q S, C N O, D P O, E P Q, be moved; of which let three A N S, B Q S, C N O, meet each other in three Points S, N, O; let the two Points of Intersection be drawn thro' the right Lines d K, R K, given by Position, and in the mean time let the right Line D P O, moveable about the fourth Pole D, pass thro' the Remainder O, and cut the right Line A N S, in P; then let the right Line E P Q, drawn from the 5th Pole E, be drawn thro' P, and be produced on both Sides, till it meets the right Lines B Q S, C N O, in Q and W: I say that the Points Q, W, will describe Lines of the 4th Order. Through the Poles A, E, and B, D, let the right Lines A E H, B D F revolve, meeting the right Line d K, given by Position, in H, F, let D and E be joined; and A D being drawn through the Poles D, A, meeting the right Line d K in V; from V let the right Line V B be drawn to the Pole B, and cut the right Line D E in G. The Figure described will pass through the five Points B, E, G, F, H, and triply through the Pole B. Let the right Line A, B, R, be produced through the Poles A, B, and meet the Fig. 10. *Demonstrated from* Exerc. Geom. *Prop.* 11.

 right

right Line K R, given by Poſition in R; the Curve will alſo paſs thro' the Points R, K.

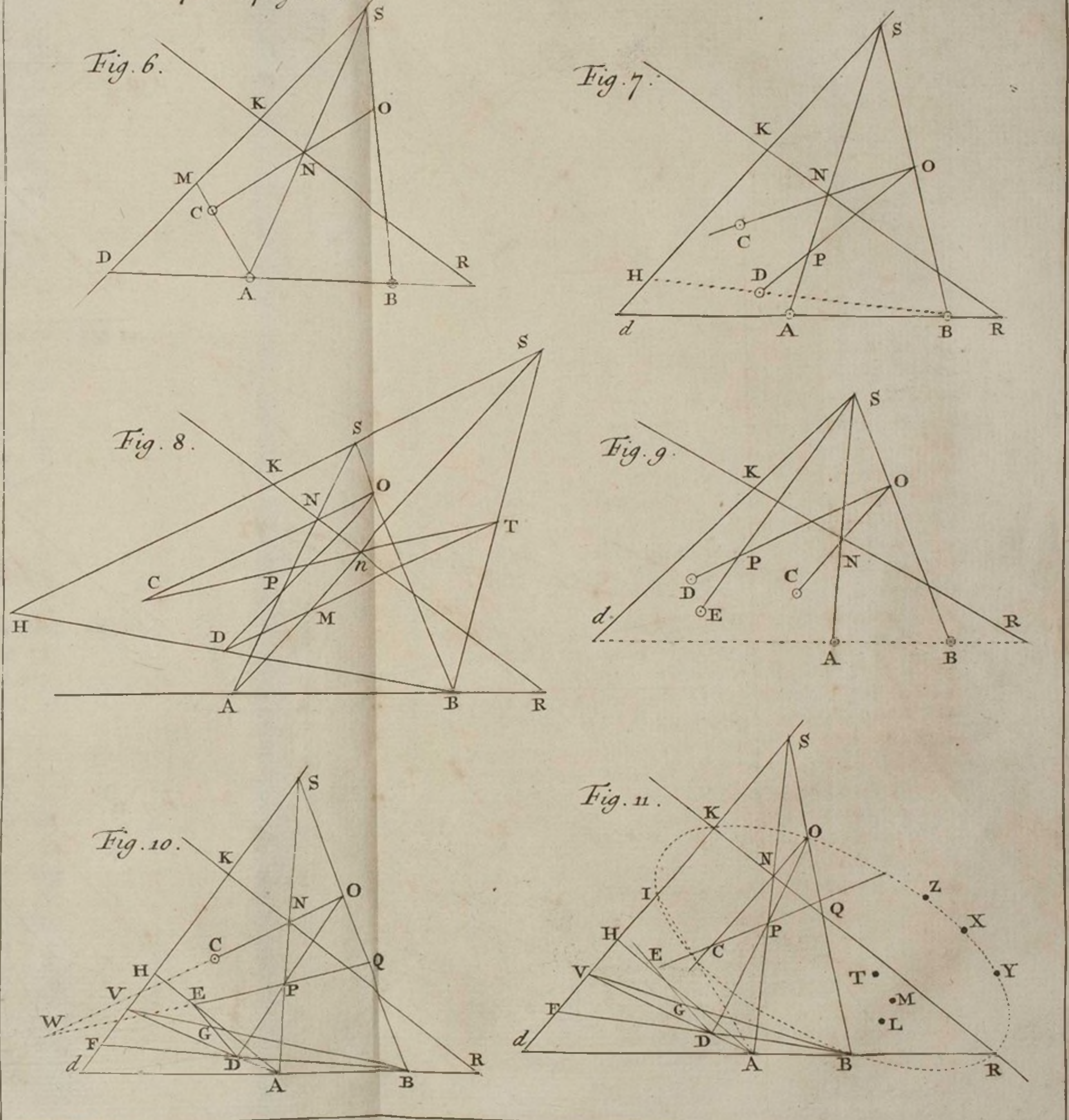
Fig. 11. Hence is derived the Method of drawing a Line of the 4th Order through nine given Points, of which one is triple. For let B, E, F, G, H, L, M, T, Q be given, and one of them B muſt be triple. Let the Points B F, F H, H E be joined, and the right Lines B F, F H, H E be produced, and through the Points E G, G B, let the right Lines E G D, B G V be drawn, of which let E G D cut the right Line B F in D, and let the other B G V cut the right Line F H in V. Then having joined V and D, and produced V D, till it meets the right Line H E in A, let the right Line d A B R be drawn thro' the Points A, B. Then from the Points B, E, let the right Lines B Q S, E P Q, be moved to the given Q, of which let the firſt B Q S meet with F H, produced to S; and A S being drawn through the Points A, S, and meeting the right Line E Q in P, let the right Line D P O be produced through P and D, and meet the right Line B Q S in O: and let the Point O be marked. And in like Manner from the ſame B, E, to another given T, let the right Lines B T s, E p T (ſupply the Figure) be turned, of which let B T s meet with F H in s, and the right Line A s cutting the right Line E p T in p being drawn, let the right Line D p Z be moved through p and D, and meet the right Line B T s in Z, and let Z be marked. And ſo on let right Lines be drawn from the ſame B, E, to the other given M L, and right Lines being drawn from A and D as before, let the Points found be marked X Y. Then thro' the four Points found, O, Z, X, Y, and the given one B, let a Conic Section be deſcribed, cutting the right Line F H in the Points I K, and the right Line d A B in B, R. Through the Points A, I, let the right Line A I be drawn, cutting the Conic Section in I and C; and let the Points K, R be joined, and let the right Line K R be produced. Now about the five Points A, B, C, D, E, as Poles, let as many right Lines A S, B S, C N, D O, E Q, revolve, of which let three A S, B S, C N, meet each other in N, S, O, and let the Interſections N and S of the right Lines A S, C N, and A S, B S be drawn through the right Lines K R, F H K; and in the mean time let the right Line D P O paſs through the Pole D, and the Interſection O of the right Lines B S, C N, and cut the right Line A S in P; and through P and the Pole E, let the right Line E P Q be produced, cutting the right Line B S, in Q, and this Interſection Q of the right Lines B S, E P will deſcribe a Line of the 4th Order paſſing through nine given Points, B, E, F, G, H, L, M, T, Q, one of which B will be triple.

Vid. Exerc. Geom. Prop. 3.

By a Method not much unlike this, a Line of the 4th Order may be deſcribed through eight given Points, three of which are double, as alſo a Line of the ſame Order through eleven given Points, two of which are double, and more of the ſame Sort.

2 But

Pl a. II. Vol. VIII. part 1st. page 60.



But as for the Number of Points which determine a Line of any Order, I find that, if n is the Number of the Dimensions of a Line, $n^2 + 1$ will be the Number of Points through which the Line may be described. For Instance, a Line of the second Order through five Points; of the third through 10; of the 4th through 17; of the 5th through 26. And hence is deduced, that if a Line of the Order n has a *punctum multiplex*, it may be described through $2n + 1$. For Example, a Line of the third Order, with a *punctum duplex*; that is, $n - 1 = 2$, thro' seven Points, and a Line of the 4th Order with a *punctum triplex* through nine, *&c.* And generally if p, q, r, &c. denote *puncta multiplicia*, of which the Number is m, a Curve may be described through $n^2 - p^2 - q^2 - r^2 + m + 1$ Points, in which m are *multiplicia*; for Instance, a Line of the 4th Order, which has three *puncta duplicia* may be described through eight Points; for $n = 4$, $p = q = r = 2$, $m = 3$, and $16 - 4 - 4 - 4 + 3 + 1 = 8$.

There is another Method also, not very different from the first, of describing Lines of the 4th Order, but a little more complicated. About seven Poles A, B, C, D, E, F, G, let there revolve as many right Lines A S, B S, C N, D S, E N, F O, G T, of which let one A N S, in revolving cut the right Lines d K, R K, given by Position, in the Points S, N; let the right Lines C N, E N be drawn through one of them N, and the right Lines B S, D S through the other S, and meet the right Lines C N, E N in the Points O, T, describing Conic Sections as above; and in the mean time let the right Lines F O, G T, drawn from the Poles F, G, pass through the same O, T, and meet in P; the intersection P will describe a Line of the 4th Order, with a double Point in both Poles F and G. Fig. 12.

But not to detain you any longer with these, I shall now give you the general Theorem. About the Points A, B, C, D, E, F, G, H, *&c.* as Poles, of which let the Number be n, let as many right Lines A S, B S, C N, D P, E Q, F W, X G, H Y, *&c.* revolve, of which let three A S, B S, C N intersect each other in the Points N, S, O, let two, S, N, be drawn through the right Lines d K, K R given by Position; and in the mean while through the Remainder O and the Pole D let the right Line D P pass, cutting the right Line A S in P, and the right Line E, Q, being drawn through P and the Pole E, cutting the right Line B S in Q, and from Q through the Pole F, let F Q be drawn, and cut the right Line A S in W, and W G being drawn through W and the Pole G, cutting the right Line B S in X, and then let the right Line H Y be produced through X and the Pole H, meeting the right Line S A in Y, and so on; the Intersection Y of the right Line Y H drawn from the last Pole H, with either of the right Lines A S, B S, will describe a Line of the Order $n - 1$; and the manifold Curve will have the Point $n - 2$ in the Pole A or B, as it has been described by the Intersection of the right Line A S or B S. The Points O, P, Q, W, X, Y, *&c.* will describe Lines of the 2d, 3d, 4th, Fig. 13.

4th, 5th, 6th, 7th, &c. Order, but if all the Poles A, B, C, D, E, F, G, H, &c. are placed in the same right Line, those Points, O, P, Q, W, X, Y, &c. will also describe as many right Lines.

Fig. 14. The *Newtonian* description is also greatly promoted by this Method. It is well known, that, if the given Angles O A N, O B N revolve about the given Points A B and the Intersection N of the Legs A N, B N, is drawn through the right Line N R, given by Positions, the Intersection O of the Legs A O, B O will describe a Conic Section. Now let another Point C be taken, about which let the right Line O C P, be moved, which shall always pass through the Intersection O of the Legs A O, B O, and meet the other Leg A N of the Angle A in P; the Intersection P will describe a Line of the 3d Order passing doubly through the Pole A. And in like manner, if by the Intersection B N of the Angle N a Curve is described, it will be of the same Order, and have a *punctum duplex* in the Pole B. And hence also it appears, how a Line of the third Order may be described, through seven given Points, one of which is double.

Fig. 15. Let the Angles O A N, O B N, be moved, as before, about the given Points A, B, and through the Intersection O of the Sides O A, O B, let the right Line O C P pass, being drawn from another given one C, meeting the Side A N of the Angle A in P; then through P and a 4th given one D, drawn the right Line D P Q meeting the Leg A O in Q; the Point Q will describe a Line of the 4th Order, with a *punctum triplex* in the Pole A.

And thus, by increasing the Number of the Poles A, B, C, D, &c. so that their Number at Length may be *n*, the Line described will be of the same Order *n*. But it should be observed, that for the Angle O B N we substitute the right Line, which revolves about the Pole B, the Description will be more easy.

Concerning the Description of Curve Lines, *by Mr* Colin M^c^Laurin, Math. Prof. Edinburgh, *F. R. S. Communicated* Dec. 21, 1732.

2. I am informed that some Papers * have been presented to the Royal Society of late, concerning the Description of Curves, in a manner that has a near Affinity to that which I communicated to them of old, and have carried farther since; and that it would not be unseasonable, nor unacceptable, if I should send an Account of what I have done further on that Subject since the Year 1719. The Author of those Papers taught Mathematicks here privately for some Years, and some time ago (*viz.* in 1727.) mentioned to me some Theorems he had on that Subject; which, at the same Time, I shewed him in my Papers. Some Time before that, he shewed me a Theorem which coincided with one of those in my Book, tho' he seemed not to have observed that Coincidence; and indeed Methods of that kind are often found coincident that do not appear such at first Sight. I am unwilling to be the Occasion of discouraging any thing that is truly ingenious,

* The Papers here hinted at are printed in a Treatise, intituled, *Exercitatio Geometrica de Descriptione Curvarum.* Authore *Gulielmo Braikenridge*, Lond. 1733. 4to.

 and

and renounce any Pretensions of appropriating Subjects to myself; but, on the contrary, wish Justice may be done to every Person, or to any Performance in Proportion to it's Merit; yet I find it is fit I should take Precautions lest any one should take it in his Head afterwards to say, I take Things from him which I may have had long before him; and therefore shall send you an Abstract of what I have done in Relation to this Matter, since the Year 1719.

I have so much on this Subject by me, that I am at a Loss what to send; but at present I shall only give you an Abstract of those Propositions, which I take to be more nearly related to those which this Author has offered to the Society from the Conversations I had with him. In 1721, I printed several Sheets of a Supplement to my Book on the Description of Curve Lines, which I have never yet published, having been engaged for the most part in Business of a different Nature, and in Pursuits on other Subjects since that time. I shall first give you an Abstract of that Supplement, as far as it was then printed, and shall subjoin to this an Account of some Theorems I added to it the following Year, *viz.* in 1722. I was led into those new Theorems by Mr *Robert Sympson*'s giving me at that Time a Hint of the ingenious Paper which has been since published in the Philosophical Transactions. I had tried in the Year 1719, what could be done by the Rotation of Angles on more than two Poles; and had observed, that if the Intersections of the Legs of the Angles were carried over right Lines, as in Sir *I. Newton*'s Description, the Dimensions of the Curve were not raised by this Increase of the Number of Poles, Angles, and right Lines; and therefore neglected this at that Time, as of no Use to me; confining myself to two Poles only, and varying the Motions of the Angles as you find them in my Book. I found this by inquiring in how many Points the Locus could cut a right Line drawn in it's Plane, and found, by a Method I often use in my Book, that it could meet it in 2 Points only.

Having found then, that 3 or more Poles, were of no more Service than 2, while the Intersections were carried over fixed right Lines, I thought it needless to prosecute that Matter then, since by increasing the Number of Poles, my Descriptions would become more complex without any Advantage. But in *June* or *July* 1722, upon the Hint I got from Mr *Sympson* of *Pappus*'s Porisms, I saw that what he has there ingeniously demonstrated, might be considered as a Case of the abovementioned Description of a Conic Section, by the Rotation of any Number of Angles about as many Poles; the Intersections of their Legs, in the mean time, being carried over fixed right Lines, excepting that of two of them which describes the Locus. For by substituting right Lines in place of the Angles, in certain Situations of the Poles and of the fixed right Lines, the Locus becomes a right Line; as for Example, in the Case of 3 Poles, when these 3 are in one right Line, in which Case the Locus is a right Line, which is a Case of the Porism.

This

This led me to consider this Subject anew; and first I demonstrated the Locus to be a Conic Section algebraically; and found Theorems for drawing Tangents to it, and determining it's Asymptotes. I also drew from it at that Time a Method of describing a Conic Section thro' 5 given Points *. This encouraged me to substitute Curves for the right Lines, to see if by this Method I could be enabled to carry on my Theorems about the Descriptions of Lines through given Points to the higher Orders of Lines. Some of the Theorems I found at that Time, I now send you. In *Nov.* 1722, looking into Sir *Isaac*'s *Principia*, I saw that the Description of the Conic Section by 3 right Lines, moving as above, about 3 Poles, could be immediately drawn from his 20th Lemma, which itself is a Case of this Description. This gradually led me to seek Geometrical Demonstrations for the whole, as far as it related to the Conic Sections. I send you some Leaves of this Paper dated at *Nancy*, *Nov.* 1722. Since that Time, I have not added much to this Subject, but what relates to the drawing Tangents, determining the Asymptotes, and the *Puncta Duplicia*, or *Multiplicia* of these Curves. I considered it the less, that I did not find it more advantageous in any Respects, than the Method I had considered in my Book, or more general.

In 1727 I added to a Chapter in my Algebra, which is very public in this Place, an Algebraic Demonstration of the Locus, when three Poles are employed; and the Method of describing a Conic Section through 5 given Points, subjoining at the same Time, that if more Poles are employed, and Angles or right Lines, the Locus was still a Conic Section; which I thought was a remarkable Property of the Conic Sections not observed before.

These Things I intended to put in order, and publish in the Supplement to my Book, a Part of which has been printed since the Year 1721. I have in my View also to give several other Things in that Supplement; two of which, I shall only just mention at present, because I believe they are foreign to the present Affair. I subjoin a Problem determining the Figure of a Fluid, whose Parts are supposed to be attracted to two or more Centers; and a Solution of a general Problem about the Collision of Bodies.

The Author of the Papers given in to the Royal Society, will not refuse that I shewed him the Theorems I now send you, in 1727. He owned it last Summer at least: I am to publish these very soon. Whether he has carried the Subject farther, I leave to the Judgment of the Gentlemen to whom they were referred. As to the Demonstrations, it would take some Time to put them in a proper Form to be published. I could send those that are algebraic easily; but do not care to send those that are geometrical, till I have Leisure.

* *The Paper on this Subject I have, is dated* July 31, 1722, *at Sea, being then in my Way to* London, *going for* Cambray.

In

In the firſt Part of the Supplement, there is a general Demonſtration given of the Theorem, that if two Lines of the Orders or Dimenſions, expreſſed by the Numbers *m* and *n*, be deſcribed in the ſame Plane, the greateſt Number of Points in which theſe Lines can interſect each other, will be *m n*, or the Product of the Numbers which expreſs the Dimenſions of the Lines, or the Orders to which they belong. *An Abſtract of what has been printed ſince the Year 1721, as a Supplement to a Treatiſe concerning the Deſcription of Curve Lines, publiſhed in 1719, and of what the Author propoſes to add to that Supplement.*

In the next Part, Theorems are given for drawing Tangents to all the Curves that were deſcribed in that Treatiſe by the Motions of Angles upon given Lines. Their Aſymptotes are alſo determined by more ſimple Conſtructions than thoſe which are ſubjoined to their Deſcriptions in that Treatiſe. Of theſe we ſhall give one Inſtance here.

Suppoſe the invariable Angles F C G, K S H, to revolve about the fixed Points or Poles, C and S. Suppoſe the Interſection of the two Sides C F, S K, to be carried over the Curve B Q M, whoſe Tangent at the Point Q is ſuppoſed to be the Right Line A E; and let it be required to draw a Tangent at P to the Curve Line deſcribed by P the Interſection of the other two Sides C G and S H. Fig. 16, 17.

Draw Q T conſtituting the Angle S Q T, equal to C Q A, on the oppoſite Side of S Q, that Q A is from C Q; and let Q T meet C S (produced if neceſſary) in T. Join P T, and conſtitute the Angle C P N equal to S P T, on the oppoſite Side of C P, that P T is from S P, and the Right Line P N, ſhall be a Tangent at P, to the Curve deſcribed by the Motion of P, which is always ſuppoſed to be the Interſection of C G and S H. *Conſtruction.*

The Aſymptotes of the Curve deſcribed by P, are determined thus. Find, as in the abovementioned Treatiſe, when theſe Sides become parallel, whoſe Interſection is ſuppoſed to trace the Curve; which always happens when the Angle C Q S becomes equal to the Supplement of the Sum of the invariable Angles F C G, K S H, to four Right ones, becauſe the Angle C P S then vaniſhes. Suppoſe that when this happens, the Interſection of the Sides C F, S K is found in Q. Fig. 18, 19.

Conſtitute the Angle S Q T equal to C Q A, as before, and let Q T meet C S in T. Take C N equal to S T, the oppoſite Way from C that S T lies from S. Through N draw D N parallel to C G or S H, which are now parallel to each other, and D N ſhall be an Aſymptote of the Curve deſcribed by the Motion of P.

If in place of a Curve Line B Q M, a fixed Right Line A E be ſubſtituted, then the Point P will deſcribe a Conick Section, whoſe Tangents and Aſymptotes are determined by theſe Conſtructions. In this Supplement, it is afterwards ſhewn how to draw the Tangents and Aſymptotes of all the Curves which are deſcribed in the above-mentioned Treatiſe by more Angles and Lines.

The ſame Method is afterwards applied for to draw Tangents to Lines deſcribed by other Motions than thoſe which are conſidered in that Treatiſe; of which the following is an Inſtance. Suppoſe that the

Lines C P and S P revolve about the Poles C and S, ſo that the Angle A C P bears always the ſame invariable Proportion to A S P, ſuppoſe
Fig. 20. that of m to n. In the Line C S, take the Point T, ſo that S T may be to C T in that ſame Proportion of m to n; and this Point T will be an invariable Point, ſince C S is to C T, as $m-n$ to n. Draw T P, and conſtitute the Angle S P N, equal to C P T, ſo that P N and P T, may lie contrary ways from S P and C P, and P N ſhall be a Tangent of the Curve deſcribed by the Motion of the Point P. Several other Theorems of this kind are ſubjoined here.

After theſe, Lines or Angles are ſuppoſed to revolve about three or more Poles, and the Dimenſions of the Curves with their Tangents and Aſymptotes are determined. Suppoſe in the firſt Place, that the three
Fig. 21. Poles are C, S, and D, and that Lines or Rulers C R, S Q, Q D R, revolve about theſe Poles. The Line which revolves about D, ſerves only to guide the Motion of the other two, ſo that it's Interſection with each of them being carried over a fixed Right Line, their Interſection with each other deſcribes the Locus, which is ſhewn to be a Conick Section. the Interſection of Q D R with S Q, is ſuppoſed to be carried over the fixed Right Line A F; the Interſection of the ſame Q D R, with C R, is ſuppoſed to be carried over the fixed Right Line A E; and in the mean time, the Interſection of the Right Lines S Q, C R, that revolve about the Poles S and C, deſcribes a Conick Section.

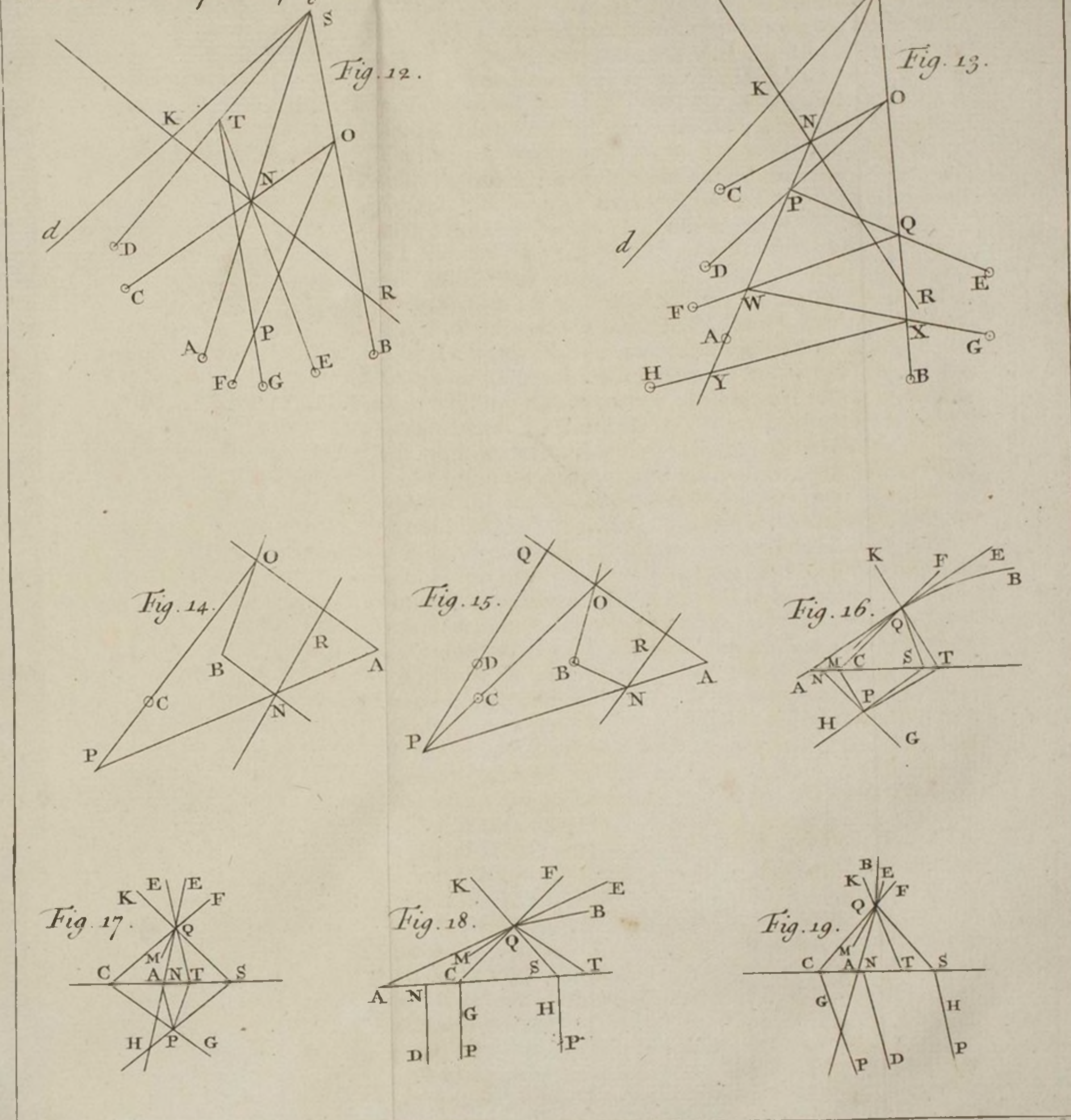
This Conick Section paſſes through the Poles C and S; and if you produce D C and D S, till they meet with A Q and A R in F and E, it will alſo paſs through F and E: It alſo paſſes always through A, the Interſection of the fixed Lines Q F and E R; from which this eaſy Method follows for drawing a Conick Section through 5 given Points. Suppoſe that theſe 5 given Points are A, F, C, S and E: Join 4 of them by the Lines A F, F C, A E, E S, and produce 2 of theſe F C, E S, till they meet, and by their Interſection give the Point D. Suppoſe infinite Right Lines revolve about this Point D, and the Points C and S, two of thoſe that were given, and let the Interſections of the Line revolving about D, with thoſe that revolve about C and S, be carried over the given Right Lines A E, A F; and the Interſection of thoſe that revolve about C and S with each other, will, in the mean Time, deſcribe a Conick Section, that ſhall paſs through the five given Points A, F, C, S and E.

Fig. 22. It is then ſhewn, that when C, S and D are taken in the ſame Right Line, the Point P deſcribes a Right Line; as alſo when C, S and A are in the ſame Right Line; which alſo follows from what is demonſtrated in that very ingenious Paper concerning *Pappus*'s Poriſms,
Vid. Vol. VI. p. 76. communicated by Mr *Sympſon*, Profeſſor of Mathematicks at *Glaſgow*.

In the next Place it is ſhewn, that if four Right Lines revolve about four Poles C, S, D, and E, and thoſe that revolve about D and E, ſerve only to guide thoſe that revolve about C and S; ſo that Q and R, the Interſections of that which revolves about D, with thoſe that revolve

about

Pla. III. Vol. VIII. part 1st page 66.



about E and S, be carried over the fixed Lines A B and A F; and M the Interſection of that which revolves about E with that which revolves about C, be carried over a third fixed Line B F, then the Interſection P of thoſe that revolve about C and S, will in the mean time, deſcribe a Conick Section, and not a Curve of a higher Order. The Conick Section degenerates into Right Lines, when C P and S P coincide at the ſame time with the Line C S, that joins the Poles C and S, as in the preceding Deſcription; which coincides again with what is demonſtrated in the abovementioned ingenious Paper.

After this it is ſhewn generally, that tho' the Poles and Lines revolving about them be increaſed to any Number, and the fixed Lines over which ſuch Interſections, as we deſcribed in the two laſt Caſes, are ſuppoſed to be carried, be equally increaſed, the Locus of the Point P will never be higher than a Conick Section: That is, let a Polygon of any number of Sides have all it's Angles, one only excepted, carried over fixed Right Lines, and let each of it's Sides produced, paſs through a given Point or Pole, and that one Angle which we excepted, will either deſcribe a ſtreight Line, or Conick Section.

Thus if a hexagonal Figure L Q R P M N, have all it's Angles excepting P carried reſpectively over the fixed Right Lines A *a*, B *b*, G *g*, H *h*, K *k*, the Point P in the mean time will deſcribe a Conick Section, or a Right Line. The Locus of P is a Right Line when C P and S P coincide together with the Line C S. All theſe things are demonſtrated geometrically. Fig. 23.

After this, Angles are ſubſtituted in place of Right Lines revolving about theſe Poles; and it is ſtill demonſtrated geometrically, that the Locus of P is a Conick Section or Right Line.

Suppoſe that there are 4 Poles C, S, D and E, about which the invariable Angles PCQ, P S R, R D M, M E Q revolve; and that Q, M and R, the Interſections of the Legs C Q and E Q, of E M and D M, and of D R and S R, are carried over the fixed Right Lines A *a*, B *b*, and G *g* reſpectively, then the Locus of P is a Conick Section, when C P and S P do not coincide at once with the Line C S, but is a Right Line when C P and S P coincide at the ſame time with C S, and never a Curve of a higher Order. Fig. 24.

Having demonſtrated this which ſeems a remarkable Property of the Conick Sections or Lines of the Second Order; I proceed to ſubſtitute Curve Lines in place of Right Lines in theſe Deſcriptions, (as I always do in the Treatiſe concerning the Deſcription of Lines) and to determine the Dimenſions of the Locus of P, and to ſhew how to draw Tangents to it to determine it's Aſymptotes, and other Properties of it. I had obſerved in 1719, that by increaſing the Number of Poles and Angles beyond two, the Dimenſions of the Locus of P, did not riſe above thoſe of the Lines of the Second Order, while the Interſections moved on Right Lines; and therefore I did not think it of uſe to me then to take more Poles than two, ſince by taking more, the Deſcriptions became more complex without any Advantage. When the Interfections are carried over Curve Lines, the Dimenfions of the Locus of P rife higher, but the Curves defcribed, have *Puncta Duplicia*, or *Multiplicia*, as well as when two Poles only are affumed; and therefore this Speculation is more curious than ufeful. However, I fhall fubjoin fome of the Theorems that I found on this Subject concerning the Dimenfions of the Locus of P, and the drawing Tangents to it.

Fig. 21. 1. If you fuppofe Q and R to be carried over Curve Lines of the Dimenfions m and n refpectively, then the Point P may defcribe a Locus of $2\,m\,n$ Dimenfions.

Fig. 23. 2. If you fuppofe L, Q, R, M, N, to be carried over Curve Lines of the Dimenfions m, n, r, s, t, refpectively, the Locus of P may arife to $2\,m\,n\,r\,s\,t$ Dimenfions, but no higher; and if in place of Lines revolving about the Poles, you ufe invariable Angles, the Dimenfions of the Locus of P will rife no higher.

Fig. 25. 3. I then affumed three Poles C, D and S, and fuppofed one of the Angles S N L, to have it's angular Point N carried over the Curve A N, while the Leg N Q paffes always through S, as in the Defcription in the Treatife of the General Defcription of Curve Lines, while the Angles Q D R, R C P, revolve about the Poles D and C: I fuppofe alfo the Interfections Q and R to be carried over the Curve Lines B Q, G R, and that the Dimenfions of the the Curve Lines A N, B Q, G R, are m, n, r, refpectively; and find that the Locus of P may be of $3\,m\,n\,r$ Dimenfions; but that the Point C is fuch, that the Curve paffes through it as often as there are Units in $2\,n\,m\,r$.

4. If any number of Poles are affumed, fo as to have Angles revolving about them, as about C and D in the laft Article, and the Interfections are carried over other Curves, the Dimenfions of the Locus of P will be equal to the triple Product of the Number of Dimenfions of all the Curves employed in the Defcription.

Fig. 26. 5. If the invariable Angles P N R, P M Q, move fo that while the Sides P N, P M, pafs always through the Poles C and S, the angular Points N and M defcribe the Curves A N and B M; and at the fame time, the invariable Angle R D Q, revolve about the third Pole D, fo that the Interfections R and Q defcribe the Curves E R and G Q; then the Dimenfions of the Locus of P, when higheft, fhall be equal to the quadruple Product of the Numbers that exprefs the Dimenfions of the given Curves A N, E R, G Q and B M, multiplied continually into each other. If more Poles are affumed, about which Angles be fuppofed to move, as R D Q moves about D in this Defcription, and the Interfections of the Sides be ftill carried over Curves, as in this Example; the Dimenfions, of the Locus of P, when higheft, fhall ftill be found equal to the quadruple Product of all the Numbers that exprefs the Dimenfions of the Curves employed in this Defcription.

Fig. 27. 6. Suppofe that the three invariable Angles P Q K, K L R, R N P, move over the Curves G Q, E L, A N, fo that the Sides P Q, K L, P N produced,

produced, pass always through the Poles C, D, S, and that the Intersections of their Sides K and R, at the same time move over the Curves F K and B R; and the Dimensions of the Locus of P when highest, shall be equal to the Product of the Numbers that express the Dimensions of the given Curves multiplyed by 6. If more Poles, with the necessary Angles and Curves, are assumed betwixt C and D, as here D is assumed betwixt C and S, and the Motions be in other respects like to what they are in this Example; then in order to find the Dimensions of the Locus of P when highest, raise the Number 2 to a Power whose Index is less than the Number of Poles by a Unit; add 2 to this Power, and multiply the Sum by the Product of the Numbers that express the Dimensions of the Curves employed in the Description; and this last Product shall shew the Dimensions of the Locus of P when highest.

I am able to continue these Theorems much farther: But it is not worth while, especially since I find that there is not any considerable Advantage obtained by increasing the number of Poles above the Method delivered in the abovementioned Treatise of the Description of Curve Lines. On the contrary, the Descriptions there given by means of two Poles, will produce a Locus of higher Dimensions by the same number of Curves and Angles, than these that require three or more Poles; and are therefore preferrable, unless perhaps in some particular Cases.

However, I have also found how to draw Tangents to the Curves that arise in all these Descriptions; of which I shall give one Instance where 3 Right Lines are supposed to revolve about 3 Poles, and 2 of their Intersections are supposed to be carried over given Curve Lines, and the third describes the Locus required.

Let the Right Lines C Q, S N, D N, revolve about the Poles C, S, D, Fig. 28.
where that which revolves about D, serves to guide the Motion of the other two; it's Intersection with C Q moving over the Curve G Q, while it's Intersection with S N moves over the Curve F N. Suppose that the Right Line B *b* touches the Curve G Q in Q, and that the Right Line A *a* touches the Curve F N in N. In order to draw a Tangent to the Locus of P; join D C, D S and C S, and constitute the Angle D Q R, equal to C Q B, so that Q R lie the contrary way from Q D that Q B lies from Q C, and let Q R meet D C in R. Constitute also the Angle D N T, equal to S N A with the like precaution, and let N T meet D S in T. Join R T, and produce it till it meet C S in H; then join P H, and make the Angle C P L equal to S P H, so that P L and P H, may lie contrary ways from C P and S P; and P L shall be a Tangent at P, to the Locus described by P, the Intersection of C Q and S N.

I have also applied this Doctrine to the Description of Lines through given Points. But I suppose I have said enough at present on this Subject; and shall conclude, after observing that in the abovementioned Treatise, I have given an easy Theorem for calculating the Resistance of the Medium when a given Curve is described with a given centripetal

Force in a refifting Medium, which I fhall here repeat, becaufe it has been mifreprefented in a foreign Journal.

Let V exprefs the centripetal Force with which the Body that is fuppofed to defcribe the Curve, is acted on in the Medium; let v exprefs the centripetal Force with which the fame Curve could be defcribed in a Void; fuppofe $z = \frac{V}{v}$, and the Refiftance fhall be proportional to the Fluxion of z multiplied by the Fluxion of the Curve, fuppofing the Area defcribed by a Ray, drawn from the Body to the Center of the Forces, to flow uniformly. Let this Theorem be compared with what the celebrated Mathematician mentioned by that Journalift has given on the fame Subject, and it will eafily appear what judgment is to be made of his Affertion; and fince feveral Perfons, and particularly the Gentleman mentioned above in this Paper, teftify that I communicated to them this Theorem before any Thing was publifhed on this Subject by the learned Mathematician he names, his Obfervation on this Occafion muft appear the more groundlefs.

From this Theorem, I draw this very general Corollary, that if the Curve is fuch as could be defcribed in a Void by a centripetal Force, varying according to any Power of the Diftance, then the Denfity of the Medium in any place, is reciprocally proportional to the Tangent of the Curve at that place, bounded at one Extremity by the Point of Contact, and, at the other, by it's Interfection with a Perpendicular raifed at the Center of the Forces to the Ray drawn from that Center to the Point of Contact. Let A L be the Curve defcribed by a Force directed to the point S; let L T touch the Curve at L, and raife S T perpendicular to S L, meeting L T in T, and the Denfity in L fhall be inverfely as L T, if the Refiftance be fuppofed to obferve the compound Proportion of the Denfity, and of the Square of the Velocity.

Fig. 29.

Befides what I have obferved here, I propofe to illuftrate and improve feveral other Parts of the Treatife concerning the Defcription of Curve Lines in this Supplement.

That Treatife requires thefe Additions and Illuftrations the more, that tho' the whole almoft was new, it was publifhed in a hurry, when I was very young, before I had time to confider fufficiently which were the beft ways of demonftrating the Theorems, or refolving the Problems, for which this Supplement I hope, will make fome Apology.

The Paper dated at Nancy, Nov. 27. 1722, *mentioned in the foregoing article Prop.* 1. Sect. 1.

3. About the Poles C, B, D, let the Right Lines Cd, Bm, Dr be moved, and let the Interfection of the Legs Bm, Dr be drawn thro' the given Right Line P G, the Interfection of the Legs Cd, Dr thro' the given Right Line P Q, and the Interfection of the Legs Cd, Bd will defcribe a Conick Section.

Fig. 30.

Draw rt parallel to the Right Line B D given by Pofition, and let it meet the Right Line Bd in t; join Pt and produce it till it meets the Right Line B D in F; and you will have the Point F. For as the

Proportion is given of ru to rt, which is the same as of DG to DB, because of the similar figures Dm BG and $rmtu$, and ru is to rt as QG to QF, the Proportion will also be given of QF to QG; and so because of the given one QG, QF will be given, and therefore the Point F and the Right Line PF. Since therefore Bt and Cr cut off the parts Pt, Pr, from the Right Lines given by Position PF, PQ, their Intersection d will always be in a given *Ratio* in a Conick Section, by *Lem.* 20. *Lib.* I. *Newt. Princip.*

If the Point D be taken any where in the Right Line BF, and if DG is always to QG as BD to QF, the Conick Section will be the same that d shall describe.

The Conick Section passes thro' C, P, B, and a by compleating the Parallellogram PSau. It also passes thro' L where the Right Line BG being produced meets Pu, as also thro' K, where the Right Line CD cuts the given one PG. Whence the Pentagon PKCLB is inscribed on the Section. And if 5 points C, K, P, B, L are given, thro' which the Conick Section is to be drawn, or if the Conick Section is to be circumscribed about the given Pentagon CLBPK, let any 2 sides CK, LB be produced to their Intersection D, and then let the rest PL, PK be joined, and let the Intersections of the Right Lines Cd, Dr, and Bd, DR be always drawn thro' those Right Lines PL, PK, and the Intersection d will describe the Section.

About the given Points F, C, G, S, as Poles, let the Right Lines FQ, CN, GQ, SL be moved, and let the Intersections of the Right Lines FQ and CN, FQ and GQ, GQ and SL, namely the Points M, Q, L, always touch the Right Lines given by Position AE, BE, HL, and the Intersection of the Right Lines CN, SL, will describe a Conick Section. *Prop.* 2. Fig. 31.

Let the Right Lines AM, HR meet BQ in E and H. Let CF and GS be joined meeting each other in D, let DQ be joined meeting the Right Lines CM, SL in N and R; and if EN and HR are joined, EN and HR will be Right Lines given by Position by *Lemma* I. For as the Points F, C, D are in the same Right Line, and the Intersections of the Right Lines FM, CM, and FQ, DQ run over the given Right Lines, the Intersection of the Legs CM, DQ will also touch the given one. And for the like Reason as S, D, G are in the same Right Line, the Intersection of the Right Lines DQ, SL will also touch the given one.

Therefore omitting the Poles F and G, the Curve is to be found which the Intersection of the Right Lines CN, SL, viz. P will describe, whilst, as the Right Lines CN, DN, SR revolve about the Poles C, D, S, the Intersection of the Right Lines CN, DN touches the given EN, and the Intersection of the Right Lines SR, DN touches the given one HR, and that this is a Conick Section is manifest from the foregoing Proposition.

VII. Ha-

Concerning two Species of Lines *of the* Third Order, (*not mentioned by Sir* I. Newton, *nor Mr* Sterling) *by Mr* Edmund Stone, F.R.S. N°. 456. p. 318. *Jan. &c.* 1740. dated *July* 31. 1736.

VII. Having for ſome time paſt been reading and conſidering the little Treatiſe of Sir *I. Newton* intituled *Enumeratio Linearum tertii Ordinis*, as alſo the ingenious Piece of Mr *Sterling* called *Illuſtratio Tractatus* Domini Newtoni *Linearum tertii Ordinis*; I have obſerved, that they have neither of them taken Notice of the two following Species of Lines of the Third Order; and venture to affirm, that the 72 Species mentioned by Sir *Iſaac*, together with the 4 more of Mr *Sterling*, and theſe Two, making in all 78, is the exact Number of the different Species of the Lines of the Third Order, according to what Sir *Iſaac* has thought fit to conſtitute a different Species.

The two Species I mean, are to be reckoned amongſt the Hyperbolo-parabolical Curves, having one Diameter, and one Aſymptote, at N°. 8. of *Newton's* Treatiſe, or Page 104. of Mr *Sterling's*; whoſe Equation is $xyy = \pm bx^2 \pm cx \pm d$; which will give, not 4, as in theſe Authors, but 6 Species of theſe Curves: For,

I. If the Equation $bx^2 \pm cx + d = 0$, has two impoſſible Roots, the Equation $xyy = bx^2 \pm cx + d$, will (as they ſay) give two Hyperbolo-parabolical Figures equally diſtant on each ſide the Diameter A B. See the 57th Figure in *Newton*'s Treatiſe, and this is his 53d Species, and *Sterling*'s 57th.

II. If the Equation $bx^2 - cx + d = 0$, has two equal Roots both with the Sign $+$; the Equation $xyy = bx^2 - cx + d$, will (as they ſay) give two Hyperbolo-parabolical Curves croſſing each other at the Point τ in the Diameter. See Fig. the 58th in *Newton*; and this is his 54th Species, and *Sterling*'s 58th.

Fig. 32. III. But if the Equation $bx^2 + cx + d = 0$, has two poſſible unequal negative Roots A ρ and A τ, the Curve given by the Equation $xyy = + bx^2 + cx + d$, will conſiſt of two Hyperbolo-parabolical Parts, as alſo of an Oval on the contrary Side the Aſymptote or principal Abſciſs. And this is one of the Species omitted by Sir *Iſaac* and Mr *Sterling*, which is really the 59th Species.

Fig. 33. IV. Alſo if the Equation $bx^2 + cx + d = 0$, has two equal negative Roots A ρ and A τ; the Curve given by the Equation $xyy = + bx \pm cx + d$, will conſiſt of two Hyperbolo-parabolical Parts, and alſo of a Conjugate Point on the contrary Side the Aſymptote or principal Ordinate: And this is the other Species of theſe Curves omitted by Sir *Iſaac* and Mr *Sterling*, which is really the 60th Species.

V. If the Roots of the Equation $bx^2 - cx + d = 0$ are real, and unequal, having both the Sign $+$; the Curve given by the Equation $xyy = bx^2 - cx + d$, will (as they ſay) conſiſt of a conchoidal Hyperbola and a Parabola, on the ſame ſide the Aſymptote or principal Ordinate. See Fig. the 59th in *Newton*; and this is really the 61ſt Species.

VI. If the Roots of the Equation $bx^2 + cx - d = 0$, have contrary Signs, the Equation $xyy = bx^2 \pm cx - d$, will (as they ſay) give a conchoidal

conchoidal *Hyperbola* with a *Parabola* on the contrary Side the Afymptote or principal Ordinate. See Fig. the 60th in *Newton*; and this is really the 62d Species.

VIII. Many Attempts have been made at different times, but, if I miftake not, never any yet with tolerable Succefs, towards the Solution of the Problem propofed by *Kepler*: To divide the Area of a Semicircle into given Parts, by a Line from a given Point of the Diameter, in order to find an univerfal Rule for the Motion of a Body in an Elliptic Orbit. For among the feveral Methods offered, fome are only true in Speculation, but are really of no Service. Others are not different from his own, which he judged improper: And as to the reft, they are all fome way or other fo limited and confined to particular Conditions and Circumftances, as ftill to leave the Problem in general untouched. To be more particular; it is evident, that all Conftructions by Mechanical Curves are feeming Solutions only, but in reality unapplicable; that the Roots of infinite Series's are, upon account of their known Limitations in all refpects, fo far from affording an Appearance of being fufficient Rules, that they cannot well be fuppofed as offered for any thing more than Exercifes in a Method of Calculation. And then, as to the univerfal Method, which proceeds by a continued Correction of the Errors of a falfe Pofition, it is, when duly confidered, no Method of Solution at all in itfelf; becaufe unlefs there be fome antecedent Rule or Hypothefis to begin the Operation, (as fuppofe that of an uniform Motion about the upper Focus, for the Orbit of a Planet; or that of a Motion in a Parabola for the perihelian Part of the Orbit of a Comet; or fome other fuch) it would be impoffible to proceed one ftep in it. But as no general Rule has ever yet been laid down, to affift this Method, fo as to make it always operate, it is the fame in Effect as if there were no Method at all. And accordingly in Experience it is found, that there is no Rule now fubfifting but what is abfolutely ufelefs in the Elliptic Orbits of Comets; for in fuch Cafes there is no other way to proceed but that which was ufed by *Kepler*: To compute a Table for fome part of the Orbit, and therein examine if the Time to which the Place is required, will fall out any-where in that Part. So that, upon the whole, I think, it appears evident, that this Problem (contrary to the received Opinion) has never yet been advanced one Step towards it's true Solution: A Confideration which will furnifh a fufficient Plea for meddling with a Subject fo frequently handled; efpecially if what is offered fhall at the fame time appear (as I truft it will) to contribute towards fupplying the main Defect.

The Solution of Kepler's *Problem, by* J. Machin, *Aftr. Prof.* Grefh. *and Secr.* R. S. N°. 447. p. 205. Jan. &c. 1738.

The Tangent of an Arch being given, to find the Tangent of it's Multiple. Lemma I.

Let r be the Radius of the Circle, t the Tangent of a given Arch A, and n a given Number. And let T be the Tangent of the Multiple Arch $n \times A$ to be found.

 Then

Then if $\varrho\varrho$ be put for $-rr$, and $\tau\tau$ for $-tt$;

The Tangent T will be $\dfrac{\overline{r+\tau}|^n - \overline{r-\tau}|^n}{\overline{r+\tau}|^n + \overline{r-\tau}|^n}\varrho$: Which Binomials being raised according to Sir *I. Newton*'s Rule, the fictitious Quantities τ and ϱ will disappear, and the Tangent T will become equal to

$$\frac{nt - \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.\frac{t^3}{r^2} + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}\frac{n-3}{4}.\frac{n-4}{5}.\frac{t^5}{r^4} - \&c.}{1 - \frac{n}{1}.\frac{n-1}{2}.\frac{tt}{rr} + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}.\frac{t^4}{r^4} - \&c.}$$

This Theorem (which I formerly found for the Quadrature of the Circle, at a time when it was not known here to have been invented before) has now been common for many Years; for which Reason I shall premise it, at present, without any Proof; only for the sake of some Uses that have not yet been made of it.

Corol. 1. From this Theorem for the Tangent, the Sine (suppose) Y, and Cosine Z of the Multiple Arch $n \times A$, may be readily found.

For if y be the Sine, and z the Cosine of the given Arch A, then putting vv for $-yy$, and substituting $\frac{ry}{z}$ for t, and $\frac{rv}{z}$ for τ, and $\dfrac{rT}{\sqrt{rr+TT}}$ for Y: The Sine Y will be $\dfrac{\overline{z+v}|^n - \overline{z-v}|^n}{2r^n}\varrho$. The Cosine Z will be $\dfrac{\overline{z+v}|^n + \overline{z-v}|^n}{2r^{n-1}}$.

Each of these may be expressed differently in a Series, either by the Sine and Cosine conjointly, or by either of them separately.

Thus Y *the Sine* of the multiple Arch $n \times A$, may be in either of these two Forms, *viz.*

$$= \frac{z^{n-1}}{r^{n-1}} y \text{ in } n - \frac{n-1}{2}.\frac{n-2}{3}\overset{|}{A}.\frac{y^2}{z^2} + \frac{n-3}{4}.\frac{n-4}{5}\overset{|}{B}.\frac{y^4}{z^4} - \&c.$$

$$\text{or} = ny - \frac{nn-1}{2.3rr}\overset{|}{A}y^3 - \frac{nn-9}{4.5rr}\overset{|}{B}y^5 - \frac{nn-25}{6.7rr}\overset{|}{C}y^7 - \&c.$$

Wherein the Letters $\overset{|}{A}$, $\overset{|}{B}$, $\overset{|}{C}$, *&c.* stand, as usual, for the Coefficients of the preceding Terms.

The first of these Theorems terminates when n is any integer Number, the other (which is Sir *I. Newton*'s Rule, and is derived from the former by substituting $\sqrt{rr-yy}$ for z) terminates when n is any odd Number.

The *Cosine* Z may, in like manner, be in either of these two Forms *viz.*

=

$= \frac{z^n}{r^{n-1}}$ in $1 - \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{y^2}{z^2} + \frac{n}{1} \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} \cdot \frac{y^4}{z^4} -$ &c.

or $= r - \frac{nn}{2rr} A y^3 - \frac{nn-4}{3 \cdot 4rr} B y^4 - \frac{nn-16}{5 \cdot 6rr} C y^6 -$ &c.

The latter of which terminates when the Number n is even, and the other as before, when it is any Integer.

Hence the Sine, Cosine, and Tangent of any Submultiple Part of an Arch (suppose) $\frac{1}{n} A$, may be determined thus: *Corol.* 2.

The Tangent of $\frac{1}{n} A$ will be $\dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}} \varrho$

The Sine of $\frac{1}{n} A$ will be $\dfrac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2 r^{\frac{1}{n}}} \varrho$

For these Equations will arise from the Transposition and Reduction of the former for the Tangent and Sine of the Multiple Arch, upon the Substitution of t, y, z and A; for T, Y, Z and $n \times A$.

Hence regular Polygons of any given Number of Sides may be inscribed within, or circumscribed without, a given Arch of a Circle. *Corol.* 3. For if the Number n express the double of the Number of Sides to be inscribed within, or circumscribed about, the given Arch A; then one of the Sides inscribed will be the double of the Sine, and one of the Sides circumscribed the double of the Tangent of the Submultiple Part of the Arch, *viz.* $\frac{1}{n} A$.

To find the Length of the Arch of a Circle within certain Limits, by means of the Tangent and Sine of the Arch. **Lemma II.**

Let t be the Tangent, y the Sine and z the Cosine of the Arch A, whose Length is to be determined, and let ϱ, τ, v be expounded as before; then, if any Number n be taken, the Arch of the Circle will be always less than $\dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} \times n \varrho$, and bigger than

$\dfrac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2 r^{\frac{1}{n}}} \times n \varrho$.

For if, by the preceding *Corollaries*, a regular rectilinear Polygon be inscribed within, and another without, the Arch A, each having half so many Sides as is expressed by the Number n; then will the former of theſe Quantities be the Length of the Bow of the circumſcribed Polygon, (or the Sum of all it's Sides) which is always bigger and the latter will be the Length of the Bow of the inſcribed Polygon, which is always leſs, than the Arch of the Circle; how great ſoever the Number n be taken.

Corol. 1. Hence the Series's for the Rectification of the Arch of a Circle may be derived.

For by converting the Binomials into the Form of a Series, that the fictitious Quantities, ϱ, τ, v may be deſtroyed; it will appear, that no Number n can be taken ſo large as to make the inſcribed Polygon ſo big, or the circumſcribed ſo little as the Series.

$\frac{ry}{z} - \frac{ry^3}{3z^3} + \frac{ry^5}{5z^5} - \frac{ry^7}{7z^7} +$ &c. in one Caſe, or it's Equal $t - \frac{t^3}{3r^2}$ $+ \frac{t^5}{5r^4} - \frac{t^7}{7r^6} +$ &c. in the other Caſe.

Wherefore ſince the Quantity denoted by the Sum of the Terms in either of theſe Series's is always bigger than any inſcribed Polygon, and always leſs than any circumſcribed, it muſt therefore be equal to the Arch of the Circle.

Corol. 2. If, in the firſt of the above Series's, the Root $\sqrt{rr - yy}$, be extracted and ſubſtituted for z, there will ariſe the other Series of Sir *I. Newton*, for giving the Arch from the Sine; namely, $y + \frac{y^3}{6r^2} + \frac{3y^5}{40r^4} + \frac{5y^7}{112r^6}$ $+$ &c. or otherwiſe, $= y + \frac{1}{1.2.3} \times \frac{y^3}{r^2} + \frac{3 \cdot 3}{1.2.3.4.5.} \times \frac{y^5}{r^4} +$ $\frac{3 \cdot 3 \cdot 5 \cdot 5}{1.2.3.4.5.6.7.} \times \frac{y^7}{r^6} +$ &c.

Scholium. In like manner, as the Arches of the Polygons ſerve to determine the Arch of the Circle, ſo by comparing the Areas of the circumſcribed and inſcribed Polygons, $\frac{1}{2}nrT$ and $\frac{1}{2}nYZ$, the Area of the Sector of a Circle may be found. For if T, Y and Z are the Tangent, Sine, and Coſine of the Arch A; then by the ſecond *Lemma* the Area of the circumſcribed Polygon will be found to be $\frac{1}{2}nr\varrho \times \frac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}}$ $= \frac{1}{2}nrT$. and the Area of the inſcribed will appear to be $\frac{1}{2}n\varrho \times$ $\frac{\overline{z+v}|^{\frac{2}{n}} - \overline{z-v}|^{\frac{2}{n}}}{4r^{\frac{2}{n}-1}} = \frac{1}{2}nYZ.$

But

But upon the Expanſion of theſe Binomials it will appear, that no Number n can be taken ſo large as to make the one ſo big, or the other ſo little, as the Area denoted by the Series. $\frac{1}{2}r$ in $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} +$ &c.

So that this Area being larger than any inſcribed, and ſmaller than any circumſcribed, Polygon, muſt be equal to the Area of the Sector.

It may further be obſerved, that as the Arch or Area is found from the Sine, Coſine, or Tangent of the Arch, by means of the limiting Polygons, ſo may the Sine, Coſine, or Tangent be found from the Length of the Arch by the ſame Method.

Thus, if A be the Arch whoſe Tangent T, Sine Y, and Coſine Z, are to be determined, then will the

$$\text{Tangent } T \text{ be} = \frac{A - \frac{1}{1.2.3} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5} \times \frac{A^5}{r^4} - \text{\&c.}}{1 - \frac{1}{1.2} \times \frac{A^2}{rr} + \frac{1}{1.2.3.4} \times \frac{A^4}{r4} - \text{\&c.}}$$

$$\text{Sine } Y = A - \frac{1}{1.2.3} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5} \times \frac{A^5}{r^4} - \text{\&c.}$$

$$\text{Coſine } Z = r - \frac{1}{1.2} \times \frac{A^2}{r} + \frac{1}{1.2.3.4} \times \frac{A^4}{r^3} \text{ \&c.}$$

For it may be made to appear, from the firſt *Lemma*, and it's *Corollaries*, that if in any of theſe Theorems, as ſuppoſe in the Firſt, the Quantity A ſtand for the Bow of the circumſcribed Polygon, then will the Quantity T exhibited by the Theorem, be always bigger; but if for the Bow of the inſcribed, always leſs than the Tangent of the Arch, how great ſoever the Number n be taken; and conſequently, if A ſtand for the Length of the Arch itſelf, the Quantity T muſt be equal to the Tangent; and the like may be ſhewn for the Sine, and *mutatis mutandis*, for the Coſine.

Theſe Principles, from whence I have here derived the Quadrature of the Circle, which is wanted in the Solution of the Problem in hand, happen to be upon another Account abſolutely requiſite for the Reduction of it to a manageable Equation. But I have inlarged, more than was neceſſary to the Problem itſelf, on the Uſes of this ſort of Quadrature by the limiting Polygons, becauſe it is one of that kind which requires no other Knowledge but what depends on the common Properties of Number and Magnitude; and ſo may ſerve as an Inſtance to ſhew that no other is requiſite for the Eſtabliſhment of Principles for Arithmetick and Geometry. A Truth, which though certain in itſelf, may

may perhaps ſeem doubtful from the Nature and Tendency of the preſent Inquiries in Mathematicks. For among the Moderns ſome have thought it neceſſary, for the Inveſtigation of the Relations of Quantities, to have Recourſe to very hard Hypotheſes; ſuch as that of Number infinite and indeterminate; and that of Magnitudes in *Statu fieri*, exiſting in a potential Manner, which are actually of no Bigneſs. And others, whoſe Names are truly to be reverenced on Account of their great and ſingular Inventions, have thought it requiſite to have Recourſe even to Principles foreign to Mathematicks, and have introduced the Conſideration of efficient Cauſes and Phyſical Powers for the Production of Mathematical Quantities; and have ſpoken of them, and uſed them, as if they were a Species of Quantities by themſelves.

N. B. In the following Propoſition I have, for the Sake of Brevity, made uſe of a peculiar Notation for compoſite Numbers (or ſuch Quantities as are analogous to them) whoſe Factors are in Arithmetical Progreſſion.

The Quantity expreſſed by this Notation has a double Index: that as the Head of the Root at the Right-hand, but ſeparated by a Hook to diſtinguiſh it from the common Index, denotes the Number of Factors; and that above, within the Hook on the Left-hand, denotes the common Difference of the Factors proceeding in a decreaſing or increaſing Arithmetical Progreſſion.

Thus the Quantity $\overline{n+a}\overset{\alpha}{\smile}(^{m}$ denotes by it's Index m on the Right-hand, that it is a compoſite Quantity, conſiſting of ſo many Factors as there are Units in the Number m; and the Index α above, on the Left, denotes the common Difference of the Factors, decreaſing in an Arithmetical Progreſſion, if it be poſitive; or increaſing, if it be negative; and ſo ſignifies, in the common Notation, the compoſite Number or Quantity, $\overline{n+a}.\ \overline{n+a-\alpha}.\ \overline{n+a-2\alpha}.\ \overline{n+a-3\alpha}.$ and ſo on.

For Example: $\overline{n+5}\overset{2}{\smile}(^{6}$ is $=\overline{n+5}.\ \overline{n+3}.\ \overline{n+1}.\ \overline{n-1}.\ \overline{n-3}.\ \overline{n-5}$, conſiſting of ſix Factors whoſe common Difference is 2. After the ſame Manner $\overline{n+4}\overset{2}{\smile}(^{5}$ is $=\overline{n+4}.\ \overline{n+2}.\ n.\ \overline{n-2}.\ \overline{n-4}$, conſiſting of five Factors. According to which Method it will eaſily appear, that if a be any Integer, then $\overline{n+2a+1}\overset{2}{\smile}(^{2a+2}$ will be =

$=\overline{nn-1}.\ \overline{nn-9}.\ \overline{nn-25}$, continued to ſuch a Number of double Factors as are expreſſed by $a+1$, or half the Index, which in this Caſe is an even Number. So $\overline{n+2a}\overset{2}{\smile}^{(2a+1}$ will be equal to $n.\ \overline{nn-4}.\ \overline{nn-16}.\ \overline{nn-36}$, and ſo on, where there are to be ſo many double Factors as with one ſingle one (n) will make up the Index $2a+1$, which is an odd Number.

If the common Difference α be an Unit, it is omitted:

Thus, $\overset{\smile}{n}^{(6}$ is $=n.\ \overline{n-1}.\ \overline{n-2}.\ \overline{n-3}.\ \overline{n-4}.\ \overline{n-5}$, containing 6 Factors. So $\overset{\smile}{6}^{(6}$ is $=6.\ 5.\ 4.\ 3.\ 2.\ 1$, and the like for others.

If the common Difference α be nothing, then the Hook is omitted, and it becomes the ſame with the Geometrical Power:

So $\overline{n+a}\overset{0}{\smile}^{(m}$ is $=\overline{n+}|^{m}$ according to the common Notation.

An Arch leſs than a Semicircle being given, with a Point in the Diameter paſſing through one of it's Extremities; to find, by means of the Sine of a given Part of the Arch leſs than one half, the Area of the Sector ſubtended by the given Arch, and comprehended in the Angle made at the given Point. Prop. I.

Let P N A be a Semicircle deſcribed on the Centre C, and Diameter A P, and let P N be the given Arch leſs than a Semicircle, and S the given Point in the Diameter A P paſſing through one of the Extremities of the Arch N P in P. Then taking any Number n bigger than 2, let P K be an Arch in Proportion to the given Arch P N, as Unity to the Number n; and let it be required to find by means of the Sine of the Arch P K, the Area of the Sector N S P ſubtended by the given Arch N P, and comprehended in the Angle N S P made at the given Point S.

From N and K let fall on the Diameter A P the Perpendiculars N M and K L, and join C N and C K.

Then let t ſtand for C P the Semidiameter of the Circle; f for C S, the Diſtance of the given Point S from the Center; p for S P the Diſtance of it from the Extremity of the Arch through which the Diameter A P paſſes; and y for K L the Sine of the Arch K P in the given Circle.

Theſe Subſtitutions being preſuppoſed, the Problem is to be divided into two Caſes; one when S P is leſs, and the other when it is greater than the Semidiameter C P.

If S P be leſs than C P, then take an Area H equal to the Sum of the Rectangles expreſſed by the ſeveral Terms of the following Series continued *ad libitum*: Caſe I.

$$\frac{py}{1}$$

$$\frac{py}{1} + \frac{t + \overline{n^2+1}|^2 \times f}{3|^3} \times \frac{)^3}{t^2} + \frac{9t - \overline{n^2+3}|^4 \times f}{5|^5} \times \frac{)^5}{t^4}$$

$+ \frac{9 \times 25\, t + \overline{n^2+5}|^6 \times f}{7|^7} \times \frac{)^7}{t^6} +$ &c. And the Area $\frac{1}{2}$ $n \times H$ will determine the Area of the Sector N S P *ad libitum.*

For the Sector P S N, being the Excess of the Sector N C P above the Triangle N C S, will be the Difference of two Rectangles: $\frac{1}{2}$ C P × P N — $\frac{1}{2}$ C S × N M; but P N is the Multiple of the Arch P K, namely $n \times$ P K; and N M is the Sine of that multiple Arch: Wherefore if for C P be put t, for C S, f, according to the Supposition; and if for P K be substituted: $\frac{y}{1} + \frac{1}{3|^3} \times \frac{)^3}{t^2} + \frac{9}{5|^5} \times \frac{)^5}{t^4} + \frac{9 \times 25}{7|^7} \times \frac{)^7}{t^6} +$ &c. by *Cor.* 2. *Lem.* 2; and for N M:

$$\frac{ny}{1} - \frac{n.\,\overline{n^2+1}|^2}{3|^3} \times \frac{)^3}{t^2} + \frac{n.\,\overline{n^2+3}|^4}{5|^5} \times \frac{)^5}{t^4} - \frac{n.\,\overline{n^2+5}|^6}{7|^7} \times \frac{)^7}{t^6} + \text{\&c.}$$

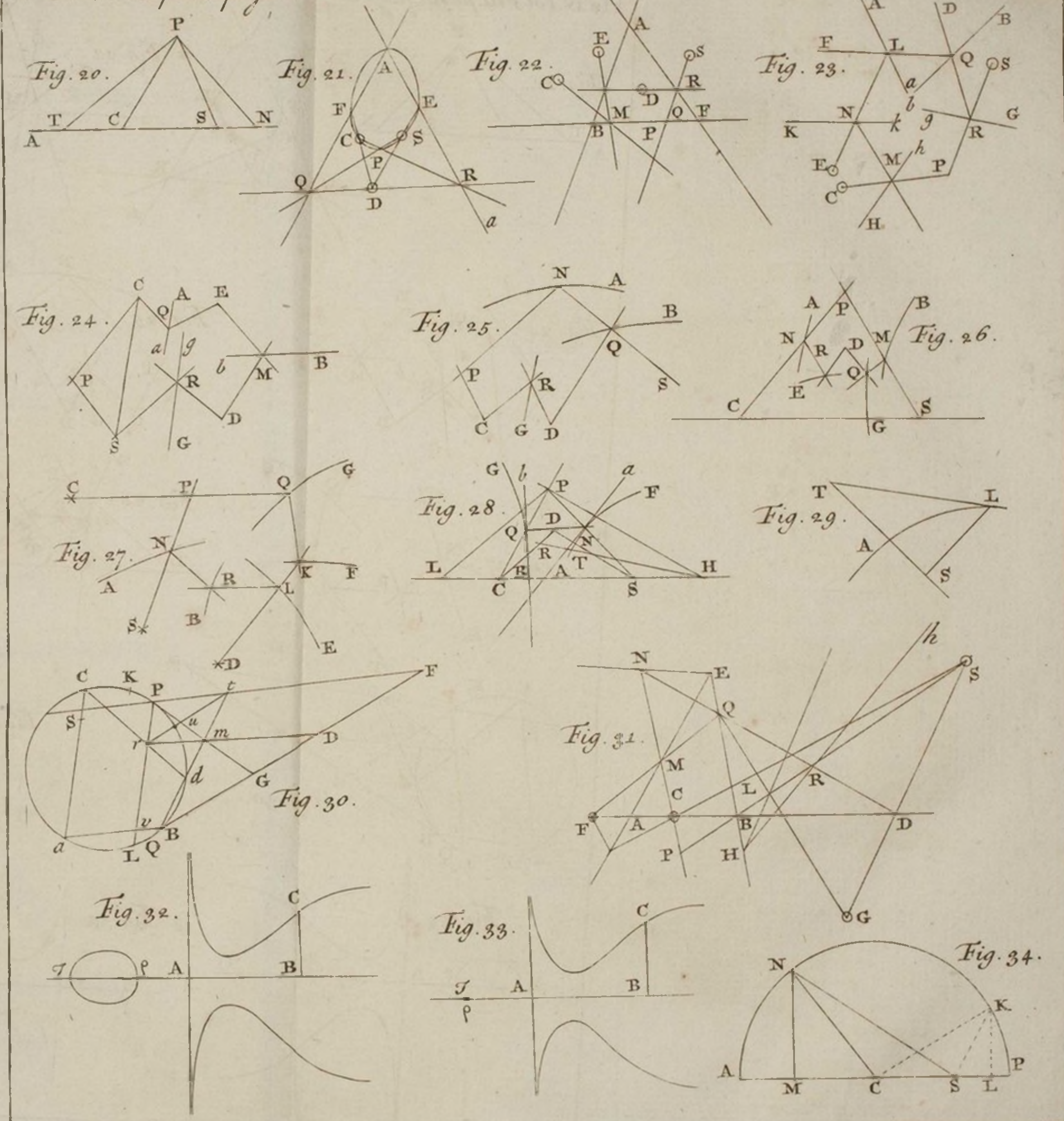
according to *Cor.* 1. *Lem.* 1. the Area of the Sector will appear in a Series, as is above determined.

But since the Number n is greater than 2, and the given Arch P N is less than a Semicircle, and consequently K L or y, the Sine of the Submultiple Arch P K, is less than the Semidiameter C P or t; it may thence be easily proved, that the Series will approximate to the just Quantity of the Area, *ad libitum.*

Corol. 1. Hence, if the Number n be taken equal to $\sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$ the Sector N S P will be $= \frac{1}{2}npy + \frac{n^3 t - n.\,\overline{nn-1}.\,p}{12tt}y^3$ $+ **** + \frac{n^3}{1120t^5})^7 +$ &c.

For the Numerator of the Coefficient of the third Term in the Series, that determines the Area H, namely, $9t - \overline{n^2+3}|^4 \times f$ is equal to $9t - \overline{nn-1}.\,\overline{nn-9}.\,f$, which according to the above Deter-

Pla. IV. Vol. VIII. part 1st page 80.



Determination of the Number n, will become nothing; wherefore, if for $t-p$ be put f in the ſecond Term, and the Value of n be ſubſtituted for n in the Third and Fourth, the Series for the Area will appear upon Reduction to be as is here laid down.

Hence the Area of the Sector N S P may be always defined nearly by the Terms of a Cubic Equation. *Corol.* 2.

For the Number n, as conſtructed in the former *Corollary*, is always greater than the ſquare Root of 10, and conſequently $\frac{y}{t}$ is always leſs than the Sine of one third Part of the given Arch; ſo that the fourth Term $\frac{n^3}{1120t^7}y^7$, with the Sum of all the following Terms of the Series, can never be more than a ſmall Part of the whole Sector.

If R ſtand for 57,2957795, *&c.* Degrees, (or the Number of Degrees contained in an Angle ſubtended by an Arch of the ſame Length with the Radius of the Circle) and M be the Number of Degrees in an Angle which is to 4 right Angles, as the Area N S P to the Area of the whole Circle; then will M be $= \frac{np}{t} \times \frac{Ry}{t}$ *Corol.* 3.

$$+ \frac{n^3 t - n . \overline{nn-1} . p}{6t} \times \frac{Ry^3}{t^3}, \text{ nearly.}$$

For $\frac{M}{R} \times \frac{tt}{2}$ will appear by the Construction to be equal to the Sector N S P.

If S P be greater than C P, then take an Area H equal to the Sum of the Terms in the following Series: $\frac{py}{1} + \frac{t - \overline{n+1}^{\,2}|^{2} \times f}{3|^{3}}$ CASE II.

$$\times \frac{y^3}{t^2} + \frac{9t + \overline{n+3}^{\,2}|^{4} \times f}{5|^{5}} \times \frac{y^5}{t^4} + \frac{9 \times 25t - \overline{n+5}^{\,2}|^{6} \times f}{7|^{7}} \times \frac{y^7}{t^6} + \text{\&c. and}$$

the Area $\frac{1}{2} n \times H$, will be the Sector, as before.

For the Point S being on the contrary Side of the Centre to what it was before, it will eaſily appear, that the Change of $+f$ into $-f$, muſt reduce one Caſe to the other, without any other Proof.

Hence, if the Number n be taken equal to $\sqrt{\frac{t+f}{f}}$ or in this Caſe $\sqrt{\frac{p}{f}}$, then the Series for the Sector will want the ſecond Term, as in the former it wanted the Third. *Corollary*

Definition. The Angle called by *Kepler* the *Anomalia Eccentri*, is a fictitious Angle in the Elliptic Orbit of a Planet, being analogous to the Area described by a Line from the Centre of the Orbit, and revolving with the Planet from the Line of *Apsides*; in like manner as the *Mean Anomaly* is a fictitious Angle, analogous to the Area described by a Line from the Focus

Otherwise, if C be the Centre, S the Focus of an Elliptic Orbit described on the transverse Axis A P, and the Area N S P in the Circle be taken in Proportion to the whole, as the Area described in the Ellipsis about the Focus, to the whole: Then is the Arch of the Circle P N, or the Angle N C P, that which *Kepler* calls the *Anomalia Eccentri*.

This Angle may be measured either from the *Aphelion*, or from the *Perihelion*; in the following Proposition it is supposed to be taken from the *Perihelion*.

Prop. II. *The mean Anomaly of a Comet or Planet revolving in a given Elliptic Orbit being given; to find the* ANOMALIA ECCENTRI.

The Solution of this Problem requires two different Rules; the first and principal one serves to make a Beginning for a further Approximation, and the other is for the Progression in approximating nearer and nearer *ad libitum*.

I. *The Rule for the first Assumption:* Let t, f, and p, stand as before, for the Semi-transverse Axis of the Ellipsis, the Semi-distance of the Foci, and the *Perihelian* Distance; then taking the Number n equal to $\sqrt{5+\sqrt{25+\frac{9p}{f}}}$; let T stand for $\frac{2t}{nnt-\overline{nn-1}.p}$; and P for $\frac{2p}{nnt-\overline{nn-1}.p}$ (or $\frac{p}{t}T$); which constant Numbers, being once computed for the given Orbit, will serve to find the Angle required nearly by the following Rule.

Let M be the Number of Degrees in the Angle of mean *Anomaly* to the given Time, reckoned from or to the *Perihelion*; and supposing R, as before, to stand for 57,2957, &c. Degrees; take the Number $N=\sqrt[3]{\frac{3T}{nR}}M$, and let A be the Angle whose Sine is $N\sqrt[3]{\frac{1}{2}+\sqrt{\frac{1}{4}+\frac{P^3}{N^6}}}$ $+N\sqrt[3]{\frac{1}{2}-\sqrt{\frac{1}{4}+\frac{P^3}{N^6}}}$; then the Multiple Angle $n\times A$ will be nearly equal to the *Anomalia Eccentri*.

The Truth of which will appear from the Resolution of the Cubic Equation in the last *Corollary* to the preceding Proposition.

If

If the Quadruple of the Quantity $\frac{P^3}{N^6}$ be many times greater or many times lefs than Unity; or, which amounts to the fame, if the mean *Anomaly* M, be many times lefs, or many times greater, than the Angle denoted by the given Quantity $\frac{2np}{3t} R\sqrt{P}$ (one or the other of which two Cafes moft frequently happens in Orbits of very large Eccentricity) then the Theorem will be reduced to a fimpler Form near enough for Ufe. *Corol.* 1.

If M be many times lefs than $\frac{2np}{3t} R\sqrt{P}$, then the Angle A may be taken for that whofe Sine is $\frac{t \times M}{np \times R}$. *Cafe* I.

If M be many times greater than $\frac{2np}{3t} R\sqrt{P}$, then let A be the Angle whofe Sine is $N - \frac{P}{N}$; and the Multiple Angle $n \times A$, according to it's Cafe, will be nearly equal to the Angle required. *Cafe* II.

In Orbits of very large Eccentricity, the *Perihelian* Diftance p is many times lefs than the Semi-diftance of the Foci f, and the Number $n = \sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$; is always nearly equal to $\sqrt{10}$ or to the Integer 3, either of which may be ufed for it without any material Error in the Orbits of Comets. *Corol.* 2.

II. *The Rule for a further Correction* ad libitum.

Let M be the given mean *Anomaly*, t the Semi-tranfverfe Axis, as before; and let B be equal to or nearly equal to the Multiple Angle $n \times A$ before found, then if μ be the mean *Anomaly*, and x the Planet's Diftance from the Sun, computed to the *Anomalia Eccentri* B; the Angle $\acute{B}$ taken equal to $B + \frac{t}{x} \times \overline{M - \mu}$, will approach nearer to the true Value of the Angle fought; and by Repetitions of the fame Operation, the Approximation may be carried on nearer and nearer, *ad libitum*.

This laft Rule being obvious, the Explication of it may be omitted at prefent.

In this Solution, where the Motion is reckoned from the *Perihelion*, the Rule is univerfal, and under no Limitation, but had the Motion been taken from the *Aphelion*, the Problem muft have been divided *Scholium.*

 into

into two Cafes: One is, when the Eccentricity is lefs than $\frac{9}{16}$; the other is, when it is not lefs, but is either equal to, or more than in that Proportion.

If the Eccentricity be not lefs than $\frac{9}{16}$, then the fame Rule will hold, as before, only putting the *Aphelian* Diftance, fuppofe (a) inftead of the *Perihelian* Diftance (p), and fubftituting $-f$ for $+f$ in the Rule for the Number n.

If the Eccentricity be lefs than $\frac{9}{16}$, then take the Number n equal to $\sqrt{\frac{a}{f}}$, and $\frac{t}{na} \times \frac{M}{R}$ will be nearly equal to the Sine of the Submultiple Part of the *Anomalia Eccentri* denominated by the Number n, as before.

It is needlefs to obferve, that the like Rules would obtain in Hyperbolic Orbits, *mutatis mutandis.* But that which perhaps may not appear unworthy of being remarked, concerning this fort of Solution from the Cubic Root, is, that although the Rule be altogether impoffible, upon a total Change of the Figure of the Orbit either into a Circle, or into a Parabola; yet it will operate fo much better, and ftand in need of lefs Correction, according as the Figure advances nearer in it's Change towards either of thofe two Forms.

That the Ufe of the Method may better appear, it may not be amifs to add a few Examples.

I have given two for the Orbits of Planets, one the moft, and the other the leaft Eccentric; but which are more to fhew the Extent of the Rule, than to recommend the Ufe of it in fuch Cafes; for there are many other much better and more expeditious Methods in Orbits of fmall Eccentricity. The other two Examples are adapted to the Orbits of two Comets, whofe Periods have been already difcovered by Dr *Halley*; the one is to fhew the Ufe of one of the Rules in the firft *Corollary*, and the other is to explain the Ufe of the other Rule.

EXAMPLE I. *For the Orbit of* Mercury.

If an Unit be put for the Semi-tranfverfe Axis (t), the Eccentricity 0,20589 will become (f), and the *Perihelian* Diftance (p) will be 0,79411; wherefore by means of the Number R given as before, the conftant Numbers for this Orbit will appear to be, $n = 3{,}56755$, $T = 0{,}5857271$, $P = \frac{p}{t} T = 0{,}4651319$, and hence $\frac{3T}{n \times R} = 0{,}0085965$.

Example.

Suppofe M the mean *Anomaly* from the *Perihelion* to be 120°. 00′. 00″, to which it is required to find the *Anomalia Eccentri.*

Here, fince the mean *Anomaly* M is not many times more than the limiting Angle $\frac{2np}{3t} R \sqrt{P}$, (which in this Orbit is about 74 Degrees) Recourfe muft be had to the general Rule in the Propofition.

The

The Number N then, which is $\sqrt[3]{\frac{3\,T}{n\,R}\,M}$ will be $= 1{,}0104195$;

which found gives $N\sqrt[3]{\frac{1}{2} + \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}} = 1{,}0389090$; and also

$N\sqrt[3]{\frac{1}{2} - \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}} = -\,0{,}4477126$. Wherefore the Sum of both (under their proper Signs) *viz.* 0,5911964 will be the Sine whose Arch $36^\circ{,}24195$ is the Angle A; the Multiple whereof $n \times A = 129^\circ{,}295503$, will be the Angle to be first assumed for the *Anomalia Eccentri*.

For a further Correction; this Angle, now called B, whose Sine is suppose y, and it's Cosine z, gives, by a known Rule, $t + \frac{f}{t}\,z = 1{,}1304$ for x the Planet's Distance from the Sun; and by another known Rule $B - \frac{fR}{tt}\,y = 120^\circ{,}16568$ for μ the mean *Anomaly* to the *Anomalia Eccentri* B. Wherefore the correct Angle $B = B + \frac{t}{x} \times \overline{M - \mu}$ will be $129^\circ{,}14846 = 129^\circ.\ 08'.\ 54''{,}5$, erring, as will appear from a further Correction, about $\frac{1}{10}$ of a Second.

This Angle being thus determined, will give by the common Methods $137^\circ.\ 48'.\ 33''\frac{1}{2}$, for the true *Anomaly* or Angle at the Sun: The Sine of the true *Anomaly* being in Proportion to the Sine of the *Anomalia Eccentri*, as the Semi-conjugate Axis to the Planet's Distance from the Sun. So that the Equation of the Center in this Example is $17^\circ.\ 48'.\ 33''\frac{1}{2}$.

Supposing, as before, the mean Distance t to be Unity, and the Eccentricity f to be 0,0069855; the constant Numbers for this Orbit will be, $p = 0{,}9930115$; $n = 6{,}4116$; $T = 1{,}562134$; $P = 0{,}1551217$; $\frac{3\,T}{n\,R} = 0{,}0127571$; and the limiting Angle $\frac{2\,n\,p}{3\,t}\,R\,\sqrt{P}$, will appear to be about 303 Degrees. EXAMPLE II. *For the Orbit of* Venus.

Let M be $120^\circ.\ 00'.\ 00''$, as in the former Example. Then, since the mean *Anomaly* is, in this Case, not many times less than the limiting Angle, the general Rule must be used as before; according to which the Number N will appear to be 1,152585; the Sine of A will be 0,3217917; the Angle A, $18^\circ{,}77132$; and the Multiple $n \times A$, or Angle B; for the first Assumption of the *Anomalia Eccentri* will be $120^\circ{,}35416$. *Example.*

This

This Angle B will give, by the Method before explained, the Angle $B' = 120°,34555$, or $120°. 21'. 44''$ *ferè*, for the *Anomalia Eccentri* correct; the Error of which will appear, upon Examination, to be but a ſmall Part of a Second.

In this Example the true *Anomaly* is $120°. 41' 25'',1$; and conſequently the Equation of the Center no more than $41'. 25'',1$.

EXAMPLE III. *For the Orbit of the* Comet *of* 1682.

To know the mean *Anomaly* of this Comet to any given Time, it is to be premiſed, that it was at the *Perihelion* in the Year 1682, on the 4th Day of *September*, at 21 Ho. 22 Min. equated Time to the Meridian of *Greenwich*, and makes it's Revolution about the Sun, as Dr *Halley* has diſcovered, in $75\frac{1}{2}$ Years.

The *Perihelian* Diſtance p is, according to his Determination, 0,0326085 Parts of the mean Diſtance t. So that the conſtant Numbers for the Orbit will be, $n = 3,1676061$; $T = 0,2054272$; $P = 0,00669867$; and the limiting Angle $\frac{2\,np}{3\,t}\ R \surd\ P$ will be about 19 Minutes or $\frac{1}{3}$ of a Degree.

In the Orbits of Comets, the Rule for the firſt Aſſumption of the *Anomalia Eccentri* is generally ſufficient without Correction.

Thus, ſuppoſe the mean *Anomaly* M to be 0,072706, (as it was at the Time of an Obſervation made at *Greenwich* on the 30th of *Auguſt* 1682, at $7^h\ 42'$. Æq. T.) then the general Rule (which muſt be here uſed, ſince the Angle of mean *Anomaly* is not above 4 or 5 Times leſs than the limiting Angle) will give $n \times A$ or $B = 2°.\ 12'.\ 48'',7$, erring about $\frac{1}{10}$ of a Second from the true *Anomalia Eccentri*.

But in theſe Orbits the Rules in the firſt *Corollary* to the ſecond Propoſition moſt frequently take Place, eſpecially the laſt; and the Calculation may alſo be further abbreviated, by putting the ſquare Root of 10, or the Integer 3, for the Number n.

Example.

Suppoſe the mean *Anomaly* to be $0°,006522$, or $23'',4792$: Here, ſince M is 50 Times leſs than the limiting Angle, the Rule in the firſt Caſe of the firſt *Corollary* may be uſed; that is, to take the Sine of the Angle $A = \frac{t \times M}{n\,p \times R}$.

Wherefore, if the Number 3 be put for n, the Sine of A, which is $\frac{t\ M}{3\,p\ R}$, will be $= 0,00116367$; and conſequently the Angle A will be $4'. 00''\ 011$; and the multiple Angle $n \times A$ to be aſſumed for the *Anomalia Eccentri* will be $12'.\ 00'',033$, the Error of which will be found to be about $\frac{1}{30}$ of a Second.

EXAMPLE IV. *For the Orbit*

This Comet, according to Dr *Halley*, performs it's Period in 575 Years; and was in it's *Perihelion* on the 7th of *December* 1680, at

23

23 Hours 09′ Æq. T. at *London*; the *Perhelian* Diſtance p is 0,000089301, in Parts of the mean Diſtance t: Wherefore ſuppoſing the Number n to be $\sqrt{10}$, the conſtant Numbers for the Orbit will be $T = 0{,}2000161$; $P = 0{,}000017862$, and the limiting Angle $\frac{2np}{3t} R\sqrt{P}$ will be about $\frac{1}{6}$ of a Second. *of the great Comet of the Year 1680.*

Suppoſe the mean *Anomaly* to be 3′, 31″,4478 or 0°,05873541, (as it was at the Time of the firſt Obſervation made on it in *Saxony*, on *November* the 3d, at 16h. 47′ Æq. T. at *London*.) here, ſince the mean *Anomaly* is many times greater than $\frac{1}{6}$ of a Second, the Rule in the ſecond Caſe of the firſt *Corollary* may be uſed; that is, by taking the Sine of $A = N - \frac{P}{N}$. *Example.*

But the Number N or $\sqrt[3]{\frac{3T}{nR}}$ is $= 0{,}05794134$; and $\frac{P}{N}$ will be $= 0{,}0030827$; wherefore $(N - \frac{P}{N} =)$ 0,05763307, will be the Sine whoſe Arch 3°,30397 is the Angle A; and the multiple Angle $n \times A =$ 10°. 26′. 53″, 05, will be the Angle to be firſt aſſumed for the *Anomalia Eccentri*; the Error of which will be found to be leſs than a Second.

The true *Anomaly*, computed from this Angle according to the Rule in the Example for *Mercury*, will appear to be 171°. 38′. 24″. from the *Perihelion*.

By theſe Examples it appears, that the Solution is univerſal in all Reſpects; for the two firſt, compared with the two laſt, ſerve to ſhew that it is not confined to any particular Parts of the Orbit, but extends to all Degrees of mean *Anomaly*: And by comparing the ſecond with the laſt, it ſufficiently appears to be univerſal with reſpect to the ſeveral Degrees of Eccentricity; ſince in one the Equation of the Center for the Reduction of the Mean to the true Motion is not ſo much as the $\frac{1}{170}$th Part of the whole; whereas in the other it amounts to almoſt 3000 times as much as the mean Motion itſelf.

Upon reviewing the Reflections on the Quadrature of the Circle in Page 77, I believe it may be neceſſary for me to prevent any Miſtake that may ariſe from the different Opinions that obtain about the Nature of Mathematical Quantity, to explain myſelf a little upon that Head; as alſo to add a few Words to ſhew how the Method of Quadrature by limiting Polygons, takes Place in other Figures as well as the Circle. Poſtſcript.

I take then a Mathematical Quantity, and that for which any Symbol is put, to be nothing elſe but Number with Regard to ſome Meaſure which is conſidered as one. For we cannot know preciſely and deter-

determinately, that is, mathematically, how much any thing is, but by means of Number. The Notion of continued Quantity, without regard to any Meaſure, is indiſtinct and confuſed ; and although ſome Species of ſuch Quantity, conſidered phyſically, may be deſcribed by Motion, as Lines by Points, and Surfaces by Lines, and ſo on ; yet the Magnitudes or Mathematical Quantities are not made by that Motion, but by numbering according to a Meaſure.

Accordingly, all the ſeveral Notations that are found neceſſary to expreſs the Formations of Quantities, do refer to ſome Office or Property of Number or Meaſure ; but none can be interpreted to ſignify continued Quantity as ſuch.

Thus ſome Notations are found requiſite to expreſs Number in it's ordinal Capacity or the *Numerus Numerans*, as when one follows or precedes another, in the firſt, ſecond, or third Place from that upon which it depends ; as the Quantities $\grave{x}$, $\grave{\grave{x}}$, x, $\acute{x}$, $\acute{\acute{x}}$, referring to the principal one x.

So, in many Caſes, a Notation is found neceſſary to be given to a Meaſure as a Meaſure ; as for Inſtance, Sir *I. Newton*'s Symbol for a Fluxion $\dot{x}$; for this ſtands for a Meaſure of ſome Kind, and accordingly he uſually puts an Unit for it, if it be the principal one upon which the reſt depend.

So ſome Notations are expreſly to ſhew a Number in the form of it's Compoſition, as the Index to the Geometrical Power x^n denoting the Number of equal Factors which go to the Compoſition of it, or what is analogous to ſuch.

But that there is no Symbol or Notation but what refers to diſcreet Quantity, is manifeſt from the Operations, which are all Arithmetical.

And hence it is, there are ſo many Species of Mathematical Quantity as there are Forms of compoſite Numbers, or Ways in the Compoſition of them ; among which there are two more eminent for their Simplicity and Univerſality than the reſt : One is the *Geometrical Power* formed from a conſtant Root ; and the other, though well known, yet wanting a Name as well as a Notation, may be called the *Arithmetical Power* ; or the Power of a Root uniformly increaſing or diminiſhing, and is that whoſe Notation is deſigned in Page 78 : The one is only for the Form of the Quantity itſelf, the other is for the Conſtitution of it from it's Elements.

Now from the Properties of either of theſe it would be eaſy to ſhew how the Quadratures of ſimple Figures are deducible from the Areas of their limiting Polygons. I ſhall juſt point out the Method from the Arithmetical Power, as being the ſhorteſt and readieſt at Hand.

Let z, $\acute{z}$, $\acute{\acute{z}}$, &c. or z, $\grave{z}$, $\grave{\grave{z}}$, &c. be Quantities in Arithmetical Progreſſion, diminiſhing or increaſing by the common Difference

rence $\dot{z}$, and let, as before explained, $\underset{\frown}{\overset{\dot{z}}{z}}{}^{(m}$ signify the Arithmetical Power of z, denominated by the potential Index m, namely, $z \times \acute{z} \times \H{z}$, &c. whose first Root is z and last $z - \overline{m-1} \times \dot{z}$; which being supposed, the Element of the Arithmetical Power will be $m\dot{z} \times \underset{\frown}{\overset{\dot{z}}{\acute{z}}}{}^{(m-1}$ that is, the Product made from the Multiplication of the two Indices, and the next inferior Power of the next Root in Order. For the first Arithmetical Power $\underset{\frown}{\overset{\dot{z}}{z}}{}^{(m}$ is $= z . \underset{\frown}{\overset{\dot{z}}{\acute{z}}}{}^{(m-1}$, and the next $\underset{\frown}{\overset{\dot{z}}{\acute{z}}}{}^{(m}$ is $= \underset{\frown}{\overset{\dot{z}}{\acute{z}}}{}^{(m-1} \times \overline{x - m\dot{z}}$, wherefore the Difference will be as is explained.

And consequently, since the Sum of these Elements or Differences, taken in order from the first to the last, do make up the Quantity according to it's *termini*; hence, if z be the Abscifs of a curvilinear Figure whose Ordinate y is equal to $m z^{m-n}$, a Demonstration might easily be made that [the Form of the Quantity for] the Area will be z^{m}; that is, the same Multiple of the next superior Power of z divided by the Index of that Power.

For since the Arithmetical Powers do both unite and become the same with the Geometrical Power, when the differential Index $\dot{z}$ is supposed to be nothing; the Magnitude of the Geometrical Figure will be implied from the Magnitudes of the two Polygons made up of Rectangles, one from the increasing Arithmetical Power, the other from the diminishing, although it be true, that the Elements of the Polygons cannot be summed up, when $\dot{z}$, the Measure of the Abscifs z, is supposed to be nothing.

In like manner, in any other Case where z and $\acute{z}$ are two Abscisses whose Difference as a Measure is $\dot{z}$; and y, $\acute{y}$ the two Ordinates; the Magnitude of the Figure will be implied by the Magnitudes of the two Polygons which are made from the Sum of the inscribing and circumscribing Elements $\dot{z}y$ and $\dot{z}\acute{y}$, although the Figure itself is not to be resolved into any such primogenial rectangular Elements.

And thus, I think, the Symbol $\dot{z}$, considered as a component Part of the Rectangle $\dot{z}y$, may bear a plain Interpretation; *viz.* that it is the Measure according to which the Quantity z is measured; nor can I see that any other Interpretation need to be put upon a Symbol, which, like a Measure, is used only to make other things known, but is of itself for nothing but a Mark.

And what is ſaid of the Elements of the firſt Reſolution, is eaſily applied to thoſe of a ſecond or third, and ſo on; the laſt may always be conſidered as the Meaſure of the former and indiviſible, although, in reſpect of the following, it be taken as the Part according to which the Meaſure was made, and therefore diviſible.

An Inquiry concerning the Figure of ſuch Planets as revolve about an Axis, ſuppoſing the Denſity continually to vary, from the Centre towards the Surface; by Mr Alexis Clairaut, *F. R. S. and Member of the Royal Acad. of Sciences at* Paris. *Tranſlated from the* French *by the Rev.* John Colſon, *Lucaſ. Prof. Math.* Cantab. *and F. R S.* N°. 449. p. 277. *Aug. &c.* 1738.

IX. Notwithſtanding that Part of Sir *I. Newton*'s *Mathematical Principles of Natural Philoſophy*, where he treats of the Figure of the Earth is delivered with the uſual Skill and Accuracy of that great Author; yet I thought ſomething farther might be done in this Matter, and that new Inquiries may be propoſed, which are of no ſmall Importance, and which poſſibly he overlooked, through the Abundance of thoſe fine Diſcoveries he was in Purſuit of.

What at firſt ſeemed to me worth examining, when I applyed myſelf to this Subject, was to know why Sir *Iſaac* aſſumed the Conical Ellipſis for the Figure of the Earth, when he was to determine it's Axis? For he does not acquaint us why he did it, neither can we perceive how he had ſatisfied himſelf in this Particular: And unleſs we know this, I think we cannot entirely acquieſce in his Determinations of the Axes of the Planets. It ſeems as if he might have taken any other oval Curve, as well as the Conical Ellipſis of *Apollonius*, and then he would have come to other Concluſions about thoſe Axes.

I began then with convincing myſelf by Calculation, that the Meridian of the Earth, and of the other Planets, is a Curve very nearly approaching to an Ellipſis; ſo that no ſenſible Error could enſue by ſuppoſing it really ſuch. I communicated my Demonſtration of this to the *Royal Society*, at the Beginning of the laſt Year; and I have ſince been informed, that Mr *Stirling* *, had inſerted a Diſcourſe in the *Philoſophical Tranſactions*, wherein he had found the ſame thing before me, but without giving his Demonſtration. When I ſent that Paper to *London*, I was in *Lapland*, within the frigid *Zone*, where I could have no Recourſe to Mr *Stirling*'s Diſcourſe, ſo that I could not take any Notice of it.

The Elliptical Form of the Meridian being once proved, I no longer found any thing in Sir *I. Newton*, about the Figure of the Earth, which could create any new Difficulty; and I ſhould have thought this Queſtion ſufficiently diſcuſſed, if the Obſervations made under the Arctick Circle had not prevailed on us to believe, that the Shape of the Earth was ſtill flatter than that of Sir *Iſaac*'s Spheriod; and if he himſelf had not pointed at the Cauſes, which might make *Jupiter* not quite ſo flat, as by his Theory, and the Earth ſomething more.

As to *Jupiter*, he ſays †, that it's Equator conſiſts of denſer Parts than the reſt of it's Body, becauſe it's Moiſture is more dried up by the Heat of the Sun. But as to the Earth, he ſuſpects it's Flatneſs to be a ſmall matter greater than what ariſes by his Calculation. He

* See Chap. VII. of this Volume. † Princip. Math. Edit. 3. p. 416.

insinuates

insinuates, that it may possibly be more dense towards the Center than at the Superficies *. I am something surprized that Sir *Isaac* should imagine, that the Sun's Heat can be so great at *Jupiter*'s Equator, when it has no such Effect at that of the Earth; and that he does not ascribe each to a like Cause, by supposing also, that *Jupiter* may be of a different Density at the Center from that at the Superficies.

But whatever Reason he might have for introducing two different Causes, I give the Preference to the Hypothesis which supposes unequal Densities at the Center and at the Circumference. I have inquired, by the Assistance of this Theory, what would be the Figure of the Earth, and of the other Planets which revolve about an Axe, on Supposition that they are composed of similar *Strata*, or Layers, at the Surface; but that their variable Density, from the Center towards the Circumference, may be expounded by any Algebraical Equation whatsoever.

And though my Hypothesis should not be conformable to the Laws of Nature, or even though it should be of no real Use (which would be the Case, if the Observations made by the Mathematicians now in *Peru*, compared with ours in the North, should require that Proportion of the Axes, which is derived from Sir *Isaac*'s Spheroid;) I thought however that Geometricians would be pleased with the Speculations contained in this Paper, as being, if not useful, yet curious Problems at least.

To find the Attraction which a homogeneous Spheroid B N E b e, *differing but very little from a Sphere, exerts upon a Corpuscle placed at* A *in the Axis of Revolution.*

PART I. *In which are found the Laws of Attraction, which are exerted upon Bodies at a Distance, by a Spheroid composed of Orbs of different Degrees of Density.*

Prob. I. Fig. 35.

I. We may conceive the Space B N E b D M B, included between the Spheroid and the Sphere, to be divided into an infinite Number of Sections perpendicular to the Axe A C b. Supposing then that every one of the Particles, which are contained in one of these Elements or Moments N n m M, exerts the same Quantity of Attraction upon the Body at A, which may be supposed because of the Smallness of N M; we shall have $c\,\alpha \times PM^2 \times Pp \times \frac{AP}{AM^3}$ for the Attraction of any one of those Elements; putting c for the Ratio of the Circumference to the Radius, and α for the given Ratio of M N to P M, that is, of D E to C D.

Now if we make $CA = e$, $CB = r$, $AM = z$; and for P M, A P, P p, if we substitute their Values expressed by z, and then seek the Fluent of the foregoing Quantity; we shall have $\frac{4\,c\,\alpha\,r^3}{3\,e\,e}$

* See Sect. xxiv.

$-\frac{4c\alpha r^5}{5e^4}$ for the Value of the whole Attraction of the Solid generated by the Revolution of B D b E B: To which if we add $\frac{2r^3c}{3ee}$ the Attraction of the Sphere, we shall have $\frac{2r^3c}{3ee}+\frac{4cr^3\alpha}{3ee}$ $-\frac{4c\alpha r^5}{5e^4}$ for the required Attraction of the Spheroid upon the Corpuscle A.

Prob. II. Fig. 36. *Supposing now the Spheroid* B e b e, *to be no longer of a homogeneous Matter, but to be composed of an infinite Number of Elliptical* Strata, *all similar to* B E b, *the Densities of which are represented by the Ordinates* K T *of any Curve whatever* V T, *of which we have the Equation between* C K *and* K T; *the Attraction is required which this Spheroid exerts upon a Corpuscle placed at the Pole* B.

II. Making B C $=e$, C K $=r$, by the foregoing Proposition, we should have $\frac{2r^3c}{3ee}+\frac{4cr^3\alpha}{3ee}-\frac{4c\alpha r^5}{5e^4}$ for the Attraction of the Spheroid K L K, if it consisted of homogeneous Matter; and the Fluxion of this Quantity $\frac{2rrc\dot{r}}{ee}+\frac{4c\alpha r^2\dot{r}}{ee}-\frac{4c\alpha r^4\dot{r}}{e^4}$ would be the Element or Moment of the Orb K L K $k\,l\,k$. But because the Density is variable, we must multiply this Value of the Attraction of the Orb by K T, and the Fluent of this Quantity will be the Value of the Attraction of the Spheroid K L K.

As to the Value of K T, which expresses the Density of the Stratum or Bed K L K $k\,l\,k$, we shall take only fr^p+gr^q, because we shall see afterwards, that a Value more compounded, at fr^p+gr^q, $+hr^s+ir^t$, &c. which by the Property of Series may express all Curves, would not produce any Variety in the Calculation.

Therefore multiplying the foregoing Equation by fr^p+gr^q, we shall have $\frac{2cf\times\overline{1+2\alpha}\times r^{3+p}}{ee\times\overline{3+p}}-\frac{4c\alpha f r^{5+p}}{e^4\times\overline{5+p}}$ $+\frac{2cg\times\overline{1+2\alpha}\times r^{3+q}}{ee\times\overline{3+q}}-\frac{4c\alpha g r^{5+q}}{e^4\times\overline{5+q}}$ for the Quantity of Attraction of the Spheroid K L K, exerted upon a Corpuscle placed at B.

III. In this Value making $r = e$, we ſhall have $\frac{2cfe^{1+p}}{3+p}$

$$+ \frac{8cfe^{1+p}\alpha}{3+p \times 5+p} + \frac{2cge^{1+q}}{3+q} + \frac{8cge^{1+q}\alpha}{3+q \times 5+q}$$ which will expreſs the Force of Attraction of the Spheroid B E b, exerted upon a Corpuſcle placed at the Pole B.

A Corpuſcle being placed in any Point N *of the Surface of the foregoing Spheroid* B E *b e, I ſay it will undergo the ſame Attraction from this Spheroid, as if it were placed at the Pole* N *of a ſecond Spheroid revolving about the Axe* N O, *the ſecond Axe being the Radius of a Circle equal in Superficies to the Ellipſis* F G; *ſuppoſing this ſecond Spheroid* N G O F, *to be compoſed of the* Strata M *m q* Q, *whoſe Denſities are the ſame as thoſe of the* Strata K *k* L *l* K *k*, *of the firſt Spheroid.* THEOREM. Fig. 37.

IV. In the Diſcourſe which I communicated to the Royal Society *, being then at *Torneo*, printed in the *Philoſophical Tranſactions*, I have demonſtrated this Propoſition as to a homogeneous Spheroid; and the ſame Reaſoning will obtain in this Caſe alſo.

To find the Attraction which the Spheroid B *e b e exerts upon a Corpuſcle placed at any Point* N *of the Superficies.* PROB. III. Fig. 36.

V. We will make, as above, B C $= e$, C E $= e + e\alpha$, and alſo C N $= e + e\lambda$, and half the Conjugate Diameter of C N will be C G $= e + e\alpha - e\lambda$; whence the Radius of a Circle, equal in Superficies to the Ellipſis F G, will be a mean Proportional between C E and C G, that is to ſay, $e + e\alpha - \frac{1}{2}e\lambda$. Therefore the Spheroid B E $b\,e$ exerts the ſame Attraction at N, as would be exerted at the Pole of a Spheroid N G O F, (*Fig.* 37.) of which the principal Axis would be N O $= 2e + 2e\lambda$, and the ſecond would be to the Principal as $1 + \alpha - \frac{3}{2}\lambda$ to 1.

Therefore in the Expreſſion of the Attraction at the Pole, (Art. III.) we muſt ſubſtitute $e + e\lambda$ inſtead of e, and $\alpha - \frac{3}{2}\lambda$ inſtead of α.

* See Chap. vii. of this Volume.

But

But if f and g must no longer be the same; for we may easily perceive by the foregoing *Theorem*, that the Density must be the same in this Speroid N G O F, at the Distance $r + r\lambda$ from the Center, as it is in the Spheroid B E b e at the Distance r. Therefore $f \left(\frac{e}{1+\lambda}\right)^p + g\left(\frac{e}{1+\lambda}\right)^q$ must be put instead of $fe^p + ge^q$.

Thus we shall have $\frac{2cfe^{1+p}}{3+p} + \frac{\overline{2p-2}\,cf\lambda e^{1+p}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha e^{1+p}}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{1+q}}{3+q} + \frac{\overline{2q-2}\,cg\lambda e^{1+q}}{\overline{3+q}\times\overline{5+q}} + \frac{8cg\alpha e^{1+q}}{\overline{3+q}\times\overline{5+q}}$ for the Attraction of the Spheroid B E b e at N.

VI. If we make $\lambda = \alpha$, the foregoing Expression will be reduced to this $\frac{2cfe^{1+p}}{3+p} + \frac{2cfe^{1+p}\alpha}{5+p} + \frac{2cge^{1+q}}{3+q} + \frac{2cge^{1+q}\alpha}{5+q}$, which expresses the Attraction of the Equator.

VII. If we would have the Attraction at any Point M within the Spheroid, in the Expression of the Attraction at N, we must put r instead of e. The Proof of this is plain from the same Reasons that Sir *I. Newton* makes use of *, to shew that the Attraction of an elliptic Orb, at a Point within it, is none at all.

PROB. IV. Fig. 38. *Let* R Π r π *be a Circle whose Center is* Y; *'tis required to find the Attraction which this Circle exerts upon a Corpuscle at* N, *according to the Direction* H Y; *supposing the Point* H, *which answers perpendicularly below the Point* N, *to be at a very small Distance from the Point* Y.

VIII. Let there be drawn Π H π perpendicular to the Diameter R Y r, and let the Space R Π π be transferred to π Π Z. Then the Space π Z Π r will be the only Part of R Π r π, which will attract the Body N according to H Y.

* *Princip. Math. Lib.* I. *Prop.* 91. *Corol.* 3.

To

To find the Attraction of this little Space, we will suppose it to be divided into the Elements $T\,t\,s\,S$, the Attractions of which, according to HY, will be $\frac{T\,t\,s\,S \times QT}{NT^3}$, or $\frac{2\,HY \times Q\,q \times QT}{NT^3}$, the Fluent of which $\frac{2\,HY \times HQTZ}{NT^3}$ is the Attraction of $T\,Z\,r\,S$, according to HY. In which if we put $\Pi\,\pi$ for HQ, we shall have $\frac{\Pi\,H\,\pi\,R \times 2\,HY}{NT^3}$, or $\frac{\frac{1}{2}HY \times \Pi H^2 \times c}{NT^3}$, for the Attraction required.

IX. It is easy to perceive, that if, instead of a Circle, the Curve $R\,\Pi\,r$ were an Ellipsis, or any other Curve whose Axes were but very little different from one another, the foregoing Solution would be still the same.

To find the Attraction which an Elliptical Spheroid $K\,L\,k$ *exerts upon a Corpuscle placed without it's Surface at* N, *according to the Direction* CX *perpendicular to* CN. PROB. V. Fig. 39.

X. To perform this, we will begin by drawing the Diameter $C\,\mu\,\nu$, which bisects the Lines $R\,r$ perpendicular to CN; and the Ratio of CH to HY shall be called n. Then esteeming the Ellipsis $R\,r$ as a Circle, (see the foregoing Article) we shall have by the Problem aforegoing $\frac{\frac{1}{2}\,n\,c \times RH^2 \times CH}{NR^3}$ for it's Attraction, according to HY; which being multiplyed by the Fluxion of MH, the Fluent of this will be the Attraction of the Segment of the Spheroid $R\,M\,r$.

This Calculation being made, and $N\,m$ being substituted for NR, we shall have $\frac{2\,n\,c\,r^5}{5\,e^4}$ for the Attraction of the Spheroid in N, according to the Direction CX.

To find the Attraction of a Corpuscle N, *according to* CX, *towards an Ellipsoid* $B\,N\,E\,b\,e$, *composed of Strata, the Densities of which are defined by the Equation* $D = f\,r^p + g\,r^q$. PROB. VI.

XI. Take the Fluxion of the Quantity $\frac{2\,c\,n\,r^5}{5\,e^4}$, which expresses the Attraction of the homogeneous Ellipsoid $K\,L\,k$, and you will have $\frac{2\,c\,n\,r^4\,\dot{r}}{e^4}$ for the Attraction of an infinitely little elliptic Orb; which being multiplied by the Density D, gives $\frac{2cnfr^{4+p}\dot{r}}{c^4}+\frac{2cgnr^{4+q}\dot{r}}{c^4}$

the Fluent of which $\frac{2cfnr^{5+p}}{\overline{5+p}\times c^4}+\frac{2cgnr^{5+q}}{\overline{5+q}\times c^4}$, is the Attraction of the Spheroid K L k, according to C X. Therefore the total Attraction of the Spheroid B N E b e upon the Corpuscle N, according to the Direction C X, will be $\frac{2cfne^{1+p}}{5+p}+\frac{2cgne^{1+q}}{5+q}$.

Now if we have regard to the Smallness of the Line N ν, and observe how little the Angle ν N C will differ from a right one, we may perceive that the Diameter C N contains the same Angle with the perpendicular N X in N, as the Diameter C N with the perpendicular at ν; that is to say, that the Angle N C ν is the same as the Angle C N X; so that instead of n we may take $\frac{CX}{CN}$. Wherefore the foregoing Expression of the Attraction of the Ellipsoid B E b e, acting according to the Direction C X upon a Corpuscle placed in N, will be $\frac{2cfe^{1+p}}{5+p}\times\frac{CX}{CN}$ $+\frac{2cge^{1+q}}{5+q}\times\frac{CX}{CN}$.

Prob. VII. *To find the Direction of the Attraction of a Corpuscle* N *towards the Ellipsoid.*

XII. by the second Problem we shall find the Attraction of the Spheroid according to C N to be $\frac{2cfe^{1+p}}{3+p}+\frac{2cge^{1+q}}{3+q}$, by expunging what may be here expunged. Then by taking a fourth proportional to these three Quantities, the first of which is the Attraction according to C N, the second is that according to C X, and the third is the right line C N; there will arise $\dfrac{\frac{fe^{1+p}}{5+p}+\frac{ge^{1+q}}{5+q}}{\frac{fe^{1+p}}{3+p}+\frac{ge^{1+q}}{3+q}}\times CX = CI.$

Whence

Whence we shall have N I for the Direction required, of the Attraction of the Corpuscle N.

XIII. If we suppose $p=q=0$, that is, if the Spheroid be homogeneous, we shall have $CI=\frac{3}{5}CX$; which agrees with what Mr *Stirling* has found, in that curious Dissertation he has published in the *Philosophical Transactions, ut supra.*

XIV. Let us now suppose, that the foregoing Spheroid B N E b e, which is still composed of Beds or *Strata* of different Densities, revolves about it's Axis B b, and that it is now arrived at it's permanent State. It is plain that the Particles of the Fluid, which are upon it's Surface, must gravitate according to a Direction perpendicular to the Curvature B N E; for without this Condition there could be no *Æquilibrium.*

PART II. *The Use of the foregoing Problems, in finding the Figure of Spheroids, which revolve about an Axis.* Fig. 39.

We shall now inquire, whether the Elliptic Figure we have ascribed to our Spheroids can have this Property, and to produce this Effect what must be the Relation between the Time of Revolution of the Spheriod and the Difference of it's Axes.

Let us then put Φ for the centrifugal Force at the Equator, and the centrifugal Force at N will be $\frac{\phi\times PN}{CE}$, or $\frac{\phi\times Cx}{2CE\times\alpha}$, because $2PN\times\alpha=Cx$.

By resolving this centrifugal Force according to the Perpendicular to C N, we shall have $\frac{\phi\times CX}{2\alpha\times CE}$; which being added to $\frac{2cfe^{1+p}}{5+p}\times\frac{CX}{CN}+\frac{2cge^{1+q}}{5+q}\times\frac{CX}{CN}$, found by Prob. V. will give the whole Force of the Body N, according to the Direction C X, when the Spheroid is converted about it's Axis. But because this Body, by virtue of the Attraction according to C N, and the Force according to C X, ought to have a perpendicular Tendency to the Superficies; we shall have this Analogy, $CN.CX::\frac{2cfe^{1+p}}{3+p}+\frac{2cge^{1+q}}{3+q}\cdot\frac{\phi}{2\alpha}\times\frac{CX}{CE}+\frac{2cfe^{1+p}}{5+p}\times\frac{CX}{CN}+\frac{2cge^{1+q}}{5+q}\times\frac{CX}{CN}$. And hence, because C N and C E may be assumed as the same on this Occasion, it will be

$$\phi=\frac{8cfe^{1+p}\alpha}{3+p\times5+p}+\frac{8cge^{1+q}\alpha}{3+q\times5+q}.$$

 And

The Spheroid being ſuppoſed elliptical, Bodies will gravitate perpendicularly to it's Surface.

And as in this Value of the centrifugal Force, no Quantity enters but what will agree to any Point N; we may therefore conclude, that when our ſuppoſed elliptical Spheroid performs it's Rotation in a proper Time, ſo that the centrifugal Force at the Equator may be as before; then the centrifugal Force in any other Place N will be ſuch as it ought to be, to cauſe Bodies to gravitate in a perpendicular Direction to the Surface.

The Expreſſion for the Gravity at any Place on the Spheroid.

XV. If we now conſider, that E D being taken for the centrifugal Force in E, then will M N expreſs the centrifugal Force in N, and conſequently M I will be ſuch a Part of this Force as acts according

Fig. 40.

to N C; we ſhall have $\frac{8cfe^{\overline{1+p}}\lambda}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{\overline{1+q}}\lambda}{\overline{3+q}\times\overline{5+q}}$ to be ſubtracted from the Attraction at N. Hence $\frac{2cfe^{\overline{1+p}}}{3+p} + \frac{\overline{2p-10}\,cf\lambda e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}}$

$+ \frac{8cf\alpha e^{\overline{1+p}}}{\overline{3+q}\times\overline{5+q}} + \frac{2cge^{\overline{1+q}}}{3+p} + \frac{\overline{2q-10}\,cg\lambda e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}} + \frac{5cg\alpha e^{\overline{1+q}}}{\overline{8+q}\times\overline{5+q}}$

will be the Gravity at N.

The Gravity at the Equator.

XVI. In this Value making $\lambda = \alpha$, we ſhall have $\frac{2cfe^{\overline{1+p}}}{3+p}$

$+ \frac{\overline{2p-2}\,cf\alpha e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{\overline{1+q}}}{3+q} + \frac{\overline{2q-2}\,cg\alpha e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$ for the Gravity at the Equator.

XVII. If we ſubtract the Value of the Gravity in N from the Value of the Attraction or Gravity at the Pole, (Art. III.) we ſhall have $\frac{\overline{10-2p}\,cf\lambda e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{\overline{10-2q}\,cg\lambda e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$. But it is eaſy to perceive, that λ is proportional to the Square of the Sine of the Arc P M, or of the Complement of the Latitude. Whence we may therefore conclude, that the Diminution of the Gravity from the Pole to the Equator is proportional to the Square of the Coſine of the Latitude; or, which is the ſame thing, that *the Augmentation of Gravity from the Equator to the Pole is as the Square of the Sine of the Latitude,* as Sir *I. Newton* has demonſtrated in his Hypotheſis of a homogenous Spheroid.

XVIII. From the following Calculation it is eaſy to conclude, that Sir *Iſaac*'s Theorem *, which is this, that *the Gravity in any Place within*

* *Prin. Math.* Lib. 3. Prop. 20.

is reciprocally as the Diſtance from the Centre, cannot obtain here. For we may ſee by the foregoing Expreſſion, that the Gravity in N cannot be to the Gravity in P as 1 to $1 + \lambda$, except when $p = q = 0$, which happens only in Sir *Iſaac*'s homogeneous Spheroid.

It was for want of conſidering, that this Theorem was demonſtrated by Sir *Iſaac* only in the Caſe of his homogeneous Spheroid, that ſeveral Geometricians have too haſtily concluded, this Theorem might be applied to determine the Ratio of the Earth's Axes, and the Lengths of the Pendulum obſerved in two Places of different Latitudes. Dr *Gregory* is one of thoſe who have fallen into this Miſtake *, And in the *Philoſ. Tranſact.* † it is concluded, from the Proportion of Gravity at *Jamaica* to that at *London*, that the Diameter of the Equator muſt exceed the Earth's Axis by $\frac{1}{190}$th Part, which Computation was founded on this 20th Prop. Lib. III. of Sir *Iſaac*'s *Principia*, which is true only of his Spheroid.

XIX. Let us now ſuppoſe, that the centrifugal Force at the Equator is known by obſervation, as alſo within the Earth, *&c.* and that it is a certain Part $\frac{1}{m}$ of the Gravity; by Articles XIV, and XVI, we ſhall have this Equation: $\frac{2cfe^{1+p}}{3+p} + \frac{\overline{2p-2}\,cfe^{1+p}\alpha}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{1+q}}{3+q}$ *The Manner of finding the Axes of the Spheroid, the Variation of the Denſities of the Strata being taken at pleaſure.*

$$+ \frac{\overline{2q-2}\,cge^{1+q}\alpha}{\overline{3+q}\times\overline{5+q}} = \frac{8cfme^{1+q}\alpha}{\overline{3+p}\times\overline{5+p}} + \frac{8cmge^{1+q}\alpha}{\overline{3+q}\times\overline{5+q}}.$$

From hence it will be eaſy to derive the Value of α, becauſe f, g, p, q, will be given, from the Hypotheſis that will be choſen, for the Variation of the Denſity in the internal Parts of the Spheroid.

XX. And if on the contrary α be given, that is, if we know by Obſervation the Ratio of the Axes of the Planet concerned; then by the foregoing Equation we may perceive, whether we have aſſumed an agreeable Hypotheſis for the Variation of the Denſities: But we cannot preciſely determine what this Hypotheſis muſt be, becauſe there is but one Equation, in which 4 indeterminate Quantities f, g, p, q, are involved. And indeed there might be many more than 4 indeterminate Quantities, if we ſhould aſſume more than two Terms in the general Equation of the Denſities $D = fr^p + gr^q + hr^s$, *&c.*

XXI. In order to apply the foregoing Theory to the Earth, it might ſeem at firſt Sight, that by the Aſſiſtance of Obſervations made for meaſuring the Length of the Pendulum, we might have other Equations, which with the foregoing Equation A, would determine the Coëfficients and Exponents now mentioned; but we ſhall ſoon ſee the Impoſſibility

* Elem. Aſtron. Lib. 3. *Sect.* 8. *Prop.* 52. † See Chap. III. of this Vol.

of this upon two Accounts: First, There need be only two Observations, as to what concerns the Length of the Pendulum. For because by Art. XVII. the Augmentation of the Gravity from the Equator to the Pole is proportional to the Square of the Sine of the Latitude, two Observations as much determine the Problem as an infinite Number can do: So that we could have but one other Equation besides the foregoing. This Equation will be

$$(B)\ \frac{\acute{p}-p}{p}=\frac{\dfrac{\overline{5-p}\,f\alpha}{\overline{3+p}\times\overline{5+p}}+\dfrac{\overline{5-q}\,g\alpha}{\overline{3+q}\times\overline{5+q}}}{\dfrac{\overline{p-1}\,f\alpha}{\overline{3+p}\times\overline{5+p}}+\dfrac{f}{3+p}+\dfrac{g}{3+q}+\dfrac{\overline{q-1}\,g\alpha}{\overline{3+q}\times\overline{5+q}}}.$$

The first Member of this Equation expresses the Gravity at the Equator subtracted from that at the Pole, and divided by that at the Equator; a Quantity which may be known in Numbers, by determining the Length of the Pendulum at two different Latitudes. The other Member of the Equation is an Expression of the same Quantity, as it is deduced by the preceding Calculus.

Secondly, This new Equation B cannot be of any Service in determining the Coëfficients and Exponents f, g, p, q, *&c.* For we shall now shew, that the foregoing Ratio $\frac{\acute{p}-p}{p}$ has such an immediate Connexion with α, that one of them being determined, the other will necessarily be so too, independently of the Values of f, g, p, q, *&c.* This may deserve our Attention, and the Proof is thus:

XXII. Because the Ratio of the Gravity to the Centrifugal Force is very great, and is expressed by m, in the Equation A we may reject the third and fourth Terms; by which means the Equation will be reduced to this, $\frac{f}{3+p}+\frac{g}{3+q}=\frac{4mf\alpha}{\overline{3+p}\times\overline{5+p}}+\frac{4mg\alpha}{\overline{3+q}\times\overline{5+q}}$. And if from this Equation we deduce the Value either of f or g, and substitute it in the Equation B; (having first rejected the first and fourth Terms of the Denominator, as in this Case may be done) we shall have after the Calculation is made, whatever is the Number of Terms in the Equation of the Densities, $\frac{\acute{p}-p}{p}=\frac{10}{4m}-\alpha$, or $\frac{\acute{p}-p}{p}=\frac{1}{115}-\alpha$, by putting 288 for m, as has been long known. It is easily

The Figure of the Spheroid being known, the Augmentation of Gravity from the Equator to the caſily ſeen from this Equation, that when α is determined, $\frac{p' - p}{p}$ will be ſo too, which was the thing propoſed to be proved. *Pole will be known alſo; and ſo* vice verſa.

XXIII. But from this Equation there follows a very ſingular Propoſition, and which, in ſome ſort, is contrary to the Sentiments of Sir *I. Newton* *, that *if by Obſervation it ſhall be diſcovered, that the Earth is flatter than according to the Spheroid of Sir* Iſaac, *that is, if the Diameter of the Equator exceeds the Axis by more than the* $\frac{1}{230}$ *Part, the Gravity will increaſe leſs from the Equator towards the Pole, than according to the Table which he has given for his Spheroid*; Prop. XX. of the 3d Book. And on the contrary, *if the Spheroid is not ſo flat, the Gravity will increaſe more from the Equator towards the Pole.*

XXIV. 'Tis thus that Sir *I. Newton* expreſſes himſelf about it, when he relates the Experiments made towards the South, concerning the Diminution of Gravity, which Experiments make it greater than his Theory requires †. He affirms, that the Earth is denſer towards the Centre than at the Superficies, and more depreſſed than his Spheroid requires. But by the foregoing Theory we may eaſily perceive, that if the Denſity of the Earth diminiſhes from the Centre towards the Superficies, the Dimunition of Gravity from the Pole towards the Equator will be greater than according to Sir *Iſaac*'s Table; but at the ſame time the Earth will be not ſo much depreſſed as his Spheroid requires, inſtead of being more ſo, as he affirms. Yet I would not by any means be underſtood to decide againſt Sir *Iſaac*'s Determination, becauſe I cannot be aſſured of his Meaning, when he tells us, that the Denſity of the Earth diminiſhes from the Centre towards the Circumference. He does not explain this, and perhaps inſtead of the Earth's being compoſed of parallel Beds or *Strata*, it's Parts may be conceived to be otherwiſe arranged and diſpoſed, ſo as that the Propoſition of Sir *Iſaac* ſhall be agreeable to the Truth.

XXV. As to Dr *Gregory*, who has attempted to comment upon this Paſſage of Sir *Iſaac*, I think I have demonſtrated, that he has committed a Paralogiſm. He ſays ‖ that if the Earth is denſer towards the Centre, or if (for Example) it has a Nucleus of greater Weight than the other Parts, the Diminution of Gravity from the Pole towards the Equator ſhall be greater than if the whole were of the ſame Denſity; and in this he is right. But he is in the wrong (I think) immediately to conclude from thence, that the Earth has a greater Flatneſs. Whence can he conclude this? It can be only from that Propoſition of Sir *Iſaac*

* *Princip. Math.* Ed. 3. p. 430. † *Et exceſſus longitudinis Penduli Pariſienſis ſupra longitudines Pendulorum iſochronorum in his latitudinibus obſervatas, ſunt paulo majores quam pro Tabula longitudinum Penduli ſuperius computata. Et propterea Terra aliquanto altior eſt ſub æquatore, quam pro ſuperiore, calculo, & denſior ad centrum quam in fodinis prope ſuperficiem.* ‖ *Elem. Aſtron.* Lib. 3. §. 8. Prop. 52. Schol.

which informs us, that Gravity is in a reciprocal Ratio of the Diſtances; becauſe he gave us the Propoſition but the Page before, as a Method for determining the Figure of the Earth. But we are not allowed to make uſe of this Propoſition in this Caſe, becauſe it has been ſhewn, Art. XVIII. that it can take Place only on the Suppoſition of a homogeneous Spheroid. Therefore, *&c.*

XXVI. It will not be very difficult, without any Regard had to the foregoing Theory, to find the Ratio of the Axes of a Spheroid, which we may ſuppoſe to have a Nucleus at the Centre, of greater Denſity than the reſt of the Planet; and hence we ſhall be eaſily aſſured of Dr *Gregory*'s Miſtake.

XXVII. Setting aſide all Attraction of the Parts of Matter, if the Action of Gravity is directed towards a Centre, and is in the reciprocal Ratio of the Squares of the Diſtances, the Ratio of the Axes of the Spheroid will then be that of 576 to 577: And the Gravity at the Pole, is greater than at the Equator by $\frac{1}{144}$th Part, or thereabouts. Which may be a Confirmation of what is here advanced, eſpecially to ſuch as will not be at the Pains of going through the foregoing Calculations. For we may conſider the Spheroid now mentioned, in which Gravity acts in a reciprocal Ratio of the Squares of the Diſtances, as compoſed of Matter of ſuch Rarity, in reſpect of that at the Centre, that the Gravity is produced only by the Attraction of the Centre or Nucleus.

XXVIII. In the foregoing Calculations, in order to find the Axes of our Spheroids, and to know whether their Figure makes a ſenſible Approach to that of the conical Ellipſis, we have had Recourſe to this Principle, that Gravity ought always to act in a Direction perpendicular to the Surface. Two Reaſons have prevailed with us to make uſe of this Principle rather than the other, which conſiſts in the Equilibrium of the Columns. The firſt is, becauſe the Calculations founded thereon are more ſimple. The ſecond is, that conſidering the ſtate of the actual Solidity of the Earth, it ſhould ſeem as if this Principle were the more indiſpenſably neceſſary. However, becauſe Sir *I. Newton*, and all the other Philoſophers, who have treated about the Figure of the Earth, have taken it, as it were, at it's firſt Formation, at which Time they ſuppoſe it to have been fluid; we ſhall here make the ſame Suppoſition, and we ſhall aſſume no other Ratio for that of the two Axes, than that of the Spheroid, which reſults from a Coincidence of theſe two Principles.

We ſhall begin by inquiring what is the entire Weight of any
Fig. 41. Column CN. To do this we muſt reſume the Expreſſion of the Attraction in any Point M of the Column CN; then multiply it by $r + \lambda r$, and by the Denſity $f r^p + g r^q$, and afterwards we muſt find

2 the

the Fluent. Thus we ſhall have $\frac{cf^2 e^{2+2p}}{\overline{1+p}\times\overline{3+p}} + \frac{cg^2 e^{2+2q}}{\overline{1+q}\times\overline{3+q}}$

$$+ \frac{2cfge^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}} + \frac{2cfge^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}} + \frac{4cf^2\alpha e^{2+2p}}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$$

$$+ \frac{4cg^2\alpha e^{2+2q}}{\overline{1+q}\times\overline{3+q}\times\overline{5+q}} + \frac{8cfg\alpha e^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+q}}$$

$$+ \frac{8cfg\alpha e^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}} + \frac{\overline{4+2p}\,cf^2\lambda e^{2+2p}}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$$

$$+ \frac{\overline{4+2q}\,cg^2\lambda e^{2+q}}{\overline{3+q}\times\overline{5+q}\times\overline{1+p}} + \frac{\overline{8+4p}\,cgf\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}}$$

$+ \frac{\overline{8+4q}\,cfg\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}}$ for the total Gravity of any Column CN, having Regard only to the Attraction.

XXIX. If in this Expreſſion we make $\lambda = o$, we ſhall have the Gravity of the Column at the Pole.

XXX. And if we make $\lambda = \alpha$, we ſhall have the Aggregate of the Attractions of the Column at the Equator.

XXXI. Now becauſe the Column CN is *in Æquilibrio* with the Column CB; it follows from thence, that if we ſubtract the Weight of the Column CB, from the Aggregate of the Attractions of the Column CN, the Reſidue muſt be equal to the Sum of the centrifugal Forces of the Column CN. Now to endue our Spheroids with this Property, we will reſume the Expreſſion of the centrifugal Force in E, which we found Art. XIV. which will give $\left(\frac{8cfe^{1+p}\lambda}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{1+p}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\frac{r}{e}$, for that Part of the centrifugal Force which acts according to CM, in any Place M, by expunging the Terms in which $\alpha\alpha$ would be found. This Value being multiplyed by $\dot{r}$, and by the Denſity, will give (when we have taken the Fluent) $\frac{8cf^2e^{2+2p}\lambda}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}}$

$$+\frac{8cfge^{2+p+q}\lambda}{\overline{2+p}\times\overline{3+q}\times\overline{5+q}}+\frac{8cfge^{2+p+q}\lambda}{\overline{2+q}\times\overline{3+p}\times\overline{5+p}}$$

$+\frac{8cg^2e^{2+2q}\lambda}{\overline{2+q}\times\overline{3+q}\times\overline{5+q}}$ for the Sum of the centrifugal Forces of the Column C N, still expunging those Terms in which either $\alpha\alpha$ or $\lambda\lambda$ are found.

Then making this Expression equal to $\frac{\overline{4+2p}\,cf^2e^{2+2p}\lambda}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$

$$+\frac{\overline{8+4p}\,cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}}+\frac{\overline{8+4q}\,cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}}$$

$+\frac{\overline{4+2q}\,cg^2e^{2+2q}\lambda}{\overline{1+q}\times\overline{3+q}\times\overline{5+q}}$, which is the Difference of the Weight of the Column at the Pole C B, from the Sum of the Attractions of the Column C N, we shall have the Equation $\frac{ppff}{\overline{1+p}\times\overline{2+p}\times\overline{3+p}\times\overline{5+p}}$

$$+\frac{2pqfg}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}\times\overline{2+q}}+\frac{2pqfg}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}\times\overline{2+p}}$$

$+\frac{qqgg}{\overline{1+q}\times\overline{2+q}\times\overline{3+q}\times\overline{5+q}}=o$, where we have put $e=1$, for the greater Simplicity of Calculation.

Determination of such Spheroids, as make the Principle of the Equilibrium of the Columns, and that of Gravity perpendicular to the Surface, to coincide with each other.

XXXII. This Equation informs us, that when out of all the infinite Varieties, which will be supplyed by the Equation of the Densities $D=fr^p+gr^q+hr^s$, *&c.* we shall have taken at Pleasure all the Coëfficients, and all the Exponents, one only excepted; if this last is such in respect of the others, that it may fulfil the Conditions of the foregoing Equation, the Spheroid, being supposed in a State of Fluidity, will be *in Æquilibrio*, because it will unite as well the Principle of a perpendicular Tendency to the Surface, as that of an Equipoise of the several Columns.

XXXIII. Before I conclude this Paper, I shall make a few Reflections on the Principles we have now made use of, for determining the Figure of a Spheroid revolving about it's Axe.

The first Principle which, after Mr *Huygens*, we have had Recourse to, and which consists in making Bodies gravitate perpendicularly

to

to the Surface, ſeems to me of abſolute Neceſſity. For if there were never ſo little Water upon the Surface of the Earth, it could not be at Reſt, if it had a Tendency any how inclined to the Surface.

The ſecond Principle made uſe of by Sir *I. Newton*, and which conſiſts in an Equilibrium of the Columns C E, C N, C P, could be thought neceſſary (I think) only for theſe two Reaſons: The firſt is that which is uſually aſſigned, that at the firſt Formation of the Earth, it was probably in a State of perfect Fluidity; in which caſe it muſt acquire ſuch a Figure, as will reſult from the Equilibrium of the Columns, and from the Gravitation acting perpendicularly to the Surface. Indeed though this Reaſon has a Degree of Plauſibility, yet there are many who think it to be of ſmall Force. Perhaps, ſay they, the Earth has never been in this fluid Condition.

The ſecond Reaſon, which I believe will have a greater Weight with every Body is this. Conſidering the Earth as it is at preſent, and without carrying our Thoughts ſo far back as to it's Formation, if the Ocean, which is now upon it's Surface, has any conſiderable Depth, and if it's Parts preſerve a Communication with each other, from Region to Region, by ſubterraneous Canals; it can only keep an Equilibrium by this Means, becauſe it's Superficies is the ſame as it would have, were the whole a Fluid.

XXXIV. This ſecond Reaſon has ſuggeſted a Reflexion to my Mind, concerning the Equipoiſe of the Columns now calculated, Art. XXXI. and XXXII. Let us firſt ſuppoſe, that the Earth is our fluid Spheroid, compoſed of Beds of different Denſities; and that afterwards this Fluid hardens into a Solid, ſo that the different Beds or *Strata*, of which it is made up, are of no other Uſe but to cauſe a Gravity by their Attractions. Then let us ſuppoſe, that the Seas and great Waters about the Earth have a Communication with each other, by means of ſome ſubterraneous Canals. As the Waters of the Sea, which unite with one another, are probably homogeneous, the foregoing Calculation, wherein we have conſidered the Spheroid as a Fluid, can no longer take Place, becauſe we have there ſuppoſed, that the Fluid contained in the Canal B C N is of a Denſity, that varies from the Center to the Circumference. From hence it ſeems to me, we muſt undertake the Computation of the Equilibrium of the Columns after another Manner, thus:

We muſt examine whether two Canals, as C N and B C, which are filled with a homogeneous Fluid, will be *in Æquilibrio*, all the other Parts of the Spheroid continuing as above.

XXXV. To do this, we will begin with finding the Gravity of any Column C N, ariſing from Attraction alone. Firſt, then, we muſt reſume the Expreſſion of the Attraction in any Point M, Art. VII. Fig. 41.
Then we muſt multiply it by $\dot{r} + \lambda \dot{r}$, which will give

$$\frac{2cfr^{\overline{1+p}}\dot{r}}{3+p} + \frac{\overline{8+4p}\,cf\lambda r^{\overline{1+p}}\dot{r}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha r^{\overline{1+p}}\dot{r}}{\overline{3+p}\times\overline{5+p}}$$

$+\dfrac{2cgr^{\overline{1+q}}\dot{r}}{3+q}$ *&c.* And taking the Fluent of this Quantity, we shall have $\dfrac{2cfe^{\overline{2+p}}}{\overline{3+p}\times\overline{2+p}} + \dfrac{4cf\lambda e^{\overline{2+p}}}{\overline{3+p}\times\overline{5+p}}$

$+\dfrac{8cf\alpha e^{\overline{2+p}}}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}} + \dfrac{2cge^{\overline{2+q}}}{\overline{3+q}\times\overline{2+q}}$, *&c.* for the Gravity of the whole Column C N.

XXXVI. If in this Value we make $\lambda = o$, we shall have the Gravity of the Column at the Pole.

XXXVII. And if we subtract the Gravity of the Column at the Pole from the whole Sum of the Attractions of the Column C N, we shall have $\dfrac{4cf\lambda e^{\overline{2+p}}}{\overline{3+p}\times\overline{5+p}} + \dfrac{4cge^{\overline{2+q}}\lambda}{\overline{3+q}\times\overline{5+q}}$, which must be equal to the Sum of the centrifugal Forces of the Column C N, in order that the Columns C B and C N may be *in Æquilibrio.*

But we shall find this really to obtain, if we resume the Quantity $\left(\dfrac{8cfe^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \dfrac{8cge^{\overline{1+q}}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\dfrac{r}{e}$, which expresses (Art. XXXI.) that Part of the centrifugal Force in M, which acts according to C M. Then multiplying this Expression by $\dot{r}$, and seeking the Fluent, we shall have $\dfrac{4cfe^{\overline{2+p}}\lambda}{\overline{3+p}\times\overline{5+p}} + \dfrac{4cge^{\overline{2+q}}\lambda}{\overline{3+q}\times\overline{5+q}}$ for the Aggregate of the centrifugal Forces of the Column C N. And this being the same as the foregoing, shews, that the Columns C B and C N are *in Æquilibrio*, supposing them to be homogeneous; nor are we here obliged, as in Art. XXXII. where we consider them as heterogeneous, to suppose the Coëfficients $f\ p$, &c. to have any certain Relation among one another.

XXXVIII. Perhaps it may be urged, that the foregoing Calculus agrees only to a Canal, as B C N, which passes through the Center; and that we ought to prove, in the same Manner, that the Water included in any other Canal *p q r* would obſerve an *Æquilibrium.* But it appears to me, that this Property may be derived from the former: For it follows from the foregoing Calculation, that if we might be allowed to make this Hypotheſis, *viz.* That independently of the Attraction of any Matter, the Gravity at any Diſtance C N from the Center (See *Fig.* 41.) would be proportional to $\frac{2 c f e^{\overline{1+p}}}{3+p}$ $+ \frac{\overline{2p-2}\; c f \lambda e^{\overline{1+p}}}{\overline{3+p} \times \overline{5+p}} + \frac{8 c f \alpha e^{\overline{1+p}}}{\overline{3+p} \times \overline{5+p}}$, &c. it is plain from thence, that a Maſs of the homogeneous Fluid, which ſhould turn about the Axis C B, would aſſume the ſame Form as that of our heterogeneous Fluids. But if this Spheroid ſhould then put on a fixed State, except only ſome Canal *p q r*, the Water in this Canal would be *in Æquilibrio*; for without this, the Spheroid could not be eſteemed as having arrived to it's fixed State. But this Suppoſiton comes to the ſame as that of our heterogeneous Spheroid, compoſed of elliptical Beds, in which ſhould be found a Canal *p q r* of a homogeneous Fluid; provided that the Space, which this Canal poſſeſſes in the Globe, be not of ſo large an Extent, as to change the Law of Attraction.

The only three Planets, in which we can be aſſured of Gravitation, and the centrifugal Force, are the Sun, Jupiter and the Earth. As to the Sun, the centrifugal Force is there ſo ſmall, in reſpect of it's Gravity, that his Poles muſt be very little depreſſed, ſo that we cannot be ſenſible of it by Obſervation. Then as to Jupiter, Obſervations make him ſomething leſs flat than according to Sir *I. Newton*; that is to ſay, than if he were compoſed of Matter of an uniform Denſity. Therefore by the foregoing Theory, he muſt be a little more denſe towards the Center, than at the Parts near the Superficies. We might make a thouſand Hypotheſes about the Manner of diſtributing the Inequality of Denſity, proceeding from the Center towards the Circumference, which would all agree with the Figure obſerved, and which are very eaſy to calculate by the Principles here laid down.

As to what concerns the Earth, I ſhall wait till we receive the Obſervations which muſt have been lately made in *Peru*; that by comparing thoſe with what Obſervations we have made under the arctick Circle, and with thoſe of Mr *Picart* in *France*, we may have the true Difference of the Earth's Diameters at the Equator and at the Poles. Then our Theory may be applied, to determine whether the Earth is more or leſs denſe at the central Parts than at the Surface, or whether it be every-where of an uniform Denſity, as it ought to be, if (without admitting very groſs Errors in the Obſervations) it may be concluded, that the Earth is really the Spheroid of Sir *I. Newton*; and this Caſe would be the ſimpleſt and the moſt natural of all.

P 2 I am

I am here obliged to acknowledge, that if the Observations we have made in the North may be relied upon, and if we must admit as incontestible as well the Measure of a Degree as the Length of the Pendulum, the foregoing Theory could not be reconciled to the *Phænomena*. For it follows from our Observations, that the Diameter of the Equator must exceed the Earth's Axis by more than $\frac{1}{230}$ Part: And that the Gravity at the Pole must be greater than that at the Equator by more than $\frac{1}{230}$ Part likewise; which will by no means agree with what we have deduced in Art. XXIII.

As to what concerns the Measure of Gravity in *Lapland*, as being not so liable to Error as the measuring a Degree; the Earth may be not quite so flat as Sir *Isaac*'s Spheroid requires. By the Table of the Length of the Pendulum, exhibited in the Treatise concerning the Figure of the Earth, published this Year by M. *de Maupertuis*, and by Art. XXII. of the present Discourse, the Earth may be more elevated at the Equator than at the Pole by the $\frac{1}{266}$ Part, or thereabouts. After the true Quantity of the Earth's Flatness shall be fully settled, if it should be found to have this Figure, I should be apt to think it is a little more dense at the Center than towards the Superficies. But if, on the contrary, we should be well ascertained, that the Earth is raised higher at the Equator than at the Pole, by above the $\frac{1}{230}$ Part; and if, for any sufficient Reason, we may something shorten the Length of the Pendulum that beats Seconds in the North; there would be some Grounds to allow, that the Earth is not so dense at the central Regions as at those near the Surface. But if it shall happen, that we can neither diminish the Length of the Pendulum, nor the Excess of the Equatorial Diameter above the Axe, I must then give up my Hypothesis.

Of a Curve called from it's Figure, a Cardioïde, by Joannes Castillioneus. No. 461. p. 778. *Aug.* &c. 1741.

Fig. 42, 43, 44.

X. The Diameter B A of the Semicircle B M A, touching the Circumference in the Point B, so as always to pass over the Point A, will generate the Curve in question. From the *Genesis* it appears,

1. That D A *a* perpendicular to A B is equal to double the Diameter.

2. That the Periphery of this Curve A D N *a* α N A will terminate in A.

We may call this Curve, from it's Figure, a *Cardioïde*.

Now through *a* and A draw *a* E, A Q, perpendicular to *a* A; and E N perpendicular to *a* E. It follows from the *Genesis*, that $AN = BA \pm A$, and (by the Similitude of the Triangles Q A N, M B A) $AQ = BM \pm MP$, and $NQ = MA \pm AP$.

This is the chief Property of our Curve, and there is another, which is no unpleasant one, that the right Line N N is always equal to double the Diameter, and is always bisected by the Circle in M.

Now let $BA = a$ $aE = x$, $EN = y$, then $QN = \mp y \pm 2a$, $AN = \sqrt{x^2 + y^2 \mp 4ay + 4a^2}$, and $MA = \mp a \pm \sqrt{x^2 + y^2}$

Fig. 35.

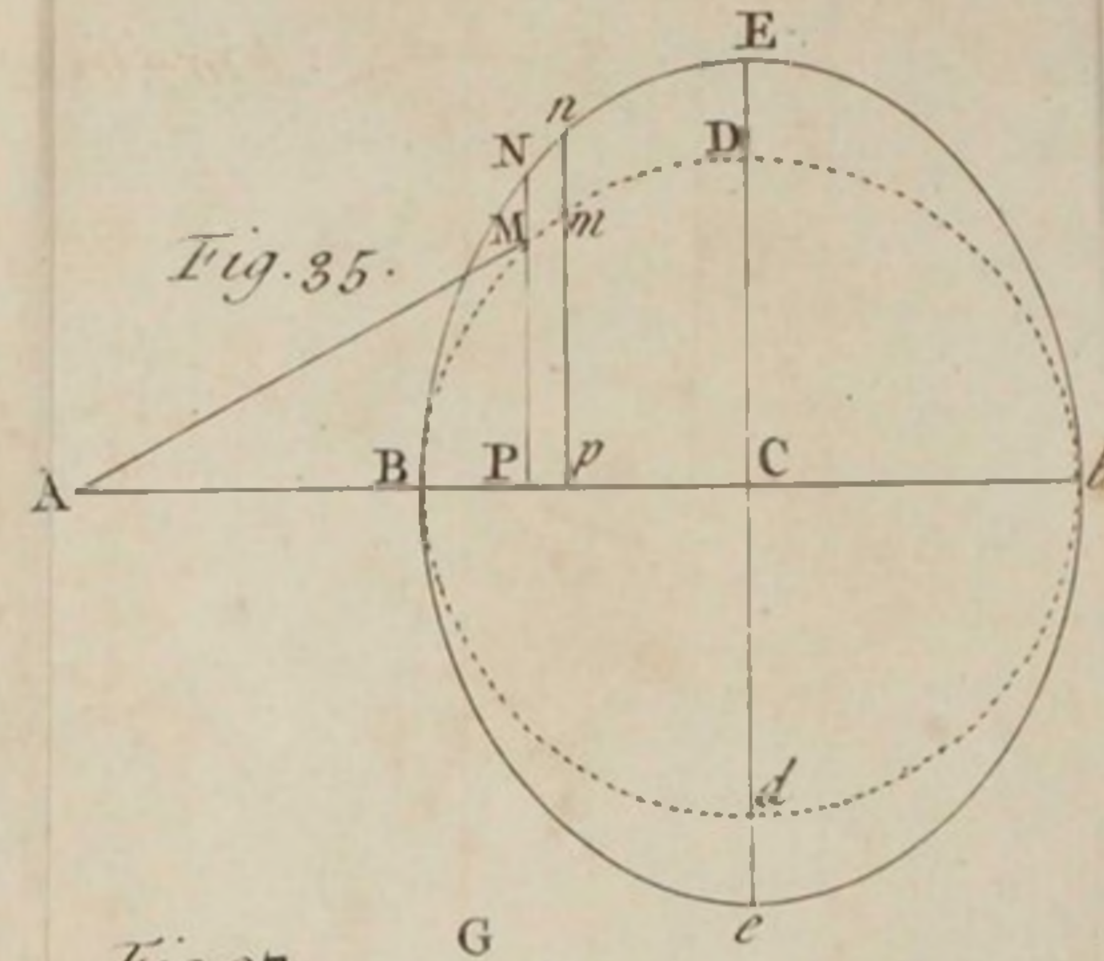


Fig. 36.

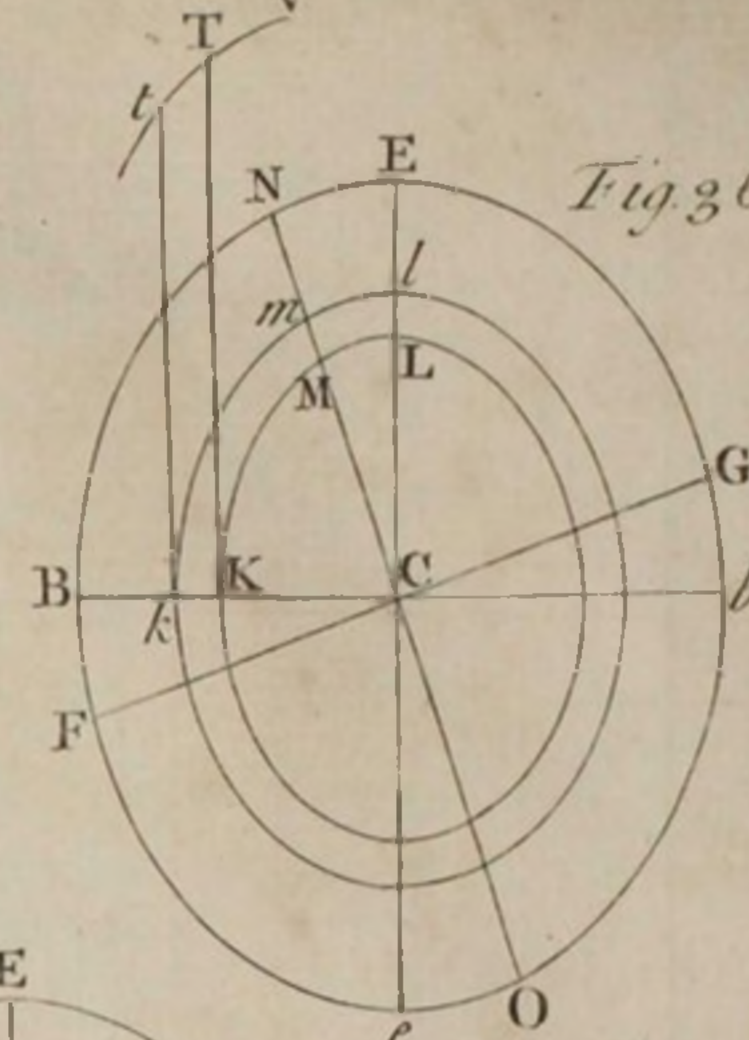


Fig. 37.

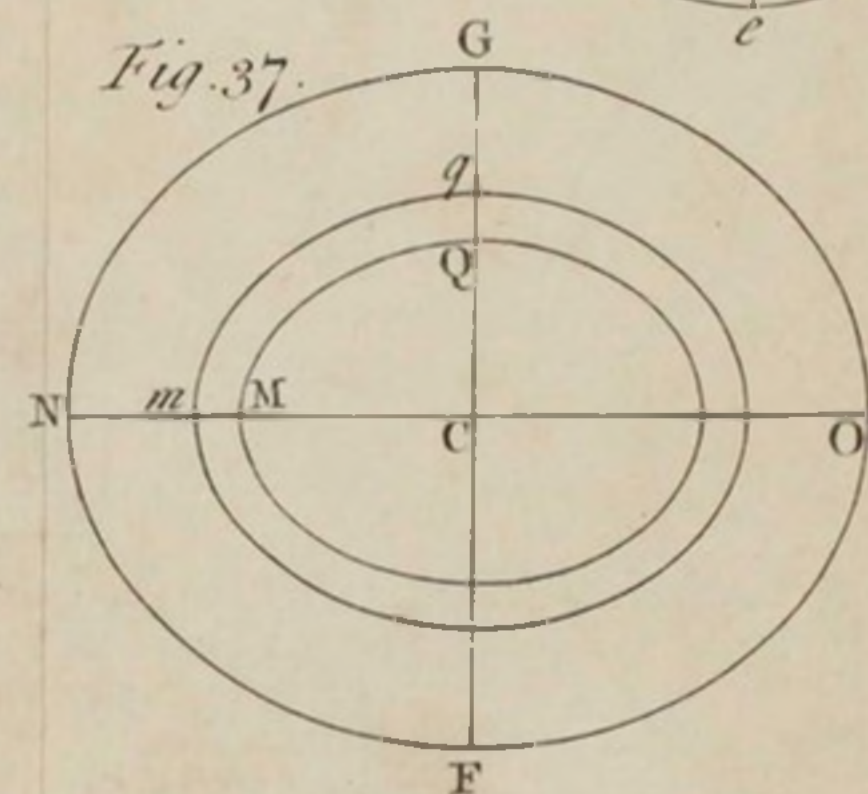


Fig. 39.

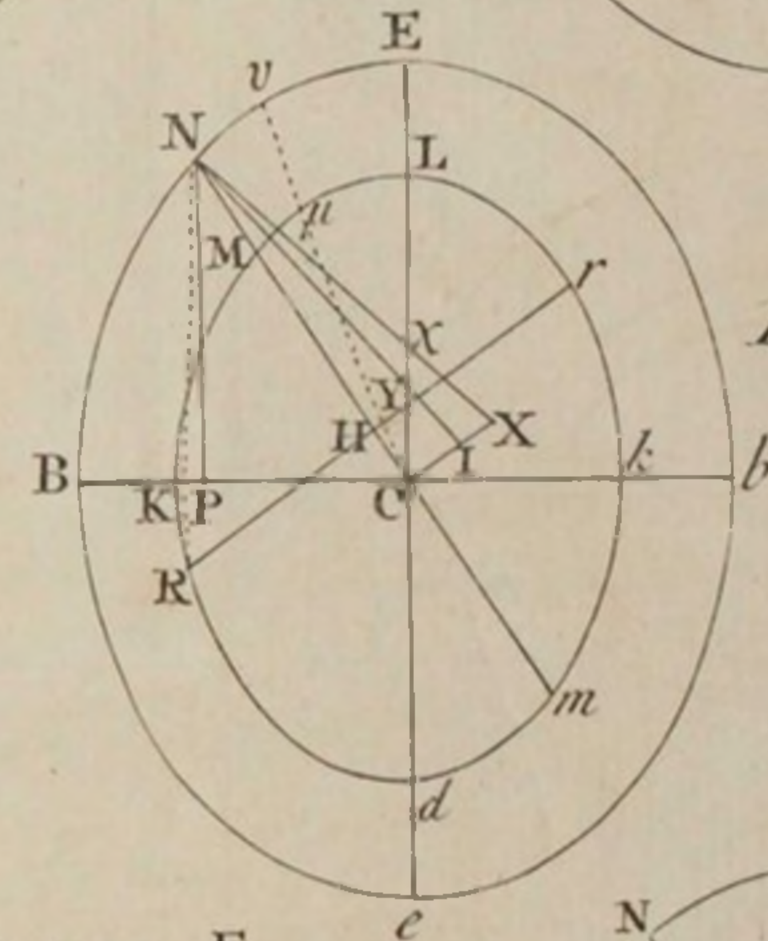


Fig. 38.

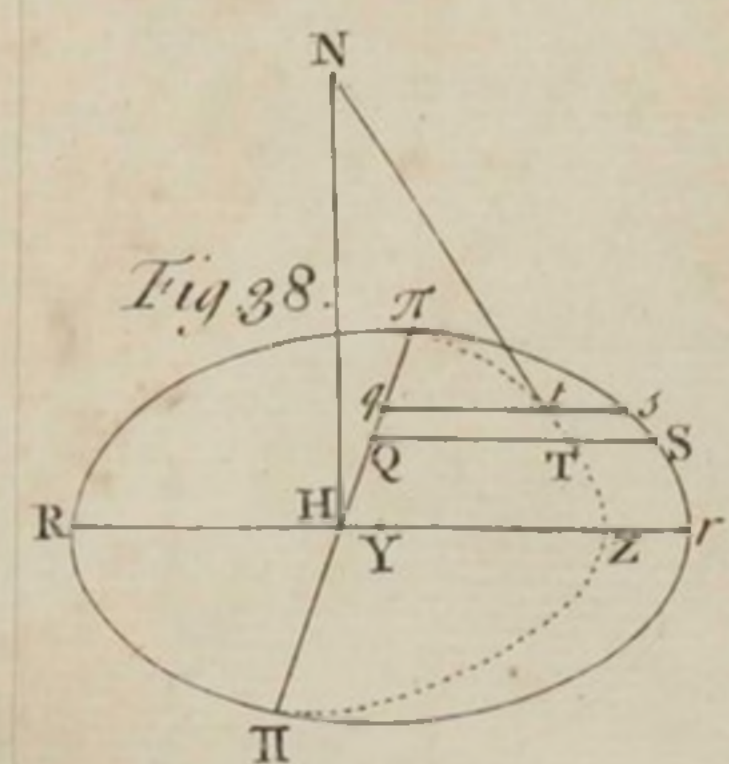


Fig. 40.

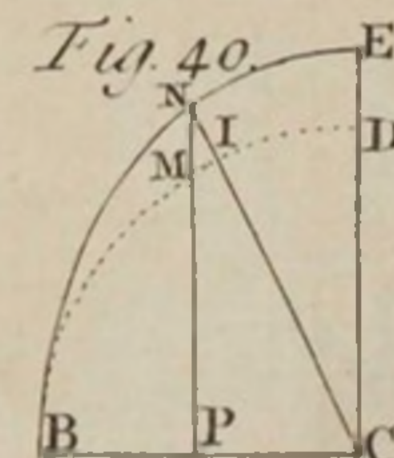
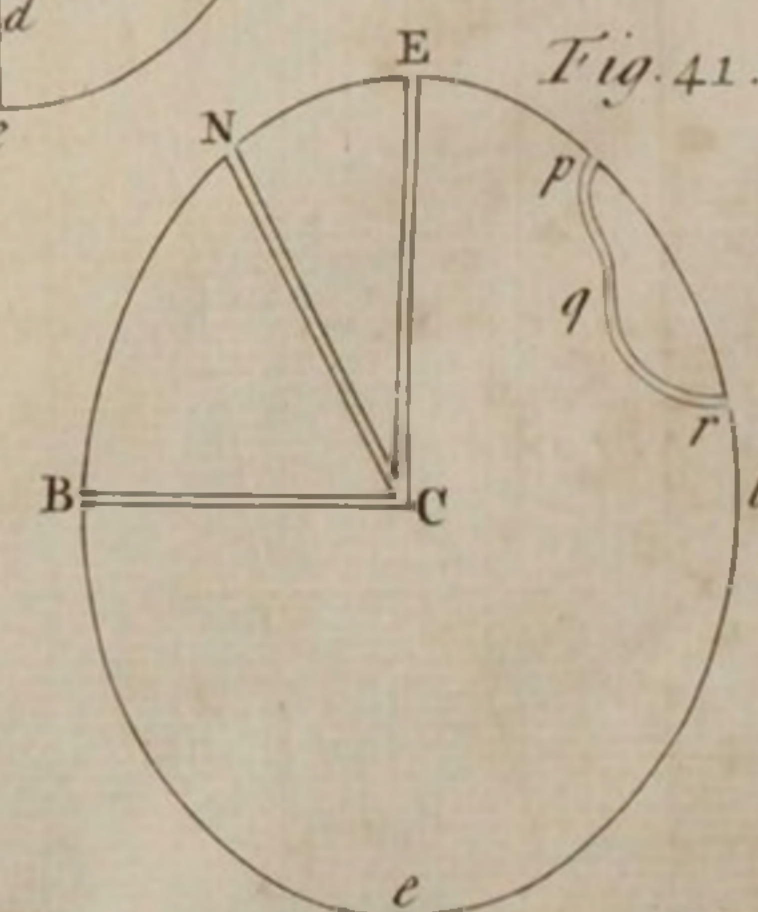


Fig. 41.



J. Mynde sc.

$\sqrt{x^2+y^2-4ay+4a^2}$, which 4 Lines being compared by Analogy, give the Equation to the Curve,

$$y^4-6ay^3\left.\begin{matrix}+\ \ 2x^2y^2-6ax^2y+x^4\\+12a^2y^2-8a^3y+3a^2x^2\end{matrix}\right\}=0.$$

The Subtangent of the Curve, according to the common Methods, is, $\dfrac{2y^4-9ay^3+2x^2y^2+12a^2y^2-3ax^2y-4ay^3}{6axy-2xy^2-3a^2x-2x^3}=\dfrac{x}{y}.$

But a more easy Method of drawing the Tangent may be deduced from the Generation of the Curve. Let M A N come into the nearest Place to the first m A n, take A R = A M, and A r = A N, and having joined M R, N r, draw through A the Right Line A T parallel to them, and through M m, N n, the Right Lines M T, $n\,t$. Now n A : A t : : $n\,r$ (or m R) : r N : : m R × M A : r N × A M : : m R × M A : M R × A N : : M A × A m : A N × A T, but in the last *Ratio* m A = M A, and T A perpendicular to M N, wherefore n A : A t : : $\overline{MA}^2$: A N × A T; now if from M be drawn through the Center of the Circle F, the Right Line M F, to be produced till it meets the Right Line produced also in G; that is, to the Periphery of the Circle, then M A^2 = T A × A G; wherefore n A : A T : : A G : A N; therefore let a Semicircle be described through G and N, which will cut the Right Line A T in t, from which the Right Line t N being drawn, will be a Tangent to the Curve, to which also the Right Line N G is perpendicular; from hence let M O be joined, to which draw a parallel from N touching the Curve.

Here let us observe by the way, that this Method of drawing Tangents agrees with most Curves.

Let A B be a Conchoide of *Nicomedes*: then, supposing the former Preparation, B P : P t : : B R (or $c\,r$) : R b : : $c\,r$ × C P : R b × C P (or r C × P R) : : $\overline{CP}^2$: T P × P R, whence the former Construction is deduced. Fig. 43.

Let a Right Line of a given Length C P B, touching the Right Line C D T perpendicular to D A, at the Point C, always pass over a given Point P in D A, and so generate the Curve A B. Fig. 44.

If you apply the former Preparation and Reasoning to this, you will have B P : P t : : b R ($r\,c$) : R B : : $c\,r$ × C P : R B × C P (B P × r C) : : C P^2 : B P × P T, as before.

But the Method *de maximis & minimis* gives the greatest Ordinate $=\dfrac{ca}{4}$, and it's Abscis $=\dfrac{a}{4}\sqrt{3}$. In the same Manner the greatest Abscis might be investigated; but this would be tedious; therefore seek it thus.

Because E N is a Tangent to the Curve, the Right Line M G drawn from the Point M thro' the Center F determines the Point G, from Fig. 42.

from which G N being drawn is perpendicular to E N, therefore also to A a, by the Hypothesis, but N Q = A V = M A + A P; therefore V P = M A; but B A : A M : : M A : A P; therefore B A : P V : : V P : P A; but P F = F V = $a - 2z$; and therefore $a : a - 2z :: a - 2z : z$. Hence is easily deduced $z = \frac{a}{4}$, E N = $\frac{7a}{4}$, A Q = $\frac{3a}{4}\sqrt{3}$. Here we must observe, that the same Point M, which affords in the Right Line N A M N the Point of the greater Ordinate, affords also the Point of the greater Abscis.

A Rule for finding the meridional Parts to any Spheroid, with the same Exactness as in a Sphere, by Colin Mac Laurin, *F. R. S. Communicated by* Andrew Mitchel, *Esq; F. R. S.* No. 461. p. 808. *Aug.* &c. 1741.

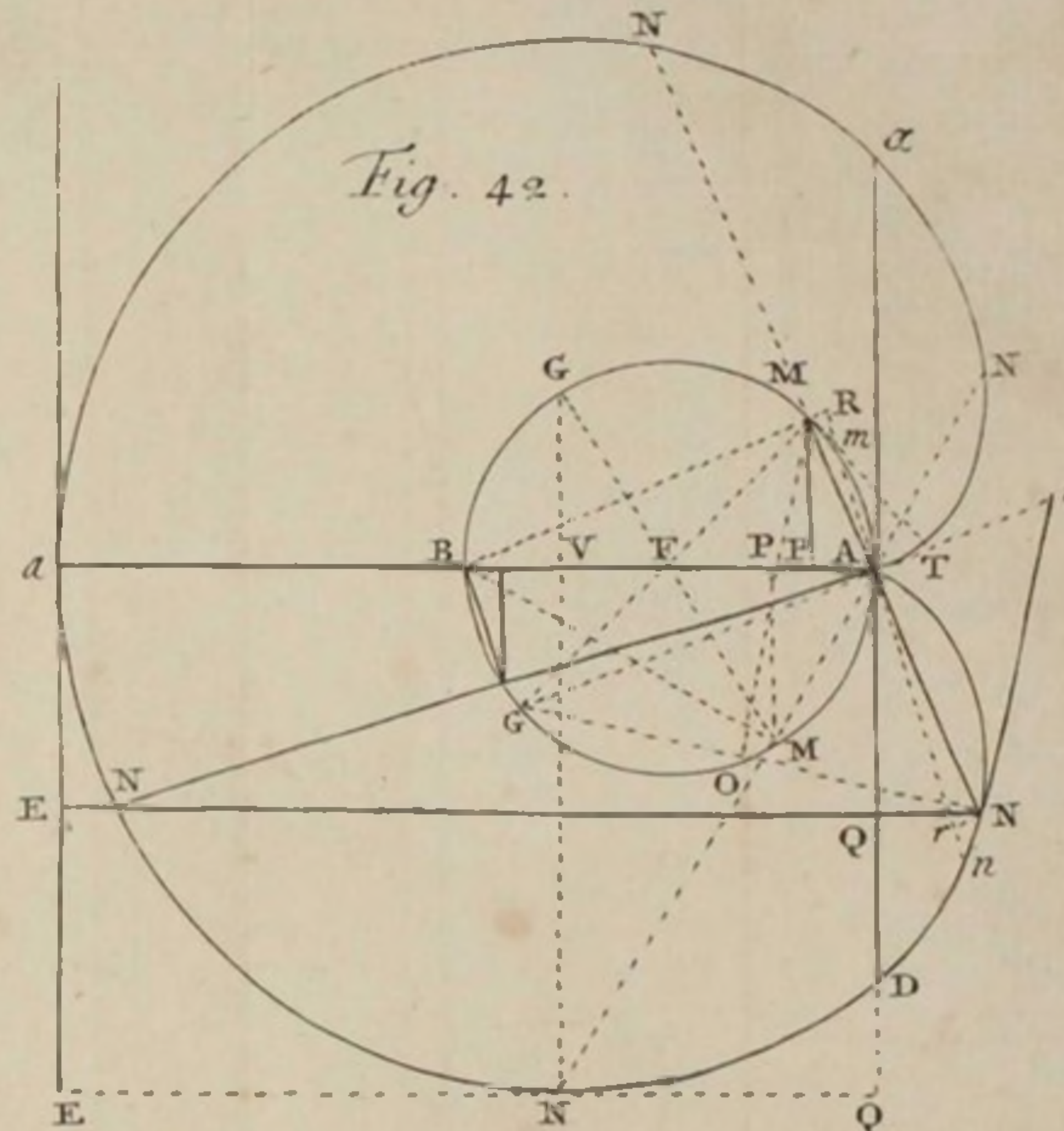
XI. It was demonstrated long ago, that in a Sphere the Nautical Meridian Line is a Scale of logarithmic Tangents of the half Complements of the Latitudes. The same may be computed with no less Exactness to any Spheroid by the following Rule.

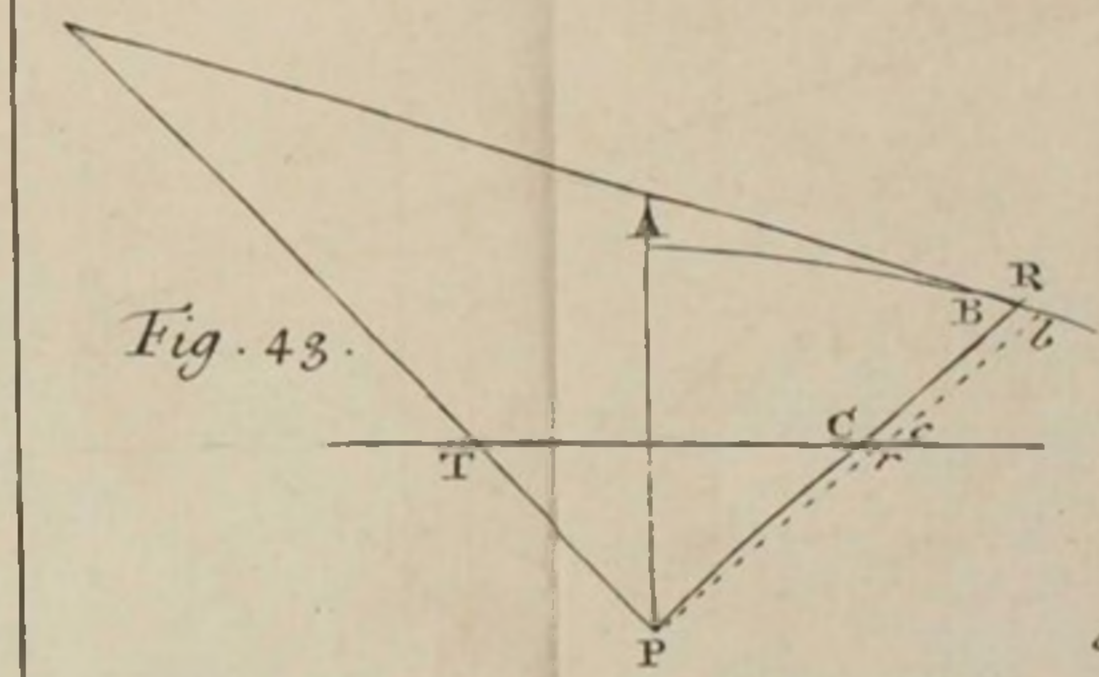
Let the Semidiameter of the Equator be to the Distance of the *Focus* of the generating Ellipse from the Center as m to 1. Let A represent the Latitude for which the meridional Parts are required, s the Sine of this Latitude, the *Radius* being Unit; find the Ark B, whose Sine is $\frac{s}{m}$; take the logarithmic Tangent of half the Complement of B from the common Tables; subtract this logarithmic Tangent from 10.000000, or the logarithmic Tangent of 45°; multiply the Remainder by $\frac{7915.7044678978}{m}$, *&c.* and the Product subtracted from the meridional Parts in the Sphere, computed in the usual manner for the Latitude A, will give the meridional Parts expressed in Minutes for the same Latitude in the Spheroid, provided it is oblate. When the Spheroid is oblong, the Difference of the meridional Parts in the Sphere and Spheroid for the same Latitude, is then determined by a circular Ark; but it is not necessary to describe this Case at present.

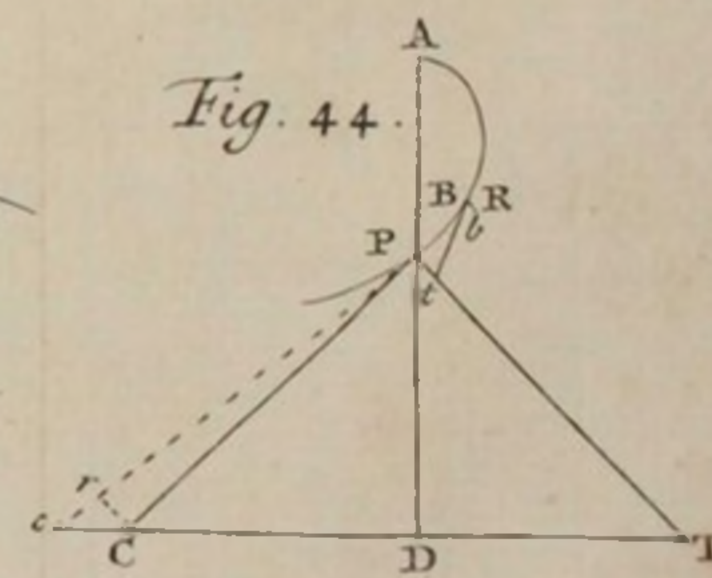
Example: If $m\,m$: 1 : : 1000 : 22. then the greatest Difference of the meridional Parts in the Sphere and Spheroid is 76.0929 Minutes: In other Cases it is found by multiplying the Remainder abovementioned by 1174.078.

CHAP.

Pla. VI. Vol. VIII. part 1st page 110.







CHAP. II.

OPTICKS.

I. IF two Lens's of equal focal Length be put together in the Form of a Telescope, and a Plane Speculum be placed before one of them, so that the Axis of the Telescope make any Angle with it's Surface, and a Ray of Light (the Line of whose Direction lies in a Plane perpendicular to that Surface, and passing through the Axis of the Telescope) fall on it, and be reflected from it, so as to pass thro' the Telescope; then the Line of it's last Direction, after passing the Telescope, will make an Angle with that of it's first Direction, before it's Incidence on the Speculum, very nearly equal to double the Angle made between the Axis of the Telescope, and the Surface of the Speculum.

A Proposition relating to the Combination of Transparent Lens's with Reflecting Planes. By J. Hadley, *Esq; V. Pr. R. S. Communicated* Jan. 9, 1734. No. 440. p. 185. *Jan.* &c. 1740.

LEMMA. Fig. 45.

Let the Line F G be the common Axis of the two Lens's I D and K E, of equal focal Lengths; to which let the Lines A D, D B and B E, be each equal; and let a Ray of Light, issuing from a Point in the Axis F, fall on the Lens I D at I, and be there refracted into the Line I G, cutting the Axis in G, and meeting the Lens K E in K, where let the Ray be again refracted into the Line K H, cutting the aforesaid Axis in H: The Angles I F D and K H E are very nearly equal.

Demonstration.

It is known from Dioptricks, that the Lines F I, I G, K H, and F G, are all in the same Plane; and by the Construction the Lines A D, D B, and B E are equal; and by *Prop.* 20 of *Huygens*'s *Dioptricks*, the Lines F A, F D, and F G are continually proportional; and consequently F A : A D : : F D : D G, and dividing, F A : A D : : F D — F A (= A D) : D G — A D (= B G.) Therefore A D : B G : : F D : D G. By the same *Proposition*, the Lines B G, E G, and H G are also continually proportional, and B E (= A D) : B G : : E H : E G. Hence it follows, that the Lines F D, D G, E H, E G, are Proportionals. But as F D is to D G, so is the Tangent of the Angle I G D or K G E to the Tangent of the Angle I F D; and as E H is to E G, so is the Tangent of the Angle K G E to the Tangent of the Angle K H E. The Tangent of the Angle K G E therefore has the same Proportion to the Tangents of each of the Angles I F D and K H E, and consequently those Angles are equal. *Q. E. D.*

N. B. In the Demonstration of the above-cited Prop. of *Huygens*, the Thickness of the Lens's are neglected, and the Distance of the Points I and K, from the Line F G, supposed very small; so that if either

either of thoſe are too great, there may ariſe a ſenſible Difference between the Angles I F D and K H E.

Fig. 46. Let D F and C G repreſent the two Lens's put together as before, having their common Axis in the Line E L, and B N a plane Speculum to which that Line is inclined in the Angle G H N, and let A B be a Ray of Light falling on the Speculum at B, as is before expreſſed, and let it be there reflected towards the Point C of the Lens C G, where it is refracted towards the Point D of the Lens D F, and there again refracted into the Line D E, cutting the Axis in E. The Angle A O P contained between this laſt Line D E, continued backwards, and the firſt Line of Incidence of the Ray A B, will be very nearly equal to double the Angle of Inclination of the Axis of the Lens's E L to the Plane of the Speculum B N; *i. e.* double the Angle G H N.

Demonſtration. Produce the Lines of Incidence and Reflection of the Ray A B and B C, till they meet the Axis of the two Lens's in I and L; and thro' the Point B draw B K perpendicular to the Plane of the Speculum, and cutting the ſame Axis in K, the Angles K B L and K B I are equal. The Angle K L B is the Difference of the Angles I K B and K B L; and the Angle H I B is the Sum of the Angles I K B and K B I (= K B L): Therefore the Angle I K B is equal to half the Sum of the Angles H I B and K L B. But by the foregoing *Lemma*, the Angles K L B and F E D are very nearly equal. Therefore the Angle I K B is nearly equal to half the Sum of the Angles H I B, and F E D; that is to half the Angle P O B, and it's Complement B H I or G H N is nearly equal to half the Angle A O P the Complement of P O B to a Semicircle. *Q. E. D.*

If the firſt Incidence of the Ray be ſuppoſed to be in the Line E D, it will proceed in the ſame Track as before, but with the contrary Directions; ſo that the Angle E O B made between the firſt incident Ray and the laſt reflected, will ſtill be equal to the Double of G H N, as before.

It is evident that on this Principle an Inſtrument might be conſtructed, the Effects of which would in a great Meaſure reſemble thoſe of that before mentioned *: But it would be liable to the Errors ariſing both from the ſpherical Figure of the Lens's, and alſo the different Refrangibility of the Rays of Light, when the Object is ſeen at a Diſtance from the Axis of the Teleſcope; altho' thoſe Errors, by a proper Diſpoſition of the Parts of the Inſtrument, may be reduced to a very ſmall Quantity. However, for this Reaſon, and alſo becauſe the Inſtrument ſeemed to me to be attended with greater Inconveniencies, both in it's Conſtruction and Uſe, than the other, I have not thought it neceſſary to give any more particular Deſcription of it.

* See Vol. VI. p. 139.

II. The Imperfections of Telescopes are attributed to two Causes; The Unfitness of the Spherical Figure to which the Glasses are usually ground, and the different Refrangibility of the Rays of Light.

A new Method of improving and perfecting Catadioptrical Telescopes, by forming the Speculums of Glass instead of Metal. By Mr Caleb Smith. No. 456. p. 326. *Jan.* &c. 1740.

The first of these Defects only, was known to the Writers of Dioptrics, before Sir *I. Newton*; for which Reason (as he informs us himself *, they 'imagined, that Optical Instruments might be brought 'to any Degree of Perfection, provided they were able to communicate 'to the Glasses, in grinding, what Geometrical Figure they pleased; 'to which Purpose various Mechanical Contrivances were thought of, 'whereby Glasses might be ground into Hyperbolical, or even Parabolical, Figures; yet nobody succeeded in the exact Description of 'such Figures; and had their Success been answerable to their 'Wishes, yet their Labour would have been lost, for the Perfection 'of Telescopes is limited, not so much for want of Glasses truly figured, 'according to the Prescriptions of Optic Authors, (which all Men 'have hitherto imagined) as because that Light itself is an heterogeneous 'Mixture of differently refrangible Rays; so that were a Glass so 'exactly figured as to collect any one sort of Rays into one Point, 'it could not collect those also into the same Point, which having 'the same Incidence upon the same Medium, are apt to suffer a 'different Refraction †.' And again,—— 'The different Refrangibility 'of different Rays, is an obstruction to the perfecting of Optical Instruments, either by Spherical or other Figures; and unless the Errors 'thence arising, can be corrected, all the Labour spent in correcting the 'rest will be to no purpose ‖.

Now, for this principal and last-mentioned Defect, no one, that we know of, has proposed any Remedy; apprehending, perhaps, the Difficulty of attaining such to be insuperable; inasmuch as the great Author of this Discovery, himself, had not shewed us any Method whereby to correct those Errors which arise from this Inequality of Refraction; but rather discouraged any such Attempts, by declaring, 'that on this Account he laid aside his Glass-works **, and looked upon 'the Improvement of Telescopes, of given Lengths, by Refraction, 'as desperate ††.'

However, as it has been proved by inconteſtable Experiments, that this Dissipation of the Rays of Light, from whatever Cause it proceeds, in passing out of one Medium into another, is not accidental and irregular; but that every sort of homogeneal Rays whether more or less refrangible, considered apart, are refracted according to some constant uniform and certain Law; and as the removal of so great an Impediment as this of unequal Refraction in the Rays of Light, is of great Importance to the Science of Dioptrics, and absolutely necessary to it's further Advancement; we have thought it worthy

* *Opt. Lect.* 1. 2. † *Phil. Transf.* No. 80. ‖ *Princip. Schol. ad fin.* Lib. 1.
** *Phil. Transf.* No. 80. †† *Opt. Ed.* 2. *p.* 91.

of a careful Examination, whether, in some Cases at least, it might not be possible for contrary Refractions so to correct each other's Inequalities, as to make their Difference regular; and if this could be conveniently effected, Sir *I. Newton* has acknowledged, 'there would 'be no farther Difficulty *.'

Now, upon a due Consideration of this subject, we have found it possible, by proper Methods and Expedients, to rectify those Errors which proceed from the different Degrees of Refrangibility in different Rays, passing from one Medium into another; admitting only this well-known and established Principle, upon which we ground our Reasoning, *viz.* 'That the Sines of Refraction of Rays differently 'refrangible, are one to another in a given Proportion, when their 'Sines of Incidence are equal †.' And our present Design is, to shew what Advantage this will yield towards improving and perfecting Catadioptrical Telescopes, by making the Speculums of Glass, instead of Metal, in the following Manner:

Fig. 47. Let A B C D E F represent the Section of a concavo-convex Speculum, whose two Surfaces are Segments of unequal Spheres; call the Radius of the Sphere, to which the concave Side is ground, a; and the Radius of the convex Surface, which must be quicksilvered over, e; let B R be the Axis of the Speculum, or a Line perpendicular to both the Surfaces; and therein let P be the principal Focus, or Point where parallel Rays of the most refrangible Kind are collected, by this Speculum; and Q the Focus, or point of Concourse, of such Rays as are least refrangible; to wit, after they have suffered two Refractions, at entering into, and passing out of, the concave Surface D E F, and also one Reflection from the convex Surface A B C: If the Radius of Concavity be greater than the Radius of Convexity, as we will in the first Place suppose, then P will fall nearer the Vertex of the Speculum than the Point Q; and the Interval Q P will be the greatest Aberration, or Error, occasioned by the Separation, or unequal Refraction, of the greatest and least refrangible Rays, after their Emergence from the concave Surface F E D. Call the common Sine of Incidence, n; the Sine of Refraction of the least refrangible Rays out of a dense Medium into a rarer, m; and, of the most refrangible, μ; then, according to the known and received Laws of Refraction and Reflection, the Focal Distance of the most refrangible Rays, from the Vertex of the Speculum, (neglecting it's Thickness, as of little or no Moment in the present Case) will be found $= \frac{n a e}{(a-e)2\mu + 2ne} =$ P B. And the Quantity of the greatest Aberration, occasioned by the different Refrangibility of the most and least refrangible Rays, P Q, will be to the focal Distance just mentioned, P B, as $(a-e(\times(\mu-m)$ to $(a-e)m+en$; which

* *Phil. Trans.* No. 88. † *Opt. Ed.* 2. p. 66.

Quantity

Quantity, or Error, thus obtained, (to abbreviate the Calculation) call ϵ; and now let it be required to form a Lens, if poffible, which placed at fome given Point in the Axis between the Focus of the moft refrangible Rays P, and the Vertex of the Speculum (as H), fhall refract not only the Rays of the moft refrangible Kind tending to the Point P, but alfo the Rays of the leaft refrangible Kind tending to Q, in fuch a Manner, that both Sorts fhall concur, after fuch Refraction, in fome other Point of the Axis R; let H P the given Diftance of the Point in the Axis H, from the Focal Point P, be called d; and then if the Point H has been affumed, fo that the faid given Quantity, or Diftance, d, is greater than $\frac{(\mu - n)\epsilon}{\mu - m}$, but lefs than $\frac{m\epsilon}{\mu - m}$, I fay the refracting Superficies G H I, that fhall perform what was required, will be part of a concave Sphere, whofe Radius is $= \frac{(dd + d\epsilon) \times (\mu - m)}{m\epsilon - (\mu - m)d}$, and HR, the Diftance of the given Point H, from R, the Point to which all the Rays will tend, after Refraction at the faid concave Surface, (whofe Radius being found, as above, we call v) will be $= \frac{\mu d v}{(d + v)n - \mu d}$. Laftly, upon the Point R thus obtained, as a Centre, with an Interval a little lefs than HR, defcribe the circumference KLM, and the Figure GHIMLK will denote the Section of a double concave Lens, which, placed at the given Point in the Axis H, (taken neverthelefs within the Limits above-mentioned) will collect all Sorts of Rays proceeding from the Speculum, into one and the fame Focus, or Point of the Axis, R, as was required; for the Surface G H I, which firft receives thofe Rays, will refract the moft refrangible Sort converging to the Point P, and alfo the leaft refrangible converging towards Q, fo that both Sorts, after fuch Refraction, will concur in the Point R; but the Rays tending to R, 'tis manifeft, will fuffer no Refraction at their Emergence from the Superficies K L M, becaufe R is the Centre thereof, by Conftruction; which Point, R, where a perfect Image of an Object infinitely diftant will be formed, we call the Focus of the Telefcope, to diftinguifh it from the Point, P, which we have before called the Focus of the Speculum.

In this manner a Lens, (or inftead thereof a triangular Prifm with two of it's Sides ground concave, and the third plain, if that be found as practicable) may be formed and fituated, fo as to correct the Errors of the Speculum arifing from the different Refrangibility of the Rays of Light. But, in order to render this kind of Telefcopes abfolutely perfect in their Conftruction, the Errors alfo that refult from the fpherical Figure muft be rectified; and with regard to this, we affert, that it is poffible to affume a Point in the Axis, between the

Q 2 Focus

Focus of the Speculum and it's Vertex, (as we have taken the Point H, in the following Example*) at which, if a refracting Superficies, or Lens, be conftituted, according to the Method already delivered, it will not only correct the Errors occafioned by the unequal Refraction of the Rays of Light, but alfo rectify fuch as proceed from the fpherical Figure of this Speculum, to a much greater Degree of Exactnefs than is requifite for any Phyfical Purpofe (meaning always the Errors of thofe Rays which refpect the Axis). Now to find or determine this Point, affords a Problem not eafy to be folved; and we recommend it, as worthy of the Confideration of Geometricians.

Seeing therefore it is poffible, and we believe alfo practicable, to remedy the Imperfections of this kind of Speculums, (from whatfoever Caufe they arife) by the Method we have here propofed; it feems to follow, that Catadioptrical Telefcopes may be carried, by this means, to as great a Degree of Perfection, as they are capable of receiving; provided fpherical Figures can be truly communicated, with an exquifite Polifh, to Glaffes of a large Aperture, and a Foil of Quickfilver made alfo to retain that Figure accurately, and without any Inequality; for the Object-Glafs or Speculum being rendered perfect, fo as that all forts of Rays, proceeding from one lucid Point in it's Axis, fhall be collected by means of the Lens exactly in another Point, it's Aperture may then be extended to it's furtheft Limits; and that is, till the whole Pupil of the Eye (or the whole Portion of the Eye-Glafs to be ufed, when that becomes neceffarily lefs than the Pupil) be filled with Rays proceeding from the Speculum, and flowing from one Point of the Object, but no farther; becaufe this is a Limitation made by Nature in the Structure of the Eye itfelf: And in Telefcopes whofe Conftruction is fuch as we have now defcribed, the largeft Aperture of the Speculum that can ever be of Ufe, will be to the Diameter of the Pupil of the Eye, very nearly, in a *Ratio* compounded of the *Ratio's* of the Focal Length of the Speculum to the Diftance of that Focus from the Lens, and of the Diftance of the Lens from the Focus of the Telefcope, to Unity: That is, of B F to F H, and of R H to 1; which Proportion holds, whatever be the Charge or the Power of Magnifying.

But if Inquiry be made as to the Charge moft proper and convenient that will be determined beft by Experience, in thefe, as well as in all other forts of Telefcopes: However, on Suppofition that one of a given Length has it's Aperture and Charge rightly ordered and proportioned, the Rule for preferving the fame Degree of Brightnefs and Diftinctnefs, in all others of a like Conftruction, will be, to make the Apertures, and magnifying Powers, directly as the Focal Lengths of the Speculums; which fhews the vaft Advantage and Perfection of thefe Telefcopes, above the common reflecting ones; where, according

* *See* Fig. 48.

to Sir *I. Newton*'s Rule, the Apertures, and Powers of Magnifying, must be as the Biquadrate Roots of the Cubes of their Lengths*.

It is likewise a considerable Advantage in this Construction, that the Reflection from the concave Side of the Speculum will do no sensible Prejudice; because the Image of any Object made thereby, is removed to so vast a Distance from the principal Image, formed by the convex Surface, as to create no manner of Confusion or Disturbance in the Vision; which necessarily happens, in some Degree, from the Vicinity of those Images, when the Glass is ground concave on one Side, and as much convex on the other; according to the Method propounded by Sir *I. Newton*, in his *Opticks*.

It may be imagined, perhaps, at first View, that (if our Reasoning is just) the Errors of refracting Telescopes, occasioned by the different Refrangibility of Light, may be corrected by a like Artifice: But the Aberration of the Rays from the principal Focus is there so great, and bears so considerable a Proportion to the Focal Length of the Telescope, that the Error cannot be rectified by the Interposition of any Lens, until the Rays are, by a contrary Refraction, collected again at an infinite Distance, which renders this Expedient quite useless; however there is no need to despair of accomplishing even this, by other Methods: And, by the way, we may observe, if it were worth while to seek a Remedy for the Errors occasioned by the spherical Figure of the Object-Glass only, in Dioptrical Telescopes; that might be obtained by the proper Application of a suitable Lens, between the Focus and the Vertex of the Object-Glass; which is much more easy and practicable, than the grinding of Glasses to Hyperbolical or Elliptical Figures.

For a further Illustration of what is gone before, it may be proper to exhibit the several Parts and Proportions of a Telescope in Numbers computed according to the Theorems already delivered; and in Practice we judge it will be most convenient, that the *Radii* of the Spheres to which the concave and convex Sides of the Speculum are ground, be nearly in the Ratio of 6 to 5; as in the following Example; where A B C D E F, represents the great Speculum of Glass, ground concave on one Side, and convex on the other; quicksilvered over the convex Side, and of an equal Thickness all round it's Circumference. Fig. 48.

The *Radius* of Concavity $= a = 48$ Inches.
The *Radius* of Convexity $= e = 40$ Inches.

Then putting n, the Sine of Incidence $= 100$; m, the Sine of Refraction of the least refrangible Rays, out of Glass into Air, $= 154$; and μ, the Sine of Refraction of the most refrangible Rays, $= 156$; as Sir *I. Newton* found them by Experiments; we shall have,

* See his Opticks, Ed. 2. p. 97.

P B, the Focal Length of the Speculum with regard to the most refrangible Rays = 18.2926 +, which will be somewhat increased by the Thickness of the Glass, when that is considerable.

P Q, the greatest Aberration of the Rays, occasioned by their different Degrees of Refrangibility, = .05594 +, which Quantity, in Practice, should be a very little augmented, rather than otherwise; wherefore we put it here = .056 = ϵ.

The *Radius* of the concave Surface of the Lens, turned towards the Speculum, *viz.* of G H I, = υ = 2.8 Inches.

The *Radius* of the concave Surface of the Lens, turned from the Speculum, *viz.* of K L M, = 6.7 Inches.

The Thickness of the Lens at the Vertex L H = $\frac{1}{10}$ of an Inch.

The Aperture of the Lens must be about $\frac{1}{6}$ of the Aperture of the Speculum.

H P, the Distance of the Focal Point P from the Point H, where the abovesaid Lens is to be placed, so as to correct the Errors arising from the different Refrangibility of the Rays, and also the Errors of the spherical Figure, = 2 . $\frac{24}{73}$ Inches.

H R, the Distance of H the Vertex of the Lens from R the Focus of the Telescope, = 6.8 Inches.

And if we suppose the Diameter of the Pupil of the Eye to be $\frac{1}{5}$ of an Inch, (though it has not one certain Measure) then the Diameter of the greatest Aperture of the Speculum, that can ever be of Use, will be 6$\frac{2}{3}$ Inches, nearly.

The small plano-convex Eye-Glass O must always have one common Focus with the Telescope, to wit, the Point R translated to *r*, by Reflection from the Base of the Prism N; for which Reason it must retain, at all times, an equal and invariable Distance from the Lens G H I K L M; which Distance will be the Focal Length of the said Eye-Glass more H R (= H N + N *r*) the Distance of the Lens from the Focus of the Telescope R.

The Form and Position of the Prism N, and the Contrivance of the other Parts necessary, will be much the same as in the *Newtonian* Telescope.

If the Focal Length of the Eye-Glass be $\frac{1}{4}$ of an Inch, the Telescope will magnify about 200 times.

This Telescope may be contrived in the *Gregorian* way, by using, instead of a Lens and Prism a small Speculum spherically concave on one side, and convex on the other; but we think it not worth while to attempt this Construction, as an Investigation of the Proportion between the two Surfaces necessarily, in this small Speculum, to unite the Rays proceeding from the great one, into one Point, would be intricate, and the Practice also very difficult; by reason that a little Inaccuracy will, in this Case, occasion Errors much more considerable than a like Imperfection in the refracting Lens.

We

We have hitherto supposed the *Radius* of the Concavity greater than that of the Convexity; as being most convenient and useful, on several Accounts, in forming this kind of Telescopes; however, it may be proper to remark, that the same Method may be used for correcting the Errors of the Speculum, when the *Radius* of it's Concavity is less than that of the Convexity; only the refracting Superficies of the Lens placed between it's Vertex and Focus, will be convex, and not concave, as in the former Case. And there is another thing worthy of Remark, that the Focus, or Point (P), where the most refrangible Rays are collected, will fall farther from the Vertex of this Speculum, than the Focus of the least refrangible (Q); a Circumstance which never happens by Refraction alone, in Glasses of any Figure whatsoever, or howsoever they be disposed.

Now all things being put as before, and making $HQ = d$, I say the convex Superficies G H I of a Lens placed at H, that shall correct the Errors arising from the different Refrangibility of Rays, in this kind of Speculum, will be part of a Sphere, whose *Radius* is $= \frac{(\mu - m) \times (dd + d\varepsilon)}{(\mu - m)\, d + n\varepsilon} = v$. And H R, the Distance of the Point R, where the Rays of all sorts will unite, after this Refraction, from H the given Point in the Axis, will be $= \frac{\mu d v}{(\mu - n)\, d + n v}$; which Point R being taken as a Centre, describe thereon the Arch K L M, and the Figure G H I M L K will represent the Section of a Meniscus-Glass, or Lens, which, placed at the Point H, assumed between the Vertex and Focus of the Speculum, will collect all sorts of Rays proceeding therefrom into one and the same Point, or Focus, R. We might also shew, how this Error may be rectified by one or more Glasses, placed in the Axis, at a Distance farther from the Vertex than the Focal Point P; but the former Speculum is so much preferable to this, for the constructing of Telescopes, that we think it not worth while to prosecute this Matter farther. To conclude this Essay; Fig. 49.

Whoever shall think fit to put the Method here proposed in Execution, we dare venture (from a Trial that has been made) to assure him of Success; provided the same Diligence, Care, and Accuracy, be applied, in choosing, figuring, polishing, and foiling, the Glass, that has of late been employed for the forming Speculums of Metal; and let none be discouraged, though the first and second Attempt should fail; for that must be expected, if the ordinary way of grinding and polishing be used: Greater Exactness is here required, than is usually thought sufficient for the Object-Glasses of refracting Telescopes: Let it be also considered how many Essays, for a long Term of Years, were made by Mr *Gregory*, Sir *I. Newton*, and others, to reduce their Constructions of the reflecting Telescope into Practice without answering,

in

in any tolerable Degree, what their Theories promiſed: The Workmen they employed were chiefly Optical Inſtrument-makers, and had it been left to ſuch Perſons only to perform by themſelves, we have reaſon to think, that it would have been pronounced impracticable to this Day, to make a reflecting Teleſcope that ſhould equal or excel refracting ones of ten times it Length; though we now ſee, that moſt of theſe Artificers are capable of making them to ſuch a Degree of Perfection as was formerly deſpaired of. *April* 5. 1739.

A Catoptric Microſcope. By Robert Barker, *M. D. F. R. S*, No. 442. p. 259. *July &c.* 1736.

III. Though *Microſcopes*, compoſed of Refracting Glaſſes only, have been vaſtly improved, as to their Effects of magnifying; yet they have been attended with ſuch great Inconveniences, that their Application to many Arts, in which they might be very convenient, is not ſo common as might be expected, and Mankind have reaped but a ſmall Part of the Advantage obtainable from ſo ſurprizing and uſeful an Inſtrument.

Among the Inconveniences mentioned, theſe are the moſt conſiderable:

1. That in order to magnify greatly, it is neceſſary the Object-Glaſs be a Portion of a very minute Sphere, whoſe *Focus* being very ſhort, the Object muſt be brought exceeding near; it will therefore be ſhaded by the *Microſcope*, and not viſible by any other Light than what paſſes through itſelf; in this Caſe therefore, opake Objects will not be ſeen at all.

2. Objects illuminated this way, may be rather ſaid to eclipſe the Light, than to be truly ſeen, little more being exactly repreſented to the Eye. than the Out-line; the Depreſſions and Elevations within the Out-line; appearing like ſo many Lights and Shades, according to their different Degree of Thickneſs or Tranſparency; though the contrary happens in ordinary Viſion, in which the Lights and Shades are produced by the different Expoſure of the Surface of the Body to the incident Light.

3. Small Parts of large Objects cannot eaſily be applied to the *Microſcope*, without being divided from their Wholes, which in the Caſe of Vivi-ſection defeats the Experiment, the Part dying, and no more Motion being obſerved therein.

4. The *Focus* in the *Dioptrick Microſcope* being ſo very ſhort, is exceeding nice, the leaſt Deviation from it rendring Viſion turbid; therefore a very ſmall Part of an Irregular Object can be ſeen diſtinctly this way.

To remedy theſe Defects I have contrived a Microſcope on the Model of the *Newtonian* Teleſcope, in which I have been greatly aſſiſted by that excellent Workman, Mr *Scarlet*, *jun.* I ſhall ſay nothing of the Effects of this Inſtrument, excepting that it magnifies from the Diſtance of 9 to 24 Inches.

Pl a. VII. Vol. VIII. part I.^d page 120.

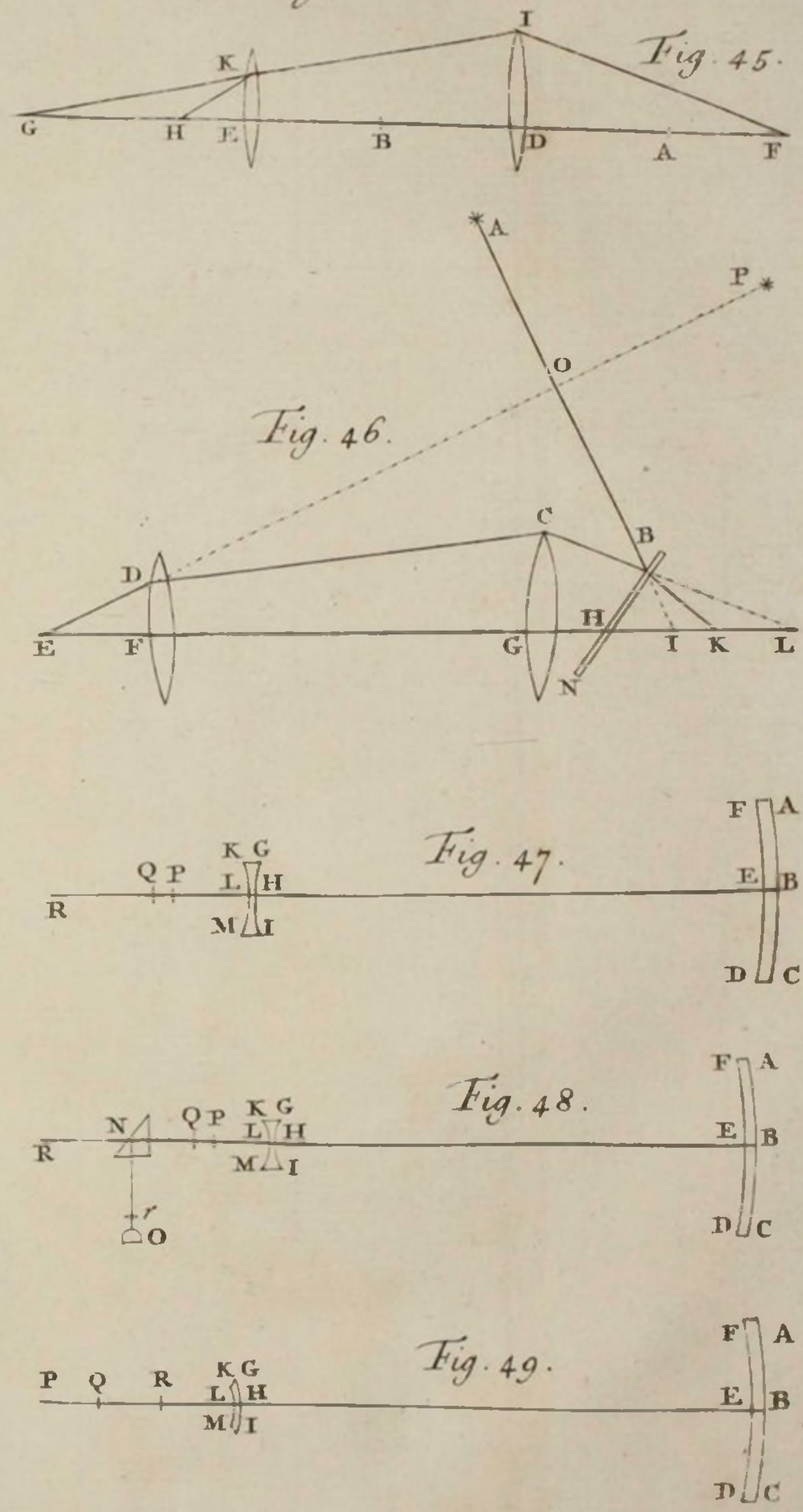


Fig. 45.

Fig. 46.

Fig. 47.

Fig. 48.

Fig. 49.

7

Fig. 50. The entire Microscope mounted on it's Pedestal, on a proper Joint, contrived so as to direct the Instrument towards any Object. *Explanation of the* Figures.

Fig. 51. The Section of the Instrument, in which A B is the larger concave metalline *Speculum*, C D is the lesser concave metalline *Speculum*; E F a hollow Brass Screw to fasten in the 1st Dioptrical Glass, or Plano-convex Lens; G H another Screw fastening on the hollow Cylinder E F I K (in which the Dioptric Glasses are contained) to the Body of the Microscope; I K a Cap with a small Perforation, serving as an Aperture to the Eye-Glass, or 2d Lens (convex on both Sides); M L is a long Screw passing through the Nuts P and V, serving to bring the small *Speculum* to a proper Distance from the larger; N Q a sliding Piece moved by the Screw, carrying the Stem Q R, and little *Speculum* C D; Y X a Screw for the Cap at *Fig.* 52; that at *Fig.* 53 is to be screwed on the Aperture I K. Fig. 50, 51. Fig. 52, 53.

Fig. 54. Shews the Construction of the Microscope, in which *i* is an Object supposed erect; from which Rays falling on the *Speculum* *a b*, will be reflected to the *Focus k*, where they will form an inverted Image, and being reflected by the small *Speculum c d*, they will pass through the Perforation of the great *Speculum*, and falling on the Plano-convex Glass *e f*, converge again, and form an erect Image at *l*; which being brought very near to the Eye, and so considerably magnified, will be distinctly seen through the Eye Glass *g h*. Fig. 54.

IV. I viewed attentively the Objects applied to these Microscopes by Mr *Leeuwenhoek* himself, which Mr *Folkes* * has given a List of in his Account; but the greatest Part of them were destroyed by Time, or struck off by Accident; which, indeed, is no Wonder, as they were only glewed on a Pin's Point, and left quite unguarded. Nine or ten of them, however, are still remaining; which, after cleaning the Glasses, appeared extremely plain and distinct, and proved the great Skill of Mr *Leeuwenhoek*, in adapting his Objects to such Magnifiers as would shew them best, as well as in the Contrivance of the Apertures to his Glasses, which, when the Object was transparent, he made exceeding small, since much Light in that Case would be prejudicial: But, when the Object itself was dark, he enlarged the Aperture, to give it all possible Advantage of the Light. The Lens being set so as to be brought close to the Eye, is also of great Use, since thereby a larger Part of the Object may be seen in one View. *An Account of Mr* Leeuwenhoek's *Microscopes; by Mr* Henry Baker, *F. R. S.* No. 458. p. 503. Sept. &c. 1740.

It must be remembered, that all these Microscopes are of one and the same Structure, and that the most simple possible, being only a single Lens, with a moveable Pin before it, on which to fix the Object, and bring it to the Eye at Pleasure.

Though I was sensible it must cost much Trouble to measure the focal Distances of these 26 Microscopes, and thereby ascertain their Powers of magnifying, I considered that, without so doing, it would be im-

* *See* Vol. VI. p. 129.

poſſible to form a right Judgement of them, or make any reaſonable Compariſon between them and our own. This Taſk therefore I have performed, with as much Care and Exactneſs as I was able; and have ſhewn, in the following Table, how many of them have the ſame Focus, and conſequently magnify in the ſame Degree; how many times they magnify the Diameter, and how many times the Superficies of any Objects applied to them. I have given the Calculations in round Numbers, the Fractions making but an inconſiderable Difference; and hope any Miſtakes I may have made in ſo nice a Matter will be excuſed.

A Table *of the* Focal Diſtances *of Mr* Leeuwenhoek's 26 *Microſcopes, calculated by an Inch Scale divided into* 100 *Parts; with a Computation of their magnifying Powers, to an Eye that ſees ſmall Objects at* 8 *Inches, which is the common Standard.*

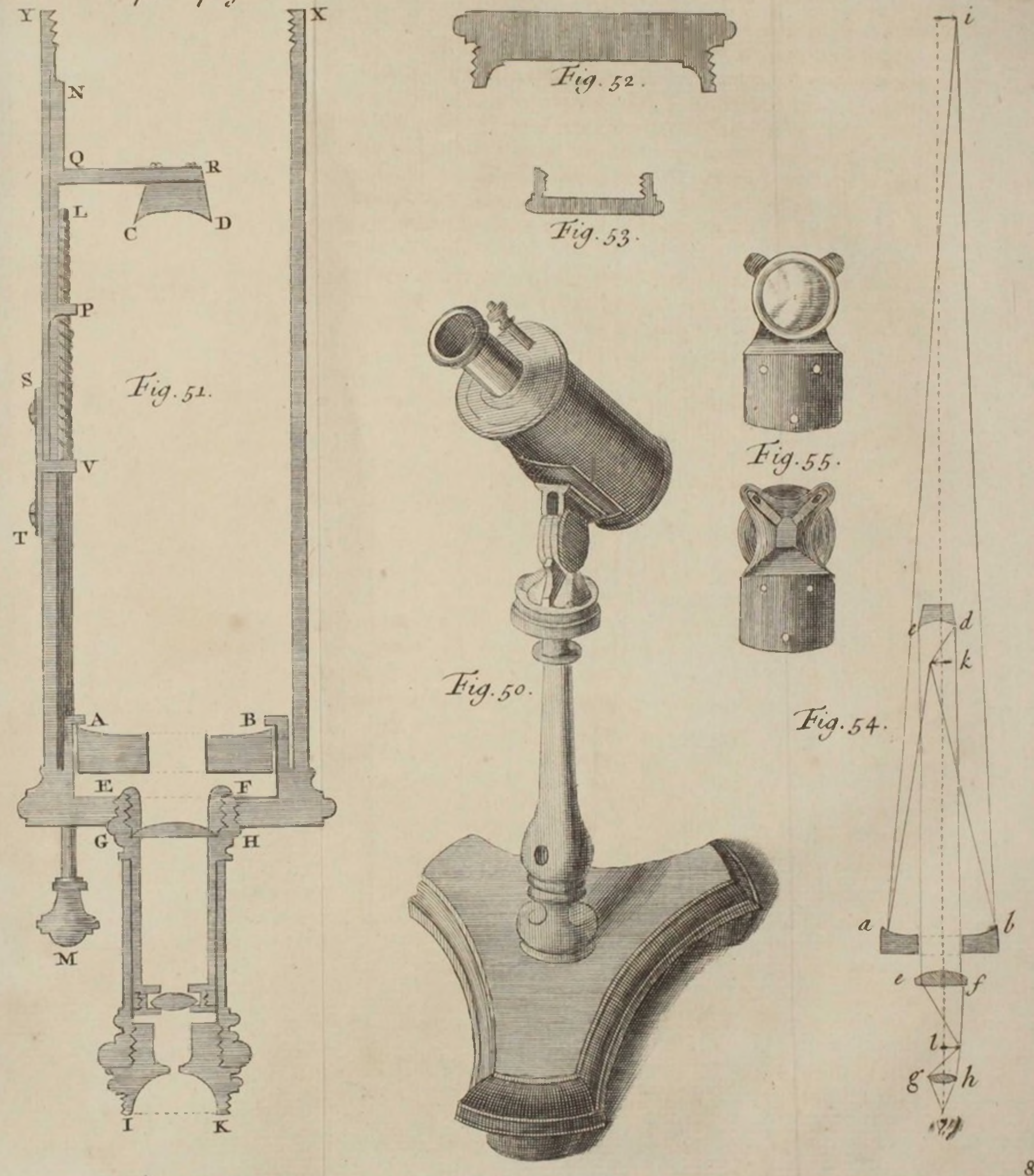
Microſcopes with the ſame Focus.	Diſtance of the Focus.	Power of magnifying the Diameter of an Object.	Power of magnifying the Superficies.
	Parts of an Inch.	*Times.*	*Times.*
*1.	$\frac{1}{20}$ or $\frac{5}{100}$	160	25600.
1.	$\frac{6}{100}$	133 nearly.	17689.
1.	$\frac{7}{100}$	114 nearly.	12996.
3.	$\frac{8}{100}$	100	10000.
3.	$\frac{9}{100}$	89 almoſt.	7921 almoſt.
8.	$\frac{1}{10}$	80.	6400.
2.	$\frac{11}{100}$	72 ſomething more.	5184 ſomething more.
3.	$\frac{12}{100}$	66 nearly.	4356 nearly.
2.	$\frac{14}{100}$	57.	3249.
1.	$\frac{15}{100}$	53 nearly.	2809 nearly.
1.	$\frac{1}{5}$	40	1600.
26.			

It appears, by the foregoing Table, that one only of theſe 26 Microſcopes is able to magnify the Diameter of an Object 160, and it's Superficies 25600 times; all the reſt falling much ſhort of that Degree. And therefore, I am fully perſuaded, and believe I ſhall be able to prove, that many of the Diſcoveries Mr *Leeuwenhoek* gives an Account of, could not poſſibly be made by Glaſſes that magnify no more than this.

Our Cabinet is but the ſecond in Mr *Leeuwenhoek*'s Collection, and is very far from containing all the Microſcopes he had, as many wrongly

* This largeſt Magnifier of all is in the Box marked 25.

Pla. VIII. Vol. VIII. part 1. page 122.



have imagined. We find here indeed, 26 Microſcopes in 13 little Boxes: each Box contains a Couple of them, and is marked in two Places with a Number, to diſtinguiſh it from the reſt. But as the firſt of theſe Boxes is marked 15, and the reſt with following Numbers on to 27; it neceſſarily implies there were 14 preceding Boxes, ſince no Man begins with the Number 15. Mr *Leeuwenhoek*, then, had another Cabinet, that held 14 Boxes before ours in numerical Order, and probably each Box contained a Couple of Microſcopes, as our Boxes do. But beſides theſe two Cabinets, he had ſeveral other Microſcopes of different Sorts, as his own Writings will make appear.

Our Cabinet ſeems to have been only his Repoſitory of Objects; for every Microſcope herein was engaged by an Object affixed to it, and thereby rendered uſeleſs for any other Purpoſe; whereas thoſe he employed in his daily Obſervations muſt have been always ready, and at full Liberty, to examine whatever offered. Many of them too muſt certainly have been much greater Magnifiers than any in our Poſſeſſion. And we are aſſured by himſelf, that ſuch he had; for he often mentions his ſhifting Objects from his common to his better, and thence to his moſt exquiſite Microſcopes: And, beſides, (in the ſecond Volume of his Works*) he ſays, "I have an hundred and an hundred Micro-" ſcopes, moſt whereof are able to ſhew Objects ſo diſtinctly, even in" the cloudyeſt Weather, and by Day-light only, that if the *Animalcula*" *in Semine maſculino* of Animals had the Extremity of their Tails" forked, (as deſcribed by a certain Writer) I ſhould eaſily have diſ-" covered it."——Among this Number, many, without doubt, were contrived for the Examination of Fluids, ſince great Part of his Obſervations were made on them: He informs us alſo, that his Method was to put them into an exceeding ſmall or capillary Tube of Glaſs, which there does not ſeem to be any Means of applying to the Microſcopes in our Cabinet, even had they been at Liberty; and much leſs for the larger Tubes he made uſe of to view the Circulation of the Blood in *Frogs*, *Eels*, *Fiſhes*, &c. his Apparatus for which we find in the fourth Volume of his Works †.——But to proceed:

Mr *Leeuwenhoek*, in a Letter to this Society concerning the *Animalcula* obſerved by him in the *Semen maſculinum* of a *Dog*, which he deſcribes and gives a Draught of, ſays, they were ſo minute, that he believed a Million of them would not equal the Size of one large Grain of Sand ‖. Again, in his 113th Letter, ſpeaking of the *Semen virile*, he declares, that a Million of the *Animalcula* ſeen therein would not equal a large Grain of Sand; and yet he gives a full Deſcription of their Form; for he ſays, their Bodies are roundiſh, ſomewhat flat before, but ending ſharp behind, with Tails exceedingly tranſparent, five or ſix times longer, and about five Times ſlenderer, than their Bodies; ſo that their Figure cannot better be repreſented, than by a ſmall *Earth-nut* with a long Root or Tail.

* Part II. *p.* 290. † Pag. 180. ‖ Vol. I. Part. I. *p.* 160.

Now the Focus of the greateſt Magnifier of his being $\frac{1}{20}$ of an Inch, as near as can well be meaſured, it is capable of magnifying the Diameter of an Object (to an Eye that ſees ſmall Objects beſt at eight Inches) no more than 160, and the Superficies 25600 times: So that Objects, one Million whereof ſcarce equal a Grain of Sand, viewed through ſuch a Lens, (as only the Superficies can be ſeen) could appear no larger than $2\frac{1}{2}$ Grains of Sand would be to the naked Eye; and I ſubmit it to be conſidered, whether that is not too ſmall a Size for any Man to deſcribe ſo particularly, and delineate the Form and Parts of.

But Mr *Leeuwenhoek* goes yet abundantly farther: For, to mention only one Inſtance, of which there are ſeveral in his Writings; he tells this SOCIETY in his Letter of *July* 25, 1684, that he could diſcern Veſſels in the human Eye, ſo amazingly minute, that, deſiring to know their Smallneſs, he meaſured them by the Diameter of a Grain of Sand, (the Proceſs of which Menſuration is there ſet down) and found by arithmetical Calculation, that a large Grain of Sand muſt be divided into 18,399,744,000 Parts *, ere it can be ſmall enough to enter theſe minute Veſſels. He muſt therefore certainly have had Glaſſes, that were much greater Magnifiers than any we have of his.

It may perhaps be objected, that Mr *Leeuwenhoek* declares, he did not uſe ſuch ſmall Glaſſes as ſome People boaſted of; and that, although for 40 Years together he had been poſſeſſed of Glaſſes exceedingly minute, he had employed them very ſeldom; ſince, in his Opinion, they could not ſo well ſerve to make the firſt Diſcoveries of Things, as thoſe of a larger Diameter. In Anſwer to this, I muſt obſerve, that Mr *Leeuwenhoek*, in this Place, is reflecting on a certain Phyſician, who boaſted of an extraordinary Microſcope †, ſcarce bigger than a viſible Point, whereby he pretended to diſcover the *Animalcules in Semine virili* to be exactly of an human Shape, with only a Skin over it. For he ſays, that while he was attentively obſerving theſe *Animalcules*, one of them (a little bigger than the reſt) preſented itſelf, having almoſt ſlipped off it's Skin: And then there plainly appeared two naked Thighs and Legs, a Breaſt, and two Arms, above which, the Skin being thruſt up, covered the Head as it were a Cap. The Sex he confeſſes he could not diſtinguiſh, and adds, that it died in endeavouring to get clear of the Skin.

Mr *Leeuwenhoek* very juſtly expoſes this romantic Diſcovery, pretended to be made by this Speck of a Microſcope; and takes occaſion therefrom to let us know, he does not think ſuch minute Glaſſes are ſo much to be depended on as thoſe of a larger Diameter. But there are ſo many Degrees between the ſmalleſt Glaſs we have of his, (whoſe Focus is at $\frac{1}{20}$ of an Inch) and this almoſt inviſible Point, that we muſt not infer from hence he uſed none of a Size between. Nay, this very Letter ſeems to imply the contrary; for it tells us, that, in examining

* Vol. I. *p.* 39. † Vol. II. Part. II. Epiſt. 116. *p.* 84.

the

the *Semen virile*, he made uſe of 8 or 10 Microſcopes of different magnifying Powers: But as all the Microſcopes we have of his, have Objects faſtened to them, and beſides have no Apparatus for Fluids, I think they could not probably be the ſame he employed for that Examination. May we not rather ſuppoſe he had 8 or 10 different Sizes of Microſcopes, that magnified more than ours? For we know, Fluids require to be examined by the greateſt Magnifiers; and doubtleſs he made uſe of ſuch for that purpoſe.

There is no Advantage in employing a greater Magnifier for any Object, than what is requiſite to ſhew the ſame diſtinctly; but when the Object is exceedingly minute, the magnifying Power of the Glaſs muſt be proportionably great, or elſe it will be impoſſible to ſee the Object clearly. A Lens, (for Example) that ſhews a whole *Flea* diſtinctly, magnifies not near enough to ſhew the *Animalcules* in the *Semen* of that *Flea*.

I am ſenſible, that Mr *Leeuwenhoek*, by long Practice, and uncommon Attention, might be able to diſcern many Objects with theſe Microſcopes, which others, leſs accuſtomed to Obſervations of this kind, cannot readily do: His Eyes too might be ſomewhat different from the Standard I meaſure by. But all theſe Allowances will not, I think, ſuffice to reconcile the Paſſages I have quoted with the Powers of the Glaſſes under Examination.

While I was overlooking theſe Microſcopes of Mr *Leeuwenhoek*, an Opportunity preſented of examining and comparing with them a curious Apparatus of Silver with ſix different Magnifiers, belonging to Mr *Folkes*, and then newly made for him by Mr *Cuff* in *Fleet-ſtreet*. The Body of this Inſtrument, into which the Glaſſes are occaſionally to be faſtened, is after the Faſhion of *Wilſon*'s Pocket-Microſcope, and contrived to ſcrew into the Side of a Scroll fixed on a Pedeſtal, from which a turning *Speculum* reflects the Light upwards upon the Object: It is likewiſe contrived to be uſed with the Apparatus of the Solar Microſcope: Deſcriptions and Figures of both of which I have ſince given in a Book intituled, *The Microſcope made eaſy*. Edit. 2^{d}. *Lond.* 1743. 8^{vo}.

I meaſured the focal Diſtances, and magnifying Powers, of the Six Glaſſes, and found them to be as follows.

A

A Table *of the Six Magnifiers belonging to Mr* Folkes's *Microſcope, calculated by an Inch Scale divided into an hundred Parts, with a Computation of their Powers, to an Eye that ſees Objects at eight Inches.*

Glaſſes.	Diſtance of the Focus.	Magnifies the Diameter.	Magnifies the Superficies.
1ſt.	$\frac{1}{50}$ of an Inch.	400.	160,000.
2d.	$\frac{1}{20}$	160.	25,600.
3d.	$\frac{8}{100}$	100.	10,000.
4th.	$\frac{18}{100}$	44.	1,936.
5th.	$\frac{3}{10}$	26.	676.
6th.	$\frac{1}{2}$	16.	256.

The above Calculation ſhews, that Mr *Folkes*'s Firſt Glaſs magnifies the Superficies of an Object 6 times as much as the greateſt Magnifier of Mr *Leeuwenhoek*: And that the *Animalcula* (a Million whereof, he ſays, ſcarce equalled the Bigneſs of a Grain of Sand) would, if viewed with this Magnifier, appear as large as 16 Grains of Sand do to the naked Eye. And I cannot ſuppoſe but Mr *Leeuwenhoek* had Glaſſes to magnify even more than this, though they are not come to us. For I cannot otherwiſe conceive, how he could obſerve the *Animalcules* in the *Semen maſculinum* of a *Flea*, and of a *Gnat*, as we find he did, or aſſert *, as he does in the ſtrongeſt Terms †, that he could ſee the minuteſt Sort of *Animalcules* in *Pepper-water*, with his Glaſſes, as plainly as he could Swarms of *Flies* or *Gnats* hovering in the Air with his naked Eye, though they were more than ten Millions of Times leſs than a Grain of Sand. And leſt this ſhould be imagined only a random Gueſs, he gives immediately a regular arithmetical Calculation to prove his Computation right. But I believe we muſt all be ſenſible, that no Glaſſes in this Cabinet are able to render ſuch minute Objects diſtinguiſhable.

I am deſirous to do all poſſible Juſtice to theſe Microſcopes, by acknowledging their Excellence, as far as their magnifying Power extends: But I ſhould do wrong to Mr *Leeuwehenhoek*, ſhould I ſuffer the World to believe theſe were his greateſt Magnifiers; ſince whoever hereafter ſhould examine them with that Imagination, would be apt to entertain a bad Opinion of his Veracity.

Experience teaches, that Globules of Glaſs extremely minute, though they magnify prodigiouſly, are ſeldom able to ſhew Objects ſufficiently diſtinct, and therefore are very apt to lead People into Errors: Which

* Vol. IV. *pag.* 21, 22. † Pag. 23.

certainly

certainly was a good Reafon for Mr *Leeuwenhoek*'s rejecting them: But a ground convex *Lens*, though much fmaller than any of his before us, if rightly applied, will fhew exceedingly minute Objects magnified to a furprifing Degree, and with fufficient Light and Clearnefs, as Mr *Folkes*'s firft Glafs witneffes.

I hope I fhall not be imagined to intend any Difrefpect to this famous Man, if I fuppofe, that our prefent Microfcopes are much more ufeful and convenient than thefe of his. Let him always be remembered with the higheft Honour, for the wonderful Difcoveries he made, and the Microfcopes he has left us, which are indeed extraordinary, when confidered as the firft almoft of their kind: Let us reverence him as our great Mafter in this Art. But the World fince muft have been ftrangely ftupid, if it could have improved nothing, where there was room for fo much Improvement. I do not mean as to the Glaffes (for the Goodnefs of thefe before us, gives juft Reafon to believe he might have others as excellent as can perhaps be ever made); but as to the Structure of the Inftrument they are fet in, and the Manner of applying Objects to them. And I fancy moft People will allow, that herein great Improvements have been made: And it is with pleafure I find, that a large Share of the Credit belongs to our own Countrymen.

One thing alone (which, when flightly confidered, may appear but trifling) has conduced greatly to thefe Improvements; and that is, the making ufe of fine tranfparent *Mufcovy Talc* or *Ifinglafs*, placed in Sliders, to inclofe Objects in. Had Mr *Leeuwenhoek* known this way, it would have faved him a vaft deal of Expence and Trouble: For then, we may reafonably fuppofe, inftead of making an entire and feparate Microfcope for every Object he was defirous to keep by him in readinefs to fhew his Friends, he would probably have fecured his Objects in Sliders, as we at prefent do, and have contrived fome fuch Means as ours, of fcrewing his feveral Glaffes of different magnifying Powers, occafionally, to one and the fame Inftrument, and of applying his Sliders to which of them he judged beft. A few good Glaffes, gradually magnifying one more than other, would, by fuch a Method, have anfwered all the Purpofes of his great Number, and his Objects would have been preferved in a much better Manner.

Two extraordinary Improvements have appeared within thefe two Years, which I beg leave to lay before you, as I think it has not been yet done. I mean, the Solar or *Camera Obfcura* Microfcope, and the Microfcope for opake Objects. Both thefe Inventions we are obliged for to the ingenious Dr *Liberkhun*, who, when he was in *England* laft Winter was Twelvemonth, fhewed an Apparatus of his own making, for each of thefe Purpofes, to feveral Gentlemen of this Society, as well as to fome Opticians, amongft whom Mr *Cuff*, in *Fleet-ftreet*, has taken great Pains to improve and bring them to Perfection; and therefore the Apparatus prepared by him is what I am about to defcribe.

This

This Solar Microſcope is compoſed of a Tube, a Looking-Glaſs, a convex Lens, and a Microſcope. The Tube is of Braſs, near two Inches in Diameter, fixed in a circular Collar of *Mahogany*, which, turning round at pleaſure, in a ſquare Frame, may be adjuſted eaſily to a Hole in the Shutter of a Window, in ſuch a manner, that no Light can paſs into the Room but through the aforeſaid Tube. Faſtened to the Frame by Hinges, on the Side that goes without the Window, is a Looking-Glaſs, which, by means of a jointed braſs Wire coming through the Frame, may be either moved vertically or horizontally, to throw the Sun's Rays through the braſs Tube into the darkened Room. The End of the braſs Tube, without the Shutter, has a convex Lens, to collect the Rays, and bring them to a Focus; and on the End within the Room, *Wilſon*'s Pocket-Microſcope is ſcrewed, with the Object to be examined applied to it in a Slider. The Sun's Rays being directed by the Looking-Glaſs through the Tube upon the Object, the Image or Picture of the Object is thrown diſtinctly and beautifully upon a Screen of white Paper, and may be magnified beyond the Imagination of thoſe who have not ſeen it. I aſſiſted lately in making ſome Experiments with Dr *Alexander Stuart*, by means of this Inſtrument, and a particular Apparatus contrived by him, for viewing the Circulation of the Blood in *Frogs*, *Mice*, &c. and had the Pleaſure of beholding the Veins and Arteries in the Meſentery of a *Frog* magnified to near 2 Inches Diameter, with the Globules of the Blood rolling through them as large almoſt as *Pepper-corns*. We examined alſo the Structure of the Muſcles of the *Abdomen*, which were prodigiouſly magnified, and exhibited a moſt delightful Picture.

The Microſcope for opake Objects remedies the Inconvenience of having the dark Side of an Object next the Eye: For by means of a concave Speculum of Silver, highly poliſhed, in whoſe Centre a magnifying Lens is placed, the Object is ſo ſtrongly illuminated, that it may be examined with all imaginable Eaſe and Pleaſure. A convenient Apparatus of this kind, with 4 different Specula, and Magnifiers of different Powers, has lately been brought to Perfection by Mr *Cuff*. Theſe, with the large double reflecting Microſcope, are, I think, the chief, if not the only uſeful Sorts now made in *England*.

I muſt not omit taking notice, that Mr *Leeuwenhoek* ſays*, that ſometimes, to throw a greater Light upon his Objects, he uſed a ſmall convex Metal Speculum. How he applied it, I will not pretend to gueſs; but it is highly probable our double reflecting Microſcope may be owing to this Hint. I muſt alſo obſerve farther, that †, after deſcribing his Apparatus for viewing *Eels* in Glaſs Tubes, Mr *Leeuwenhoek* adds, that he had another Inſtrument, whereto he ſcrewed a Microſcope ſet in Braſs; upon which Microſcope, he tells us, he faſtened a little Diſh (of Braſs alſo, I ſuppoſe,) that his Eye might be thereby aſſiſted to ſee Objects

* Vol. II. Part. II. *pag.* 93. † Vol. IV. *pag.* 182.

better: For he says, he had filed the Brass which was round his Microscope, as bright as he could, that the Light, while he was viewing Objects, might be reflected from it as much as possible. This Microscope, with it's Dish, (which I give an exact Copy of from the Picture in his Works) seems so like our opake Microscope with it's silver Speculum, that, after considering his own Words, I submit to your better Judgment, whether he is not properly the Inventor of it. His Words are these, —— " *Supra hoc Microscopium Catillum ferruminavi, ut oculus* " *objecta tanto melius videret: nam cuprum circa Microscopium, quantum* " *pote, lima abraseram, ut Lumen in conspicienda objecta, quantum pote,* " *irradiaret.*" Fig. 55.

A true Copy of a Paper found in the Hand Writing of Sir I. Newton, *among the Papers of the late Dr* Halley, *containing a Description of an Instrument for observing the* Moon's Distance *from the* Fixt Stars *at* Sea *Read* Oct 28, 1742. No. 465. p. 155.

V. In the annexed Scheme*, *P Q R S* denotes a Plate of Brass, accurately divided in the Limb *D Q*, into $\frac{1}{2}$ Degrees, $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, by a Diagonal Scale; and the $\frac{1}{2}$ Degrees, and $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, counted for Degrees, Minutes, and $\frac{1}{6}$ Minutes.

A B, is a Telescope, three or four Feet long, fixt on the Edge of that Brass Plate.

G, is a *Speculum*, fixt on the said Brass Plate perpendicularly, as near as may be to the Object-glass of the Telescope, so as to be inclined 45 Degrees to the Axis of the Telescope, and intercept half the Light which would otherwise come through the Telescope to the Eye.

C D, is a moveable Index, turning about the Centre *C*, and with it's fiducial Edge, shewing the Degrees, Minutes, and $\frac{1}{6}$ Minutes, on the Limb of the Brass Plate *P Q*; the Centre *C*, must be over-against the Middle of the *Speculum G*.

H, is another *Speculum*, parallel to the former, when the fiducial Edge of the Index falls on $00^d\,00'\,00''$; so that the same Star may then appear through the Telescope, in one and the same Place, both by the direct Rays and by the reflexed ones; but if the Index be turned, the Star shall appear in two Places, whose Distance is shewed, on the Brass Limb, by the Index.

* Fig. 56.

By this Instrument, the Distance of the Moon from any Fixt Star is thus observed: View the Star through the Perspicil by the direct Light, and the Moon by the Reflext (or on the contrary); and turn the Index till the Star touch the Limb of the Moon, and the Index shall shew upon the Brass Limb of the Instrument, the Distance of the Star from the Limb of the Moon; and though the Instrument shake, by the Motion of your Ship at Sea, yet the Moon and Star will move together, as if they did really touch one another in the Heavens; so that an Observation may be made as exactly at Sea as at Land.

And by the same Instrument, may be observed, exactly, the Altitudes of the Moon and Stars, by bringing them to the Horizon; and thereby the Latitude, and Times of Observations, may be determined more exactly than by the Ways now in use.

In the Time of the Observation, if the Instrument move angularly about the Axis of the Telescope, the Star will move in a Tangent of the

the Moon's Limb, or of the Horizon ; but the Obſervation may notwithſtanding be made exactly, by noting when the Line, deſcribed by the Star, is a Tangent to the Moon's Limb, or to the Horizon.

To make the Inſtrument uſeful, the Teleſcope ought to take in a large Angle : And to make the Obſervation true, let the Star touch the Moon's Limb, not on the Outſide of the Limb, but on the Inſide.

An Attempt to explain the Phænomenon *of the* horizontal Moon *appearing bigger, than when elevated many Degrees above the* Horizon : *ſupported by an* Experiment. *By the Rev.* J. T. Deſaguliers, *LL. D.* F. R. S. *Communicated* Jan. 30, 1734-5. No. 444. p. 390. Nov. *&c.* 1736.

VI. 1. This apparent Increaſe of the Moon's Diameter (which a Teleſcope with a Micrometer ſhews to be only apparent) is owing to the following early Prejudice, which we have imbibed from Children.

When we look at the Sky towards the Zenith, we imagine it to be much nearer to us, than when we look at it towards the Horizon ; ſo that it does not appear Spherical, according to the vertical Section E F G H I*, but Elliptical, according to the Section *e F g h i*. For this I appeal to every body's Senſe of ſeeing ; but not to their Reaſon, which is apt to take off the Prejudice in Perſons that have ſome Knowledge of Aſtronomy. Whereas any other Perſon looking up very high towards the Sky, and then forwards near the Horizon, will (when aſked) ſay, that the Sky over his Head appears much nearer. The Sky thus ſeen, ſtrikes the Eye in the ſame Manner as the long arched Roof of the Iſle of a Cathedral Church, or the Cieling of a long Room.

* Fig. 57.

This being premiſed, let us conſider the Eye at C, upon the Surface of the Earth, and imagine C at the Surface to coincide with K at the Centre ; to avoid taking into Conſideration that the Moon is really farther from the Eye when in the Horizon, than when it is ſome Degrees high. Now when the Moon is at G, we conſider it as at *g*, not much farther than G ; but when it is at H, we imagine it to be at *h*, almoſt as far again. Therefore, while it ſubtends the ſame Angle as it did before (nearly), we imagine it to be ſo much bigger as the Diſtance ſeems to us to be increaſed.

I have contrived the following Experiment to illuſtrate this :

Fig. 58.

I took two Candles of equal Height and Bigneſs, A B, C D, and having placed A B at the Diſtance of 6 or 8 Feet from the Eye, I placed C D at double that Diſtance ; then cauſing any unprejudiced Perſon to look at the Candles, I aſked which was biggeſt ? and the Spectator ſaid they were both of a Bigneſs ; and that they appeared ſo, becauſe he allowed for the greater Diſtance of C D ; and this alſo appeared to him, when he looked thro' a ſmall Hole. Then deſiring him to ſhut his Eyes for a Time, I took away the Candle C D, and placed the Candle E F cloſe by the Candle A B, and though it was as ſhort again as the others, and as little again in Diameter, the Spectator, when he opened his Eyes, thought he ſaw the ſame Candles as before. Whence it is to be concluded, that when an Object is thought to be twice as far from the Eye as it was before, we think it to be twice as big, though it ſubtends but the ſame Angle. And this is the Caſe of the Moon, which appears to us as big again, when we ſuppoſe it as far again, though it ſubtends but the ſame Angle.

 The

The Difference of Diſtance of the Moon in *Perigeo* and *Apogeo*, will account for the different Bigneſs of the horizontal Moon at different Times, adding alſo the Conſideration of the Faintneſs which Vapours ſometimes throw on the Appearance.

2. Having made an Experiment with three Ivory Balls for Confirmation of what I had advanced, that the Deception ariſes from our judging the *horizontal Moon* to be much farther than it is ; ſome Gentlemen of the Society were convinced by the Experiment, but others were not ; which obliges me to give this further Account of it, that People may judge of the Thing in Writing, which could not be ſo well attended to in the Hurry of ſeveral Perſons viewing the Experiment in Haſte.

An Explication *of the foregoing* Experiment. *By the ſame.* Ib. p. 392.

1. Two equal Ivory Balls were ſet one beyond another in reſpect of the Eye at E, namely, A B at 20 Feet Diſtance from the Eye, and C D at 40. Fig. 59.

2. It is certain, by the Rules of Optics, that the Eye at E or F will ſee the Ball C D under an Angle but half as big as it ſees the Ball A B ; that is, that the Ball C D muſt appear no bigger than the Ball *o* P placed by the Side of A B.

3. But when looking at the two Balls with the naked Eye in an open Room, we conſider that C D is as far again from the Eye as A B, we judge it to be as big as A B, (as it really is) notwithſtanding it ſubtends an Angle but of half the Bigneſs.

4. Now if, unknown to the Spectator, (or while he turns his Back) the Ball C D be taken away, and another Ball *o P* of half the Diameter be placed in the ſame Line, but as near again, at the Side of A B, the Spectator thinking this laſt Ball to be at the Place of C D, muſt judge it to be as big as C D, becauſe it ſubtends the very ſame Angle as C D did before.

It follows therefore, That if a Ball be imagined to be as far again as it really is, we make ſuch an Allowance for that imagined Diſtance, that we judge it to be as big again as it is, notwithſtanding that the Angle under which we ſee it, is no greater, than when we look at it, knowing it's real Diſtance.

For this Reaſon the Moon looks bigger in the Horizon, and near it, than at a conſiderable Height, or at the Zenith: Becauſe it being a common Prejudice to imagine that Part of the Sky much nearer to us which is at the Zenith, than that Part towards the Horizon ; when we ſee the Moon at the Horizon, we ſuppoſe it much farther ; therefore as it ſubtends the ſame Angle (or nearly the ſame Angle) as when at the Zenith, we imagine it ſo much bigger as we ſuppoſe it's Diſtance greater.

The Reaſon why this Experiment is hard to make, is becauſe the Light from the Ball *o P* is too ſtrongly reflected on account of it's Nearneſs ; but if we could give it ſo little Light as to look no brighter than the Ball C D, it would deceive every body. I have made the Experiment ſo as to deceive ſuch as were not very long-ſighted ; but I must confess I have found it very hard to deceive those who see at a great Distance; tho' they would all be deceived, if the Distances were of 300 or 600 Feet. Now in the Case of the Moon, the Deceit is helped, because the Vapours, through which we see it when low, take away of it's Brightness, and therefore have the same Effect as would (or does) happen in the Experiment, when the Light of the Ball *o* P strikes the Eye no stronger than the Light of the Ball C D.

CHAP. III.

ASTRONOMY.

Observations of the Appearances among the Fixt Stars, called Nebulous Stars, *by* W. Derham, *D. D. Canon of* Windsor, *F. R. S.* No. 428. p. 70.

I. THESE Appearances in the Heavens, have borne the Name of *Nebulous Stars*: But neither are they *Stars*, nor such Bodies as emit, or reflect Light, as the Sun, Moon, and Stars do; nor are they *Congeries*, or *Clusters* of Stars, as the *Milky Way*: but whitish *Areæ*, like a Collection of *Misty Vapours*; whence they have their Name.

There are many of them dispersed about, in diverse Parts of the Heavens. There is a Catalogue of them in *Hevelius*'s *Prodromus Astronomiæ*, which may be of good use to such as are minded to inquire into them.

Besides these Dr *Halley* * hath mentioned one in *Orion's Sword*; another in *Sagittary*; a third in the *Centaur* (never seen in *England*) a fourth preceding the right Foot of *Antinous*; a fifth in *Hercules*; and that in *Andromeda*'s *Girdle*.

Five of these six I have carefully viewed with my excellent eight Foot Reflecting Telescope, and find them to be Phænomena much alike; all except that preceding the right Foot of *Antinous*, which is not a *Nebulose*, but a *Cluster* of *Stars*, somewhat like that which is in the *Milky-Way*.

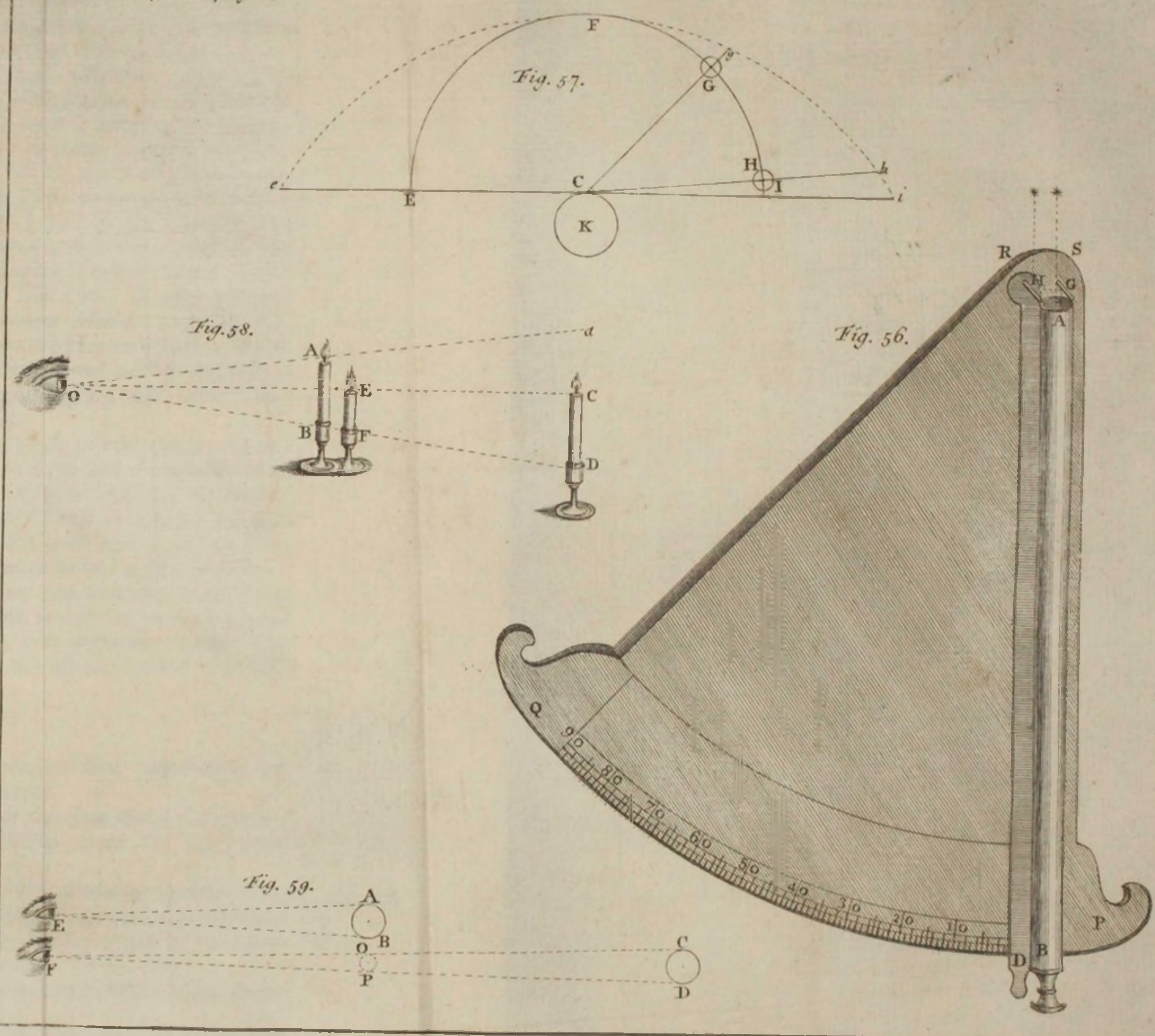
Between the other four, I find no material Difference, only some are rounder, some of a more oval Form, without any Fixed Stars in them to cause their Light; only that in *Orion*, hath some Stars in it, visible only with the Telescope, but by no means sufficient to cause the Light of the *Nebulosæ* there. But by these Stars it was, that I first perceived the Distance of the *Nebulosæ* to be greater than that of the *Fixed Stars*, and put me upon enquiring into the rest of them. Every one of which I could very visibly and plainly discern, to be at immense Distances beyond the Fixed Stars near them, whether visible to the naked Eye, or Telescopick only; yea, they seemed to be as far beyond the Fixed Stars, as any of those Stars are from the Earth.

And now from this Relation of what I have observed from very good, and frequent Views of the *Nebulosæ*, I conclude them certainly not to be *Lucid Bodies*, that send their Light to us, as the Sun and

* See Vol. I. Chap. iv. §. 13. and Vol. IV. Chap. iii. §. 7.

Moon.

Pla. IX. Vol. VIII. part 1st. page 132.



Moon. Neither are they the *combined Light* of *Clusters* of Stars, like that of the *Milky-Way*: But I take them to be *vast Areæ*, or *Regions of Light*, infallibly *beyond the Fixed Stars, and devoid of them.* I say *Regions*, meaning Spaces of a vast Extent, large enough to appear of such a Size as they do to us, at so great a Distance as they are from us.

And since those Spaces are devoid of Stars, and even that in *Orion* itself, hath it's Stars bearing a very small Proportion to it's *Nebulosæ*, and they are visibly not the Cause of it, I leave it to the great Sagacity and Penetration of this Illustrious Society, to judge whether these *Nebulosæ* are particular Spaces of Light; or rather, whether they may not, in all probabality, be Chasms, or Openings into an immense Region of Light, beyond the Fixed Stars. Because I find in this Opinion most of the Learned in all Ages (both Philosophers, and I may add Divines too) thus far concurred, that there was a *Region beyond the Stars.* Those that imagined there were *Crystalline*, or *Solid Orbs*, thought a *Cœlum Empyræum* was beyond them and the *Primum Mobile*; and they that maintained there were no such Orbs, but that the Heavenly Bodies floated in the Æther, imagined that the Starry Region was not the Bounds of the Universe, but that there was a Region beyond that, which they called the *Third Region*, and *Third Heaven.*

To conclude these Remarks, it may be of use to take notice, that in *Hevelius*'s *Nebulosæ*, some seem to be more large and remarkable than others; but whether they are really so, or no, I confess I have not had an Opportunity to see, except that in *Andromeda*'s *Girdle*, which is as considerable as any I have seen. In his Maps of the Constellations, the most remarkable are the three near the Eye of *Capricorn*; that in *Hercules*'s *Foot*; that in the third Joint of *Scorpio*'s *Tail*; and that between *Scorpio*'s *Tail* and the *Bow of Sagittary.* But if any one is desirous to have a good View of these, or any other of the *Nebulosæ*, it is absolutely necessary that he should make use of very good Glasses, else all his Labour would be in vain, as I have found by Experience.

II. 1.

Observation of the Moon's *Transit by* Aldebaran, April 3, 1736, *made at* London *by* John Bevis, *M. D.* No. 446. p. 90. July, &c. 1737. Fig. 60.

Apparent Time. H.	M.	S.	
7	40	00	The *Moon*'s Body and *Aldebaran* seen together in the distinct Base of the Telescope.
7	45	52	The *Moon*'s southern Limb running along the parallel Thread, the western Limb came to the horary Thread.
7	49	41	The Glass remaining fixed, and *Aldebaran* running along the parallel Thread, (having the same Declination with the *Moon*'s southerly Limb) came to the Intersection of the Threads.
8	13	04	The *Moon* again running along the Parallel, came to the horary Thread.
8	15	50	*Aldebaran* (the Glass remaining fixed) came to the first oblique Thread at *c*.

H.	M.	S.	
8	15	$54\frac{1}{2}$	——— to the hoary Thread at *b*.
8	15	59	——— to the ſecond oblique Thread at *a*.
8	59	54	*Aldebaran* in the Line paſſing through the Cuſps, his neareſt Diſtance from the *Moon*'s Body, being ſomewhat leſs that the Length of *Mare Criſium*, or nearly $\frac{1}{10}$ of the *Moon*'s Diameter.

Fig. 61.

An Occultation of Aldebaran, Dec. 23, 1728, Styl. Nov. *obſerved by* D. Chriſtfried Kirchius, *Aſtronomer Royal at* Berlin. No. 454. p. 223. July, &c. 1739. Fig. 61.

2.

	Time by a Pendulum Clock. h ′ ″		Parts of the Micrometer, with a Tube of 4 Feet.	Value of the Parts of the Micrometer. ′ ″	Corrected Time. h ′ ″	More correct Time. h ′ ″
1	6. 56. 37	∗ from the Centre of *M. Sinai*	38	15. 21	6. 24. 37	
2	6. 58. 30	∗ from the Centre of *M. Sinai*	34	13. 42	26. 30	
3	7. 3. 54	Immerſion of the Star			6. 31. 54	6. 32. 00
4	8. 5. 29	Emerſion of the Star. Tube 9 Feet			7. 33. 29	7. 33 33
5	5. 35	Emerſion certainly made			7. 33. 35	
6	6. 43	∗ from the Centre of *M. Sinai*	36	14. 32	34. 43	
7	15. 32	∗ from the neareſt Edge of ☽	8	3. 14	43. 32	
8	20	∗ from *M. Sinai*	$46\frac{1}{2}$	18. 47	48	
9	28	Diameter of the Moon or	74 } $73\frac{1}{2}$ }	29. 73 29. 41	56	
10	29. 12	*M. Sinai* from the neareſt Edge	8	3. 14	7. 57. 12	
11	38	Diameter of ☽. Tube 9 Feet	$99\frac{1}{2}$	29. 38	8. 6.	
12	42	Diameter of ☽. Tube 7 Feet	75	30	10	
13	46. 55	∗ from *M. Sinai*	72	29. 5	14. 55	
14	8. 48	Diameter of the Moon	$73\frac{1}{2}$	29. 41	8. 16	

The Situation of the Star, with regard to the lunar Spots was obſerved in the following Manner.

Before *Obſ.* 1. 6ʰ. 20′. I obſerved the Star in a right Line drawn from the ſouthern Edge of *Inſula Macra* (*Poſidonius*) through the northern Part of *Pontus Euxinus* (Middle of *Mare Serenitatis*) and *M. Ætna* (*Copernicus*) and a Line from *M. Sinai* to the Star almoſt touched the Shore of *Sinus Sirbonidis* (*M. Humorum*).

At the Time of *Obſ.* 1. the Star was in a right Line, drawn from the greater black Lake (*Plato*) through the eaſtern Parts of *Inſula Cercinna* (from *Kepler* toward the Eaſt).

At the Time of *Obſ.* 2. the Star was in a Line, continued through the Middle of *Palus Mæotis*, and the Middle of *M. Adriaticum* (through the Middle of *Mare Criſium* and S).

At the Time of the Immerſion of the Star, the following right Lines coincided with it, and marked the Place of the Circumference of the Moon, where the Star was hid. 1. From the Shore of *Pontus Euxinus* (*M. Serenitatis*) to the Northward, through *M. Ætna* (*Copernicus*). 2. From the Shore of the *Sinus Apollinis*, through the *Loca Paludoſa* (from the Shore of *S. Iridum* through *Kepler*). 3. From *M. Sinai* (*Tycho*) through the ſouthern Shore of *S. Sirbonis* (*M. Humorum*).

The

The Emerſion of the Star happened over-againſt *M. Paropamiſi (Furnerii)* and in a right Line drawn from the greater black Lake *(Plato)* through *Byzantium (Menelaus)*, which touched the extreme Bay of *Pontus (M. Nectaris)*.

At the Time of *Obſ.* 7. *M. Porphyrites (Ariſtarchus)* the northern Edge of *L. Theſpitis (Fracaſtorius)* and the Star in a right Line.

At the Time of *Obſ.* 8. the upper *Lacus Hyperboreus (Kermes)* the Middle of *Palus Mæotis (Mare Criſium)* and the Star in a right Line.

3.

h	′	″	
6.	27.	35	Immerſion.
7.	29.	20	Emerſion.
1.	1.	45	Duration.

At Wittenberg *in* Saxony, *obſerved by* Jo. Frid. Weidler, *F. R. S.* Ibid. p. 225.

The Obſervation was made by two Spectators at the ſame time, with one Teleſcope of 9 Feet, and another of 4.

The Immerſion and Emerſion were obſerved about a Minute ſooner by the long Tube than by the ſhort one.

The Appulſe of the Star to the eaſtern Edge of the Moon was about 163° of *Hevelius*'s moveable Scheme of the Full Moon. It emerged about 272° of the ſame Scheme. Therefore a right Line, joining the Points of Immerſion and Emerſion, touches the Extremities of *Mare Humorum* and *Nubium*, and paſſes between *Pilatus* and *Mare Nubium*.

Selenograph. p. 364.

The Sky was not clear at the time of the Immerſion, but thin Clouds almoſt continually wandered before the Moon and Star; and therefore the Star appeared oblong a great while before the Occultation, through the Vapours of the Atmoſphere.

			h	′	″
4.	The Occultation at	—	5.	27.	6.
	Emerged at	— —	5.	29.	59.
	Duration	— —	1.	2.	53.

In Fleet-ſtreet, London, *obſerved with a reflecting Teleſcope of* 15 *Inches in Length, by Mr* G. Graham, *F. R. S.* No. 459. p. 632. Jan. *&c.* 1741.

The Sun's Tranſit at Noon at $11^h.\ 59'.\ 52''$. the Clock gaining of the mean Solar Time about one ſecond in a Day.

III. 1. This Obſervation was made in *Fleet-ſtreet, London*, with a Teleſcope of 10 Feet in Length, fitted with a Micrometer.

An Obſervation of the Eclipſe of the Sun on May 2, 1733, *in the Afternoon, by Mr* G. Graham, *F. R. S.* No. 429. p. 113. July, *&c.* 1733.

App. Time.

At 5^h	$44'$	$45''$	It began.
6	25	30	The Cuſps were vertical.
6	37	30	The Eclipſe was greateſt, the lucid Part of the Sun's Diameter meaſuring 426 Parts, whereof the Sun's Diameter meaſured 2311. So that the Eclipſe was $9\frac{4}{5}$ Digits.
6	46	00	The Cuſps were horizontal.
7	28	23	The Eclipſe ended.

2. I

Of the same by Mr Stephen Gray, *F.R.S.* Ibid. p. 114.

2. I observed the late Eclipse of the Sun, at *Norton*-Court, near *Feversham* in *Kent*, the Seat of *John Godfrey*, Esq; and the Week following, being with *Granville Wheler*, Esq; at *Otterden*-Place, near *Lenham* in *Kent*, he was pleased to communicate to me his Observations of the said Eclipse.

At Norton-Court *by Mr* Gray, p. 115.

		Appar. Time h.	m.	s.	
Observat.	1	5.	49.	15	Beginning
	2	5.	53.	15	1 Digit
	3	5.	57.	30	2 Digits
	4	6.	2.	55	$3\frac{1}{4}$
	5	6.	11.	50	5
	6	6.	16.	43	6
	7	6.	21.	7	7
	8	6.	27.	0	8
	9	6.	32.	45	9
	10	6.	37.	30	$9\frac{1}{2}$
	11	6.	40.	0	$9\frac{7}{8}$ Greatest
	12	6.	56.	56	8
	13	7.	0.	35	$7\frac{5}{8}$
	14	7.	7.	0	6
	15	7.	11.	55	5
	16	7.	17.	0	4
	17	7.	21.	15	3
	18	7.	25.	55	2
	19	7.	32.	30	End

Our Observations were made with an Helioscope, or Instrument, consisting of a Telescope and Box, with a Digit Scheme at the End of it. The Telescope was 6 Feet, the Box 2 Feet in Length, and the Sun's Image on the Scheme was 6 Inches $\frac{8}{10}$ in Diameter. The Clock was rectified on the Day of the Eclipse, and proved to need no Correction for several Days afterwards, by Observations of the Sun on the Meridian. The Sun's Transit was taken by the Passage of it's Rays through a Hole made in a Brass Plate, the Center of which Hole was at 6 Feet and 3 Inches perpendicular Height, above the horizontal Plane on which the Meridian Line was drawn.

At Otterden-Place, *by Mr* Wheler, p. 116.

Mr *Wheler* observed the Beginning at $5^h\ 49'\ 0''$, and the End at $7^h\ 31'\ 49''$. His Observations were made with a Telescope of 15 Feet in length, and his Time was also rectified by a Meridian Line; but it was done by a Transit of the Rays through a Hole at a much greater Height. For the Brass Plate, in which the Hole was made, was fixed to a Window in the Roof of his Hall, at the Height of 27 Feet above the Meridian Line on the Floor.

3. The

3. The Beginning at 5.h 34.m 00s 17.° 45′ }
at 6. 00. 00 13. 36 } Sun's Altitude.
End at 7. 14. 30 2. 45 }

Of the same, by Mr J. Milner, *at* Yeovil *in* Somersetshire. Ibid. p. 116.

I made use of a Quadrant 2 Feet *Radius.*
Lat. *Yeovil*, 51°.

4. The Latitude of *Gottenburg* is 57°. 40′. 54″.

At Gottenburg *in* Sweden, *by* D. Birgerus Vassenius, *Reader of Mathematicks.* Ibid. p. 134.

The Beginning of the Eclipse, which could not be observed because of the Clouds, seems to have happened before 6h. 26′. p. m.

h ′ ″	
6. 38. 43	The Sun was about 3 Digits eclipsed.
6. 49. 52	Six Digits, more or less.
7. 14. 6	♃ appeared.
7. 14. 46	The whole Disk of the Sun began to be covered.
7. 15. 50.	The greatest Darkness, when all the Stars of the Great Bear, the Lion's Heart, Sirius, Procyon, the Bull's Eye, and some others were visible: but neither ☿ nor ♂ were seen.
7. 16. 54	The Sun began to dart his Rays with incredible Quickness.
7. 20. 12	♃ still appeared.
7. 41. 38	The Sun was 6 Digits eclipsed.
8. 5. 50	The End of the Eclipse, the whole Disk of the Sun shining.

Total Duration of the Eclipse at *Gottenburg*, 2′ 8″.

The total Duration of this Eclipse in a Place called *Swenaker*, 7 *Swedish* Miles from hence, in Latitude 58° 15′, was, according to the Observation of my Brother *Torstanus Vassenius* by a Pendulum, 2′ 31″.

Whilst the Sun was totally covered, I saw not only the greatest Part of the Spots in his Disk, but also the Atmosphere of the Moon, with a Telescope of about 21 *Swedish* Feet; it was a little brighter at the western Limb of the Moon, at the time of the greatest Immersion; but without that Irregularity and Inequality of the luminous Rays, which appeared to those who looked without a Telescope. But the most worthy of Observation were 3 or 4 little reddish Spots in it, seen without the Circumference of the lunar Disk; one of which was greater than the rest, about the middle Way between the South and West, according to the best Judgment that could be made. This was composed of 3 smaller parallel Parts or *Nubeculæ*, of unequal Length, with some Obliquity to the Circumference of the Moon. I saw it plainly preserve the same Situation for 40″ or more: but at length a Ray of the Sun breaking out like Lightning deprived me of any farther Opportunity of observing so beautiful a *Phænomenon.*

At Wittemberg *in* Saxony, *by* Joh. Frid. Weidler, *Prof. Math. and* F. R. S. No. 433. p. 332. July, *&c.* 1734.

5. Phases of the beginning Eclipse.

h	′	″		h	′	″	
6	36	5	p. m. Beginning of the Eclipse	7	2	50	six Digits
	39	50	one Digit		7	50	seven Digits
	45	1	two Digits		10	50	eight Digits
	48	50	three Digits		15	50	nine Digits
	52	50	four Digits		19	50	ten Digits
	58	5	five Digits		29	20	eleven Digits

Some light Clouds come over the Sun.

Phases of the decreasing Eclipse.

h	′	″		h	′	″	
7	35	50	ten Digits	7	41	50	eight Digits
	40	50	nine Digits		46	5	the Sun sets

Fig. 62.

The Circle drawn in the Figure represents the Image of the Sun, of the same Magnitude as it appears at the Bottom of the Helioscope.

The Light of the Sun near the Orb of the Moon, which I have usually observed, in other Solar Eclipses, to have a vehement Motion and Undulation, was in this Eclipse perfectly still and quiet.

The Orb of the Moon discovered a manifest Asperity to all the Observers, especially in the western Part, in the Phases that were observed a little before the Setting of the Sun; but there were some Intervals, in which the Tops of the Lunar Mountains were distinguished, but not very broad or deep. By the Application of a Scale nicely divided, I estimated the Depth of one Valley to be $\frac{1}{200}$ Part of the Diameter of the Moon.

The last decreasing Phases were seen thro' thin Clouds, and yet the Moon did not hide from us above 11 Inches of the Disk of the Sun.

The Setting of the Centre of the Sun was then found by Calculation to be $7^h\ 39'\ 49''$ for the Horizon of *Wittemberg*, and so it was retarded near 6 Minutes by the Refraction of the Rays in the Clouds of the Horizon.

Eclipse of the Sun April 22, 1734, *observed at* Rome *by the Abbot* Didacus de Revillas, *F. R. S. and* Andreas Celsius, *F. R. S.* Prof. Astron. Upsal. No. 442. p. 296. July, &c. 1736.

IV. This Eclipse was observed with a very good Telescope, of about 6 Roman Palms in Length.

True Time p. m.

h	′	″	Digits	
22	22	35	0	The Beginning seemed to be a small Matter over thro' the Cloud.
	27	1	0½	
	34	0	1	
	42	6	1½	
23	0	52	2	
	3	16	2	Or a little more, and the greatest Darkness seemed to be at Hand.
	10	31	2	
	28	16	1½	
	45	11	0½	
	52	1	0	The End.

From

From the 4th and 8th Obſervations we may gather, that the greateſt Darkneſs was about 23^h $5'$.

V.

Eclipſe of the the Sun Sept. 23. 1736. *obſerved at* London, *by* J Bevis, *M.D.* No. 446. p. 98. July, &c. 1737. Fig. 63.

Apparent Time. p. m.

h	′	″	
4	12	55	The northern Limb of the Sun running over the Parallel Thread P P, the weſtern Limb touches the horary Thread H H.
	12	42	The ſmall Spot near the northern Limb reaches the firſt oblique Thread 1.
	13	1	The Spot reaches the horary Thread H H.
	13	20	The Spot reaches the ſecond oblique Thread 2.
	14	45	The eaſtern Limb of the Sun reaches the horary Thread. Then cloudy.
4	45	41	The Sun getting out of the Clouds, the Eclipſe appears thro' the Teleſcope to be but juſt begun.
	45	48	Still imperceptible to the Eye thro' a coloured Glaſs.
	46	00	Now very ſenſible. Then cloudy.
5	5	29	The ſouthern Limb running along the Parallel, the weſtern Limb reaches the horary Thread.
	5	41	The weſtern Cuſp of the Sun reaches the horary Thread.
	7	5	The eaſtern Cuſp touches the horary Thread. Then the Sun was covered with Clouds till it ſet.

Fig. 64.

I place the Beginning at 4^h $45'$ $31''$, p. m.

VI. 1.

Eclipſe of the Sun, Feb. 18, 1736-7. *obſerved in* Fleetſtreet, London, *by Mr.* Geo. Graham, *F. R. S.* No. 447. p. 175. Jan.&c.1738.

Appar. Time. p. m. At

h	′	″		
2	25	9	A ſmall Impreſſion appeared on the Sun's Limb; I judge the Beginning to have been about 5 or 6″ ſooner.	
3	21	28	The middle of the firſt and larger Spot was covered.	
	29	30	The middle of the ſmaller Spot.	
	40	4	The Cuſps perpendicular.	
4	3	34	The Cuſps horizontal.	
	35	32	The middle of the larger Spot emerged.	
	38	21	The ſmaller emerged, or a little before.	
4	52	57	The Chord between the Cuſps — —	1057
	55	00	The Chord — — — —	954
	56	32	The Chord — — —	851
	59	34	The Chord — — —	632

Then a Cloud cover'd the upper Limb, and prevented a Sight of the ending, which was ſoon after.

Between twelve and one a Clock, I meaſured the Diameter of the Sun with a Micrometer. At the Time of the greateſt Obſcuration, the lucid Part of the Sun's Diameter was equal to 392 ſuch Parts as his whole Diameter contained 2188.

I had a Transit of the Sun at Noon, and of *Sirius* at Night, which, compared with preceding ones, I found my Clock went too fast for mean Solar Time, about 1'' in a Day.

——At the Royal Observatory at Greenwich, *observed by* J. Bevis, *M. D. in Company with Dr* Halley; Ibid. p. 176.

2.

Appar. Time. p. m.	At
h ' ''	
2 25 39	The Beginning.
5 3 29	The End.

At the End, the Sun's Limb appeared somewhat tremulous, and a small thin Cloud came over it. Dr *Bevis* judged the Time might be relied on to 2 or 3''.

—Edinburgh, *by* Colin Mac Laurin, *Prof. Math. F.R.S.* Ibid. p. 177.

3. In the History of Eclipses collected by *Ricciolus*, there are very few said to be Annular; and of these some have been controverted, as that seen by *Clavius* at *Rome*, *April* 9, 1567, and that seen by *Jessenius* at *Torgaw* in *Misnia*, *Feb.* 25, 1598, which are both disputed by *Kepler.* Some Astronomers, Antient and Modern, have been of Opinion, that no Eclipse can be Annular: and since such seem to have been rarely observed, and I have not met with a particular Description of any of them, I shall give as full an Account of this Eclipse as I can collect from the Observations that were made here, and those that have been communicated to me from the Country.

The Sky was generally favourable in the Southern Parts of *Scotland* during the Eclipse; and though there were great Showers of Snow in the North, they had sometimes a View of it. There was something very entertaining in the annular Appearance, a *Phænomenon* that was equally new to all who saw it, that gave great Delight to the Curious, without striking Terror into the Vulgar. It extended Southward almost to *Morpeth* in *Northumberland*, and beyond *Inverness* Northward; so that a Part of *England*, and almost all *Scotland*, were within it's Limits. I have not as yet learned how far the North Limit was from us; but I am informed, that the Weather was very unfavourable there.

Ten Days before the Eclipse, I wrote to many of my Acquaintance in the Country, desiring that they would determine the Duration of the annular Appearance as exactly as possible; in Hopes, by comparing their Observations, to have traced the Path of the Centre and the Limits of this *Phænomenon* after the Example given in 1715, by Dr *Halley*, to whom we owe the best Description of an Eclipse that Astronomical History affords. I shall give an Abstract of the Accounts I received in Answer to these Letters, after I have described our Observations at *Edinburgh.*

The Times of the Apearances here were determined by a Pendulum Clock, which Mr *Graham* gave me some Years ago, from whom I also had the meridian Instrument by which it is examined. The meridian Line was often adjusted in the usual Manner, and an exact Account of the Sun's Transits in the Meridian, and of the Transits of

Procyon

Procyon in a fixed Teleſcope, was kept by Mr *Short* for a long Time before and after the Eclipſe; and, by comparing his Obſervations, I cannot doubt but that the Times were determined with ſufficient Exactneſs. I was often with him when he examined the Meridian, and obſerved thoſe Tranſits; particularly the Day of the Eclipſe, when by the Sun's Paſſage in the Meridian, we found that the Clock was before the apparent Time 13 Minutes 27 Seconds; and ſo much I have ſubducted from the Times that were marked during the Obſervation. The Latitude of this Place is commonly ſaid to be 55 Degrees 55 Minutes; and by ſome Trials we have made lately, this muſt be near the Truth, though in ſome Maps and Tables it be repreſented greater. By comparing an Obſervation we had here of the End of the Eclipſe of the Moon, *Nov.* 20, 1732. with an Obſervation of the End of the ſame Eclipſe by Mr *Graham* in *Fleetſtreet*, the Longitude of this Place is a little more than 12′ of Time further Weſt.

Some Days before the Eclipſe, Lord *Aberdour* ſet up a Clock in the Caſtle, and adjuſted it with mine by a Watch that ſhewed the Seconds. The Clocks were compared together the Day of the Eclipſe at Noon, by a Cannon fired from the Caſtle, ſome Perſons being appointed to attend each Clock, and mark the Seconds when they heard the Sound: An Allowance of 2″½ being made for the Progreſs of the Sound, (which was determined by ſeveral Trials at Night) the Clock in the Caſtle was found to be before the apparent Time 12′ 19″, and ſo much is ſubducted from the Times that were marked in the Caſtle during the Obſervation. It was agreed that we ſhould give Signals to one another mutually at the Beginning and End of the Eclipſe, and at the Beginning and End of the annular Appearance. His Lordſhip's Signal from the Caſtle was a Cannon, ours from the College a Muſquet, Perſons being appointed to mark our Signals from a proper Place of the Caſtle: There is no Regard however had to thoſe Signals in marking the Times of the Appearances. Lord *Aberdour* made uſe of a reflecting Teleſcope of 15½ Inches focal Diſtance, that magnified 90 times; only he obſerved the annular Appearance with one of 5½ Inches, that he might have a View of the whole Diſk of the Sun at once. Mr *Short* obſerved the Beginning of the Eclipſe with a Teleſcope of 15½ Inches focal Diſtance, that magnified 104 Times, but the annular Appearance with one of the ſame Length, that alſo took in the whole Diſk of the Sun, and magnified 50 times. The reflecting Teleſcope with which I obſerved the Eclipſe from the Beginning to the End, took in the whole Diſk of the Sun, (having been made by Mr *Short* for this Purpoſe) though the focal Diſtance of the big Speculum be 9½ Inches; and though it bears a higher Charge, I made Uſe of an Eye-glaſs on this Occaſion, that magnifies only 50 times.

By a Computation that had been made here from Sir *I. Newton*'s Theory, I expected that the Eclipſe would begin at 2^h 6′, apparent Time; we therefore looked attentively towards the South-weſt Part

of

of the Sun's Limb from Two o'Clock. At $2^h\ 5'\ 36''$ we perceived a Depreſſion that was juſt diſcernible on the Sun's Limb near that Place; our Signal was then made, but by an Accident Lord *Aberdour* had been hindred from obſerving the Sun at that Time: However, when he looked for it, he ſaw it was begun, and his Signal gave general Intimation of this to the Town, about $40''$ after we had firſt perceived it; and, as far as I have learned, it was not diſcerned by the Eye, though aſſiſted with a ſmoaked Glaſs, till about this Time.

I obſerved the Progreſs of the Eclipſe by a Helioſcope; but after 10 Digits were eclipſed, I returned to the Teleſcope, to attend the Beginning of the annular Appearance. A little before the *Annulus* was complete, a remarkable Point or Speck of pale Light appeared near the Middle of the Part of the Moon's Circumference, that was not yet come upon the Diſk of the Sun; and a Gleam of Light more faint than this Point, ſeemed to be extended from it to each Horn: I did not mark the preciſe Time when I firſt perceived this Light, but am ſatisfied that it could hardly be leſs than $\frac{1}{4}$ of a Minute before the annular Appearance began. Mr *Short* (who was in another Chamber at ſome Diſtance, and made uſe of a larger Teleſcope) aſſures me that he ſaw it $20''$ before the *Annulus* was completed; and this is confirmed by a Call that was then heard from the Chamber where he was, of which I did not underſtand the Meaning till we met afterwards, and upon which the Perſon who made our Signals was about to fire, if I had not forbid him. I was ſurprized with this Light at firſt, and did not immediately recollect that it proceeded probably from the ſame Crown that was ſeen about the Moon in a total Eclipſe of the Sun at *Naples* in 1605; and was obſerved by many in different Parts of *Europe*, in the three late total Eclipſes of 1706, 1715, and 1724. I did not expect to have ſeen this Light, when ſo much of the Sun's Diſk was uncovered; but as I kept only ſo much of the Diſk in the Teleſcope as was neceſſary for aſcertaining the Time of the Formation of the *Annulus*, this muſt have contributed to my diſcovering it; for this Light was very faint, compared with that which appeared upon the Sun's Arch near the ſame Place the Moment it was uncovered, and the *Annulus* completed.

Moſt of thoſe who obſerved the Eclipſe with Teleſcopes, mention in their Letters, that as the *Annulus* was forming, they perceived the Light to break in ſeveral irregular Spots near the Point of Contact, and that the Limb of the Moon ſeemed to be indented there. Some expreſs themſelves as if thoſe irregular Parts had appeared to them in a kind of Motion. It is thus deſcribed by Mr *Bayne*, Profeſſor of the Municipal Law, ' What appeared to me moſt entertaining, con-
' ſidered as an Object of Sight, was, when the Extremities of the
' Horns formed upon the Face of the Sun ſeemed as if they had been
' in the Action of uniting their Points, the Inequalities on the
' Extremity of the Moon's Diſk gave the Appearance, as it were,
' of

'of ſmall Bodies in particular Motion.' There was not any Undulation at this Time on the Circumference of the Sun. I find that ſuch Appearances of a tremulous Motion in certain Periods of ſolar Eclipſes are mentioned by *Hevelius* and others. Lord *Aberdour* obſerved the Beginning of the annular Appearance with a ſmaller Teleſcope, and perceived only a narrow Streak of a duſky red Light to colour the dark Edge of the Moon, immediately before the Ring was completed, and after it was diſſolved.

At $3^h\ 25'\ 55''$ the Circumference of the Sun appeared complete, and perfectly circular. We called at the ſame Inſtant to the Perſon who was appointed to make our Signal, and in a Second or two the Cannon from the Caſtle was heard. The *Annulus* appeared to the Eye to be central for ſome time, but in the Teleſcope it was always broader toward the South-eaſt than towards the North-weſt Part of the Sun's Diſk. The Breadth appeared much greater to the naked Eye, than could have been expected from the Difference of the Semidiameters of the Sun and Moon. This was ſo remarkable, that ſuch a *Phænomenon* muſt have confirmed thoſe Aſtronomers in their Opinion, who imagined that the Diameter of the Moon is contracted in her Conjunctions with the Sun. This Appearance proceeded chiefly, I ſuppoſe, from the Light's incroaching on the Shade, as is uſual; but whatever was the Cauſe, every Body ſeemed ſurprized that the Moon appeared ſo ſmall upon the Diſk of the Sun.

It was obſerved, that the Motion of the Moon appeared more quick in the Formation and Diſſolution of the *Annulus*, than during it's Continuance. This is particularly deſcribed by Mr *Fullarton*, of *Fullarton*, in a very exact Account of the Eclipſe, as it appeared at his Seat at *Crosby*, near *Aire*, on the Weſt Coaſt of *Scotland*. He writes that, 'the *Annulus* appeared to be nearly of an uniform Breadth, during the 'greater Part of the Time of it's Continuance, but ſeemed to go off 'very ſuddenly; ſo that when the Diſk of the Moon approached to 'the concave Line of the Sun's Diſk, they ſeemed to run together 'like two contiguous Drops of Water on a Table when they touch 'one another;' and he adds, that it came on in the ſame way. This Appearance ſeems to be accountable from the ſame optical Deception as the former.

During the Appearance of the *Annulus*, the direct Light of the Sun was ſtill very conſiderable; but the Places that were ſhaded from his Light appeared gloomy. There was a Duſk in the Atmoſphere, eſpecially towards the North and Eaſt. In thoſe Chambers that had not their Lights Weſtwards, the Obſcurity was conſiderable. *Venus* appeared plainly, and continued viſible long after the *Annulus* was diſſolved, and I am told that other Stars were ſeen by ſome: One Gentleman is poſitive, that being ſhaded from the Sun, he diſcerned ſome Stars Northwards, which he thinks by their Poſition were in *Urſa Major*.

It was very cold at this Time; a little thin Snow fell; and some little Pools of Water in the College Area, where there was no Ice at two o'Clock, were frozen at Four. A reflecting Telescope of a large Size, and of a much greater Aperture than ordinary, that took in the whole Sun, and burned Cloth very suddenly through the tinged Glass at the Beginning of the Eclipse, and on that Account could not then be used with Safety, was that by which Mr *Short* observed the annular Appearance. Some curious Gentlemen found, that a common Burning-glass, which kindled Tinder at $3^h\ 59'$ and burned Cloth at $4^h\ 8'$ had no Effect during the annular Appearance, and for some time before and after it.

I have mentioned those Things mostly upon the Report of others; for during the greater Part of this Appearance I was observing the Progress of the Moon upon the Disk of the Sun through the Telescope. The first internal Contact of the Disks, at the Formation of the *Annulus*, was considerably below the West Point of the Sun's Disk; and the second Contact, at the Dissolution of the *Annulus*, seemed to be about 10 Degrees Eastwards from the North Point or Zenith of the Disk: But I did not find that the Position of those Points of Contact could be estimated with Exactness on several Accounts. The Breadth of the *Annulus* towards the South-east Part of the Sun's Disk, was at least double of it's Breadth towards the opposite Part, about the Middle of this Appearance. An Apparatus, by which I was in Hopes of being able to determine those Things more accurately, was not ready. I proposed to have made some Estimation of the *Ratio* of the Continuance of the annular Appearance, where it was central to it's Continuance at *Edinburgh*, from that of the Arithmetical Mean betwixt the Numbers that should express the Proportion of the greatest and least Breadth of the *Annulus* to the Geometrical Mean betwixt the same Numbers; or from the *Ratio* of the *Radius* to the Sine of half the Arch intercepted between the two Points of internal Contact; but I did not obtain these *Ratio's* with sufficient Exactness.

At $3^h\ 31'\ 43''$ the *Annulus* was dissolved, after having continued $5'\ 48''$. And here again our Signals were heard immediately after one another: The Middle of the Eclipse was therefore at $3^h\ 28'\ 49''$. In this the Time by Observation did not agree so well with the Time by Computation as in the Beginning of the Eclipse, the Difference being here about four Minutes. The Irregularities of the Moon's Surface occasioned the same Appearances, in some measure, as at the Formation of the *Annulus*. When I returned to the Helioscope, there was some Time lost in directing it towards the Sun; and when I got the Image in a due Position, there was less than 11 Digits eclipsed; and I suspect that it never amounted to full 11 Digits. I had no Micrometer.

After taking some more Digits, I went with Sir *John Clerk* to a neighbouring House, to observe the End of the Eclipse, being

2 afraid

afraid we ſhould not be able to ſee it from the College. By a Signal that was made to the Perſon who attended the Clock, (2′′ being ſubducted, that were loſt in making the Signal) the End was at $4^h\ 44'\ 51''$. The Wind blew hard at this Time, ſo that the Teleſcope could not be kept very ſteady, and there was ſome Undulation on the Circumference of the Sun; but I cannot think that the Error of this Obſervation can exceed 3 or 4′′, the Circumference of the Sun appearing to me complete at that Inſtant.

I ſhall now ſubjoin the Obſervations that were made in the Caſtle and College in one View, by which you will ſee that they agree preciſely as to the Continuance of the annular Appearance, a Coincidence that could not have been expected; but ſo it is, according to the Numbers that were given me immediately after the Eclipſe by thoſe whoattended the Clocks.

	In the College.			In the Caſtle.		
	h	′	′′	h	′	′′
The Beginning of the Eclipſe at	2	5	36			
The Beginning of the annular Apppearance	3	25	55	3	25	53
The End of the annular Appearance	3	31	43	3	31	41
The End of the Eclipſe	4	44	51	4	44	48

By Lord *Aberdour*'s Obſervations, the lowermoſt and biggeſt of the two Spots that appeared upon the Diſk of the Sun in the upper Part, was touched by the Moon at $3^h\ 4'\ 40''$ and this Spot was wholly covered at $3^h\ 5'\ 19''$. Mr *Short* obſerved another Spot at the Circumference of the Moon, at $2^h\ 24'\ 51''$. Though the Obſervations of the Digits could not be made with ſo much Exactneſs as the preceding, on ſeveral Accounts, I ſhall ſubjoin ſome of them.

		h	′	′′
The Sun was eclipſed	2 Digits at	2	21	14
	6 Dig.	2	50	54
After the annular Appearance	9 Dig.	3	45	57
	8 Dig.	3	52	55
	7 Dig.	3	59	53
	6 Dig.	4	6	51

At *Hopeton-Houſe*, nine Miles Weſt, and a little Northwards from *Edinburgh*, Lord *Hope* obſerved the annular Appearance begin at $3^h\ 25'$ the End of this Appearance at $3^h\ 31'$ and the End of the Eclipſe at $4^h\ 44'\frac{1}{2}$. His Lordſhip was obliged to obſerve the Eclipſe at a Diſtance from the Clock, and to determine the Times by a Pocket Watch, that had been adjuſted by a very good Dial that Day at 12 o'Clock; but aſſures me that the Duration of the annular Appearance was 6′, as near as could be judged by a Watch that did not ſhew the Seconds. The Moon appeared to touch the larger Spot above-mentioned at $3^h\ 4'$ and covered it in about half a Minute. The Emerſion of the ſame Spot was at $4^h\ 13'$. A leſſer Spot, higher on the Sun's Diſk, was not covered till $11'$ after the greater Spot, but appeared rather ſooner than it.

At *Crosby*, on the Weſt Coaſt of *Scotland*, about 4 Miles North from *Aire*, Mr *Fullarton* obſerved the Eclipſe to begin at 2 o'Clock. A diſtinct *Annulus* was formed about $20'$ after 3, which continued exactly $7'$, meaſured by a Pendulum vibrating Seconds. It appeared rather broader on the lower Verge of the Sun; but the Difference muſt have been very ſmall, for it was but barely diſcernible in a Species of the Eclipſe 6 Inches over, caſt on a Piece of Paper behind the Eye-piece of a Teleſcope 6 Feet long. He adds, that the Day-light was not greatly obſcured, appearing only ſo much dimmer than uſual, as that of the Sun is, when ſeen through a very gentle Miſt in a fine Morning in *April* or *May*. Sir *Thomas Wallace* found that the annular Appearance continued at his Houſe near *Lockryan* in *Galloway* $5'$.

From the Obſervation at *Crosby*, the Centre of the annular Penumbra ſeems to have entered *Scotland* not far from *Irwine*. It proceeded afterwards towards the Eaſt, with a conſiderable Inclination Northwards; and probably left *Scotland* not far from *Montroſe* on the Eaſt Coaſt: For the Reverend Mr *Auchterlony* found, that the annular Appearance continued there $7'$, as near as he could judge by an ordinary Watch. The *Annulus* alſo appeared to him of an uniform Breadth, through a common Teleſcope. This Obſervation, though not ſo exact as that at *Crosby*, is however confirmed by that at *St Andrew*'s, to be mentioned afterwards These two Obſervations at *Crosby* and *Montroſe*, were made nearer the Path of the Centre, than any others that have been communicated to me.

As for the Southern Limit of this Appearance, the Eclipſe was not annular at *Newcaſtle*, and there wanted about 40 Degrees of the Limb of the Sun to appear in order to form an *Annulus*, according to the Obſervation of Mr *Iſaac Thompſon*. The whole Duration of the Eclipſe was $50''$ leſs by his than by our Obſervation; and the bigger Spot was hid $1^h\ 9'\ 35''$ by his Obſervation, the Digits eclipſed at it's Immerſion 7, 7; at it's Emerſion 4, 1. Nor was the Eclipſe annular at *Morpeth*, whence Mr *John Willſon* writes, that the Body of the Moon appeared almoſt entirely on that of the Sun; and that to the naked Eye, the Diſk of the Sun ſeemed to be almoſt round.

But of all the Obſervations that have been communicated to me, that of Mr *Long* at *Longframlington* *, determines the Southern Limit with the greateſt Exactneſs. The *Annulus*, he ſays was very ſmall there upon the upper Part, and the Duration 40 or 41 half Seconds, meaſured by a Pendulum 9, 81 Inches long; from which we may conclude,

* *Longframlington* is 7 computed Miles on this Side of *Morpeth*.

conclude, that the Limit was very near this Place. I have received no Accounts concerning this Appearance from any Places on the West Coast of *England*. At *Alnwick* in *Northumberland* the Eclipse was annular, but I have not heard that the Time of it's Continuance was measured.

At *Berwick*, the annular Appearance continued betwixt 4 and $5'$. The End of the Eclipse at *Dunbar*, by Mr *Mark*'s Observation, was at $4^h\ 48'\ 16''$, but there was some Mistake committed in reckoning the Vibrations of the Pendulum in measuring the Continuance of the *Annulus*.

At *St Andrew*'s, this Appearance was observed to continue precisely $6'$, by a Pendulum Clock, by Mr *Charles Gregory* and Mr *David Young*, Professors in the University. By a Figure of the *Annulus* taken from it's Image, projected through a Telescope upon a Paper Screen, the Breadth towards the South-east Part of the Sun's Disk is rather more than double of it's Breadth towards the opposite Part.

I have already mentioned the Observation at *Montrose*. At *Aberdeen*, the *Annulus* was observed by Mr *John Stewart*, Math. Prof. for $3'\ 2''$. It was almost central, when the Clouds deprived him of any further View of it; he thinks it probable, that it continued there about $6'$. Several Gentlemen, who live on the Coast Northwards from *Aberdeen*, were desired to observe the Continuance of the *Annulus*; but I do not find that any of them saw this Phænomenon from the Beginning to it's End.

At *Elgin*, the Eclipse was observed annular at $3^h\ 29'$ the larger Part of the Ring being uppermost, by the Reverend Mr *Irwin*, who had a View of it for about $30''$; but by reason of intervening Clouds could not determine the Beginning or End of this Appearance. At *Castle Gordon*, Mr *Gregory* had one View of the Eclipse while it was annular, but could make no further Observation for the same Reason.

At *Inverness*, the Eclipse was annular for some Minutes, as I am informed by several Gentlemen; but they did not measure the precise Time how long it continued. By the Accounts I have had from *Fort Augustus* and *Fort William*, it is doubtful whether the Eclipse was annular in those Places or not. *Fort Augustus* is at the West End of *Lochness*, and probably was not far from the Northern Limit of this *Phænomenon*. I have as yet received no Accounts of this Appearance from any Place further Northwards, or from any Place in the West, but those I have mentioned. Some Gentlemen in *Argyleshire*, who observed this Eclipse, were deprived of a View of the *Annulus* by the Clouds.

Mr *Walker*, an ingenious Gentleman at *Frazerburgh* on the North Coast, found that from the Time of the Ring's beginning to appear upon the lower and Western Part of the Sun's Disk, till it began to break on the East and upper Part, there were 300 Vibrations of a Pendulum, or $5'$. The Ring ſeemed ſomewhat narrower even at the Middle of the Eclipſe on the lower Part.

This is the Sum of what I have been able to learn concerning the Obſervations of this Eclipſe, that were made in this Country, and in the neighbouring Parts of *England*. I have made ſome Computations relating to the Extent of the annular *Penumbra*, and the Direction and Velocity of it's Motion; but ſince I have not a ſufficient Number of exact Obſervations, by which I might examine them, it would be of little Uſe to deſcribe them. Had the Weather been more favourable in the North, and my Requeſt of having the Duration of the annular Appearance meaſured, been made more public before the Eclipſe, after Dr *Halley*'s Example in 1715; I doubt not but I ſhould have been able to have given a more exact Account of the Progreſs of the Centre of this Phænomenon, and of it's Limits; but I had been diſcouraged from publiſhing any Thing concerning it, by our bad Fortune in ſeveral late Eclipſes, of which the Clouds had not allowed us the leaſt View.

I am informed, that there was very little Notice taken of this Eclipſe by the Populace in the Country; and I cannot but add, that ſeveral Gentleman of very good Credit, who are not in the leaſt ſhort-ſighted, aſſure me, that about the middle of the annular Appearance they were not able to diſcern the Moon upon the Sun, when they looked without a ſmoaked Glaſs, or ſomething equivalent.

I have taken Notice of this, becauſe it may contribute to account for what at firſt Sight appears ſurprizing, that there are ſo few annular Eclipſes in the Liſts collected by Authors. *Kepler*, in his *Aſtron. Optic.* does not ſeem to acknowledge, that any Eclipſe, truly annular, had ever been obſerved. There are none mentioned by *Ricciolus*, from the Year 334 till 1567, though there are 13 or 14 total Eclipſes recorded within that Period; yet it is allowed, that the Extent and Duration of the annular Appearance may be conſiderably greater in the former, than of the Darkneſs in the latter. It may have contributed to this, that annular Eclipſes muſt have been rather incident in the Winter Seaſon in the Northern Hemiſphere, and that Eclipſes have been more readily total in the Summer, when their Chance of being viſible was greater, and the Seaſon more favourable for obſerving them. But perhaps the chief Reaſon why few annular Eclipſes appear upon Record, is, that they have not been diſtinguiſhed in moſt Caſes from ordinary partial ones. The Darkneſs diſtinguiſhed total Eclipſes, or ſuch as were very nearly total; and it is theſe chiefly, Hiſtorians mention. There are two central Eclipſes of the Sun ſtill famous amongſt the Populace in this Country: That of *March* 29, 1652 was total here, and that Day is known amongſt them by the Appellation of *Mirk Monday*. The Memory of the Eclipſe of *Feb.* 25, 1598, is alſo preſerved amongſt them, and that Day they term, in their way, *Black Saturday*. There is a Tradition, that ſome Perſons in in the North loſt their Way in the Time of this Eclipſe, and periſhed in the Snow.

There was a remarkable total Eclipſe of the Sun in this Country, *June* 17, 1433, the Memory of which is now loſt among the Populace; but it appears from a Paſſage in a Manuſcript in our Library, that it was formerly called by them the *Black Hour*, after their uſual Manner. It is deſcribed thus: 'This year there was a wonderful 'Eclipſe of the Sun, on *June* 17, about 3 in the Afternoon; and 'for about half an Hour, a Darkneſs like Night overſpread the Face 'of the Earth; ſo that nothing was viſible to human Eyes; whence 'it has commonly been called the *Black Hour*.' This Eclipſe is not in *Ricciolus*'s Catalogue, but is mentioned by him in another Place, *Schol. Cap.* 2. *L* 5. By a Computation of this Eclipſe, the Sun was within two Degrees of his *Apogeum*, and the Moon within 13 Degrees of her *Perigeum*; ſo that this muſt have been a remarkable Eclipſe. The Progreſs of the Shadow was towards the South-eaſt; and *Sethus Calviſius* cites the *Turkiſh* Annals for it's being total in ſome Part of their Dominions.

P. S. We looked for the Occultation of *Aldebaran* by the Moon on *Feb.* 25, in the Evening; but the Star paſſed by the upper Horn, without being hid, at a Diſtance from it, that was by Eſtimation nearly equal to the Diſtance betwixt the neareſt Part of the Spots *Eudoxus* and *Ariſtotle*.

4. We had a very fine bright Day for obſerving the Eclipſe; and never was any Thing of that kind, I believe, obſerved with more Exactneſs. In ſeveral Places for 10 Miles round this City, as well as in it, were ſome ſkilful Perſons ſtationed for that Purpoſe: I myſelf happened to be in the Caſtle here, which is an Eminence at leaſt of 500 or 600 Feet in Height, beſides a great Aſcent from the Level of the Sea to the Foot of the Rock upon which it is ſituated.

— *At* Edinburgh, *by the Hon. Sir* John Clerk, *Bart. one of the Barons of his Majeſty's Exchequer there, and F. R. S.* Ibid. p. 195.

Mr *Mac Laurin* had placed himſelf at a Window in our College; others were ſent where the Eclipſe we ſuppoſed, would be perfectly central, about 12 or 14 Miles farther North.

A Gun from the Caſtle was fired at 22″ after Twelve, mean Time, (or 12′ 22″ before Twelve, apparent Time) upon which, by Agreement, the Clocks and Watches of the Obſervers were adjuſted. A ſecond Cannon was diſcharged preciſely when the Eclipſe began, which was at 5′ 36″ after Two. A third was diſcharged when the annular Appearance began, which was at 25′ 55″ after Three; it's Continuation was 5′ 48″. A fourth Cannon was fired at the End of the Eclipſe, which was at 44′ 50″ after Four; all reckoned by apparent Time. We had half a ſcore good reflecting Teleſcopes to make theſe Obſervations, and our Calculations perfectly agreed, ſo that you may depend upon them as moſt exact.

This was not done by us as a Matter of mere Curioſity, but to aſſiſt in aſcertaining the Motions of the Moon, on Sir *I. Newton*'s

Theory

Theory upon which a good deal of the Doctrine of the Longitude will depend. Sir *Isaac*'s Calculation, as to the Beginning of this Eclipse, was pretty right; but not so well as to it's central Appearance. Two Spots in the Sun made a very distinct Appearance to us, as they entered under the Moon's Body; one was a little above the central or horizontal Line of the Sun, shaped as in the Figure; the other was near the Edge, on the East Quarter. The first, by Comparison with the Sun's Diameter, was larger than the Disk of our Earth; it was dark in the Middle, and certainly emitted no Fire or Light. The Edge of the Moon appeared a little ragged or rough, but not mountainous, because of the Sun's Light. There was no considerable Darkness, but the Ground was covered with a kind of a dark greenish Colour. Two Stars appeared, the Planet *Venus*, and another farther Eastward. This Account is what you may depend on.

Fig. 65.

—*At* Trinity-College, Cambridge, *and at* Kettering; *communicated in a Letter from Mr* Charles Mason, Ibid. p. 197.

	h	'	''	
5. The Beginning by the Clock — — —at	2	36	40	
The End — — — — — — —at	5	14	12	Exact.

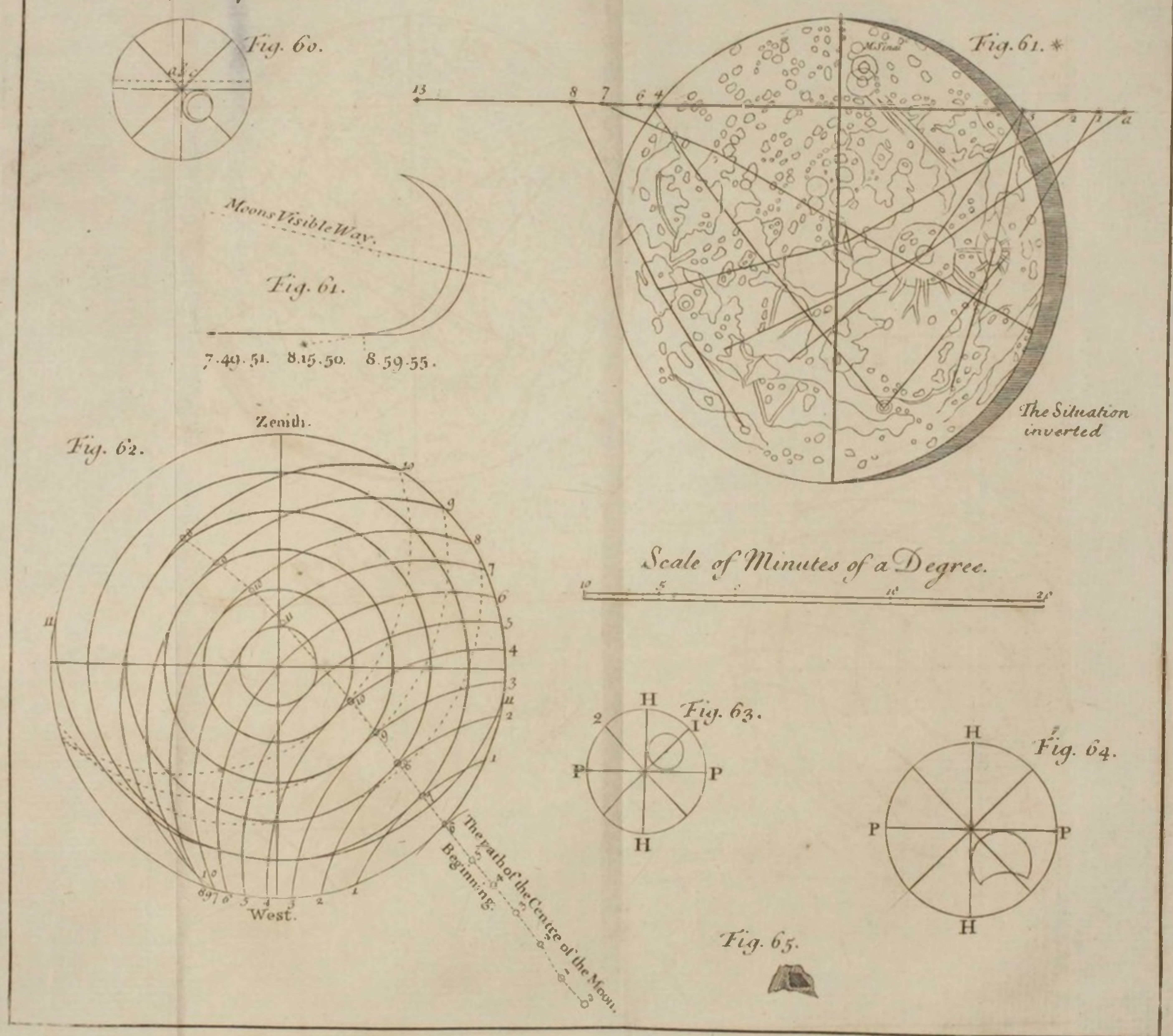
Digits.	The Eclipse observed. Increasing. h	'	''	Decreasing. h	'	''
0½	2	39	30	5	11	50
1	2	43	00	5	9	00
1½	2	46	40			
2	2	50	25			
2½	2	54	15	4	59	30
3	2	58	5			
3½	3	1	55			
4	3	5	50			
4½	3	9	50			
5	3	14	00	4	42	55
5½	3	18	10	3	39	10

Digits.	The Eclipse observed. Increasing. h	'	''	Decreasing. h	'	''
6	3	22	20	4	35	20
6½	3	26	30			
7	3	30	40	4	27	40
7½	3	34	50	4	23	55
8	3	39	30	4	20	10
8½	.	.	.	4	16	30
9	Clouds.			4	13	00
9½	.	.	.	4	10	10
10	.	.	.	4	7	50

	h	'	''
The lesser Spot immerged — — — — — —at	2	58	50
The greater Spot began to immerge — — — —at	3	33	05
The Middle — — — — —at	3	33	20
The End at — — — — — —	3	33	37

Times

Pla. X. Vol. VIII. part 1. pa. 150.



Times observed at *Kettering*, as follow:

	h	′
Beginning — — — — — — — —	2	21
2 Digits — — — — — — — —	2	36
Centre — — — — — — — —	{ 3	07
	4	22 }
End — — — — — — — — —	4	59
Great Spot immersed — — — — — —	3	18

N. B. The Observatory Clock was 1′ 50″ too slow, which being added all the way will give true Time.

6. The Beginning of this Eclipse was above seven Hours sooner than by our Calculations. For at $3^h\ 33'\ 36''$, part of the Sun's Limb seemed to be obscured by the Moon, as we looked through a smoaked Glass, fitted to a Telescope of 11 Feet, whereas but a little before, at 33″, the Sun appeared quite round thro' the same Telescope. But the Calculations placed the Beginning of the Eclipse at $3^h\ 41'$. — *At* Bologna, *by* ——— Ibid. p. 199.

We then observed the Digits of the Eclipse on a white Table, upon which the Rays of the Sun were thrown, by an Optical Tube of 6 Feet; there was a Circle inscribed on the Table, measured by the Image of the Sun, and divided into Digits and half Digits. The Observation was pretty much disturbed by the Wind shaking the Instrument. The following seem to have been the most certain Phases.

h	′	
3	40	about one Digit was eclipsed.
3	48	two Digits.
3	57	three Digits.
4	6	four Digits.
4	15½	five Digits.
—	—	
4	35	seven Digits.
4	45	seven Digits ½ which seemed to us the greatest Darkness.
4	55	seven Digits again, the Eclipse now decreasing.

When the Appearance of the Sun going down began to appear too fluctuating and trembling, and disfigured from a round into an oval Shape, we left off measuring the Digits, because it was not attended with sufficient Certainty.

Some Spots appeared in the Sun, especially 3, the Positions of which at Noon that Day, being described from the Observations, is exhibited

Fig. 66.

exhibited in the Scheme. We have thus determined the Occultations of two of these by the same Tube of 11 Feet.

h	′	″	
4	23	18	The Limb of the Moon touches the *Corona* of the Spot A,
	23	49	It begins to touch the *Nucleus* of the Spot A.
	24	25	It hides the whole *Nucleus*.
	26	14	It touches the Spot B.
	26	31	It covers the whole.

— *On Mount* Aventine *at* Rome, *by the Abbot* Didacus de Ravillas, *F. R. S.* Ibid. p. 200.

7. The Image of the Sun was thrown thro' a Telescope of *Campanus* upon a white Table, with a Circle equal to the Image, divided into 12 Digits. The Phases observed by this Instrument are as follow.

h	′	″ p. m.	
3	43	4	The Limb of the Sun was found to be a very little obscured by the Limb of the Moon.
	51	50	one Digit.
4	0	40	two Digits.
	9	30	three Digits.
	18	20	four Digits.
	27	10	five Digits.
	30	00	six Digits, whilst the Limb of the Moon touches the Centre of the Sun, thick Clouds take away the Sight of both Luminaries, and of the succeeding Phases of the Eclipse.

——*At* Wittemberg *in* Saxony, *by* J. Frederick Weidler. Ibid. p. 201. Fig. 67.

8. Digits of the decreasing Eclipse

Digits	h	′	″ p. m.
8	4	50	31
7¾		58	16
7½	5	1	36
7¼		5	26
7		8	16
6½		10	16

Afterwards, as the Sun went down, it was hid in Clouds. The Beginning could not be seen because of Clouds.

— *At* Philadelphia *in* Pensylvania, *by Dr* Kearsly. No. 446. p. 121. July, &c. 1737.

9. The Eclipse *Feb.* 18. could not be well observed here, by Reason of Clouds. I rectified my Clock by one of *Heath*'s large Ring Dials. At $7^h\ 18'$ there was a small Dent in the Sun's Edge, whence the Beginning 1 or $2'$ sooner: Just before the End, *viz.* 10^h 11 or $12'$, I had a Sight of the Sun again, and there was then a Dent in the Sun's Edge, so that the End must be 10^h 13 or $14'$ in

2 the

the Morning: About the Middle of the Eclipſe, there was a large Spot near the Middle of the enlightened Part which was the North Side of the Sun.

VII. 1. This Obſervation was made by a Refracting Teleſcope of 12 Feet *Focus*, armed with a Micrometer, and by a reflecting Teleſcope of 9 Inches focal Length.

Eclipſe of the Sun, obſerved Aug. 4, 1738. by Mr George Graham *and Mr* Short, *FF. R. S. at Mr* Graham's *Houſe in* Fleetſtreet, London. No. 453. p. 91. April, *&c.* 1739.

	h	′	″ a. m.
Beginning of the Eclipſe at — — — — —	9	59	20
End at — — — — — — — — —	11	59	36

	dig.	min.
Quantity of Obſcuration by the Micrometer — —	3	28

	h	′	″
Duration — — — — — — — — —	2	00	16

N. B. The Perſon who was obſerving the Tranſit of the Sun over the Meridian, obſerved the End to be at the ſame Inſtant with the above Obſervation.

2. This Obſervation was made with a Tube of 7 Feet, armed with one of Mr *Graham*'s Micrometers.

— At Upſal, *by* Andreas Celſius, *F. R. S. and R. S. Suec. Secr.* Ibid. p. 92.

True Time.

h	′	″			
12	18	52	Beginning of the Eclipſe.		
12	35	57	Digits eclipſed	0	$5\frac{3}{5}$
12	37	47		0	$3\frac{1}{5}$
12	42	22	End.		
0	23	30	Duration.		

Becauſe of the Clouds that covered the Sun at times, I could not obſerve the greateſt Darkneſs and other Phaſes of the Eclipſe; but we may deduce from theſe Obſervations, that the greateſt Obſcuration was 0 8′ Digits at $12^h\ 30'\ 37''$.

3. I could not obſerve either the Beginning or End of the Eclipſe, becauſe of the Clouds; but as they were ſometimes broken by the Wind, I had an Opportunity to obſerve the following Phaſes.

— At Wittemberg, *by* Joh. Frid. Weidler, *F. R. S.* Ibid. p. 92. Fig. 68.

h	′	
11	30	The firſt Phaſe of the increaſing Eclipſe was obſerved, 1 Digit.
12	19	p. m. Another Phaſe was ſeen, 2 Digits 30′.
12	37	The third Phaſe of the decreaſing Eclipſe was ſeen.

There were alſo ſeen at the ſame Time 10 Spots in the Diſk of the Sun.

 The

The Disk of the Moon under the Sun shewed the Circumference exactly terminated, without any Inequality, and very black. No Trace of any Atmosphere on the Orb of the Moon could be perceived.

The Calculation taken from the Ludovician Tables erred both as to the Magnitude and Time: For the Magnitude was predicted to be 2 Digits 20′; and the Middle to be $12^h\ 5'$.

—*At* Bologna, *by* Eustachius Manfredi, *F. R. S.* Ibid. p. 94.

4. As the Disk of the Sun abounded with Spots at this Time; on the Morning of the approaching Eclipse, about $21^h\ 30'$ p. m. *Eustachius Zanotti Phil. Doct. and Math. Prof. Publ.* my Collegue, traced out the Position of the chief of them, by the Help of a Micrometer, fitted to a Tube of 8 Feet. These occupied chiefly the southern Part of the Sun, which the Moon was to cover. It was not necessary to describe them all, nor could it be done for the Multitude of Spectators. Those, of which the Places could be determined, are shewn in the Scheme.

Fig. 69.

The Beginning of the Eclipse was not perceived before $22^h\ 52'\ 25''$, p. m. tho' I had long observed it with a Tube of 11 Feet, and others with other Tubes. I am of Opinion however, that the Contact of the Luminaries happened at least a Minute sooner, than I perceived it; which seems to be confirmed by the succeeding Phases.

The Digits described by Circles on a Table, after the usual Manner, and the Parts of Digits are determined by Estimation. The Telescope was 6 Feet; the Image was 2 Inches or thereabouts. The Phases of the Emersion are more certain than the Phases of the Immersion for many Reasons.

Phases of the Immersion. True Time.					Phases of the Emersion. True Time.				
h	′	″			h	′	″		
23	0	10	dig. 1		0	4	14	dig. 4 $\frac{4}{5}$	still
	11	20	2			18	5	4 $\frac{1}{2}$	
	25	56	3			22	43	4 $\frac{1}{3}$	
	35	14	4	doubtful		31	50	4	
	45	14	4 $\frac{1}{3}$			39	13	3 $\frac{1}{2}$	
	47	6	4 $\frac{1}{2}$			46	50	3	
	51	14	4 $\frac{3}{5}$			52	55	2 $\frac{1}{2}$	
	55	14	4 $\frac{2}{3}$			57	31	2	
	58	14	4 $\frac{3}{4}$		1	3	26	1 $\frac{1}{2}$	
0	1	46	4 $\frac{4}{5}$			7	52	1	
					1	13	4	$\frac{1}{2}$	

	h	′	″
End of the Eclipse with a Tube of 11 Feet	1	18	1
8 Feet	1	18	2

In

Pla. XI. Vol. VIII. part 1. pa. 154.

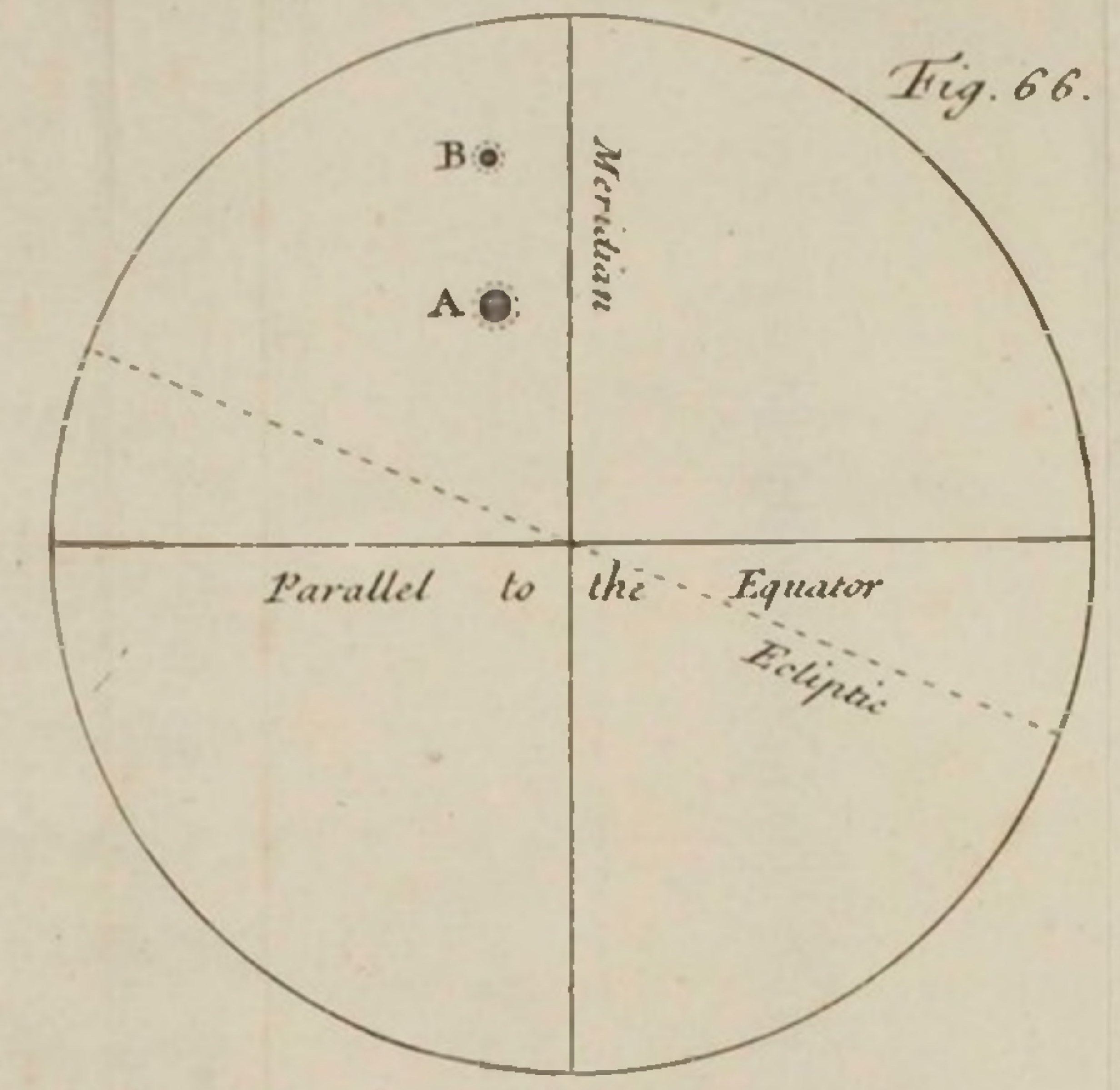


Fig. 66.

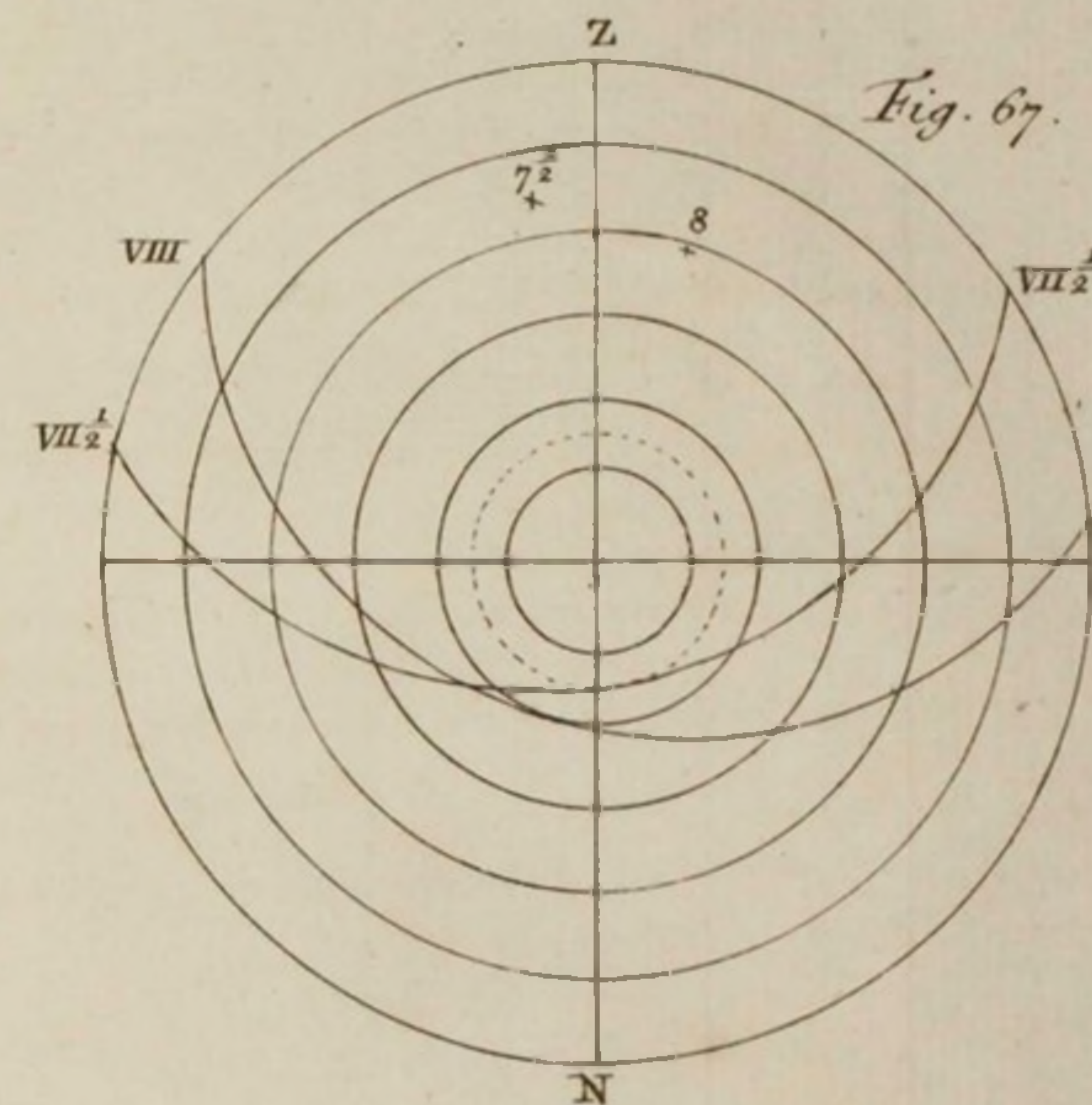


Fig. 67.

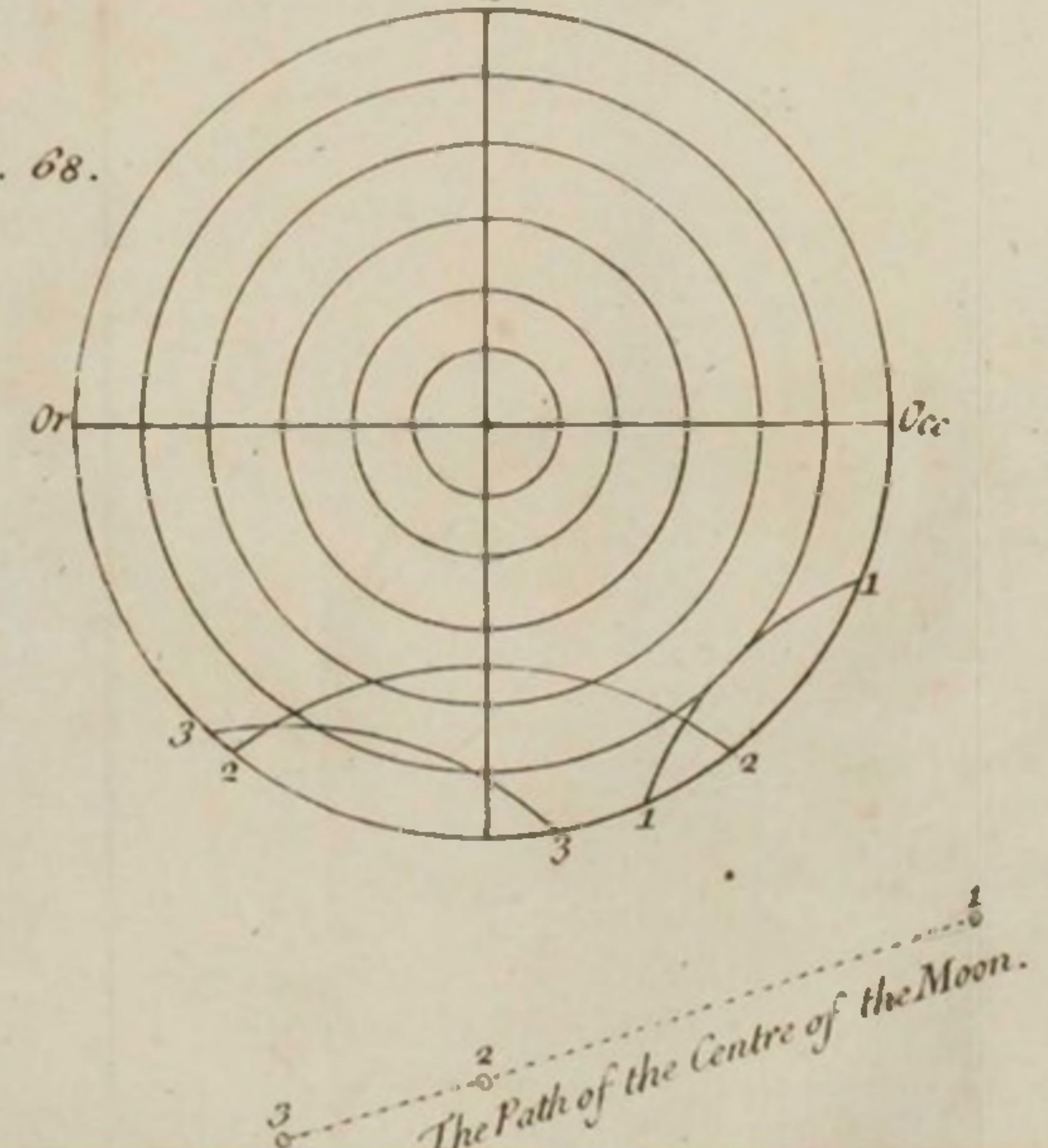


Fig. 68.

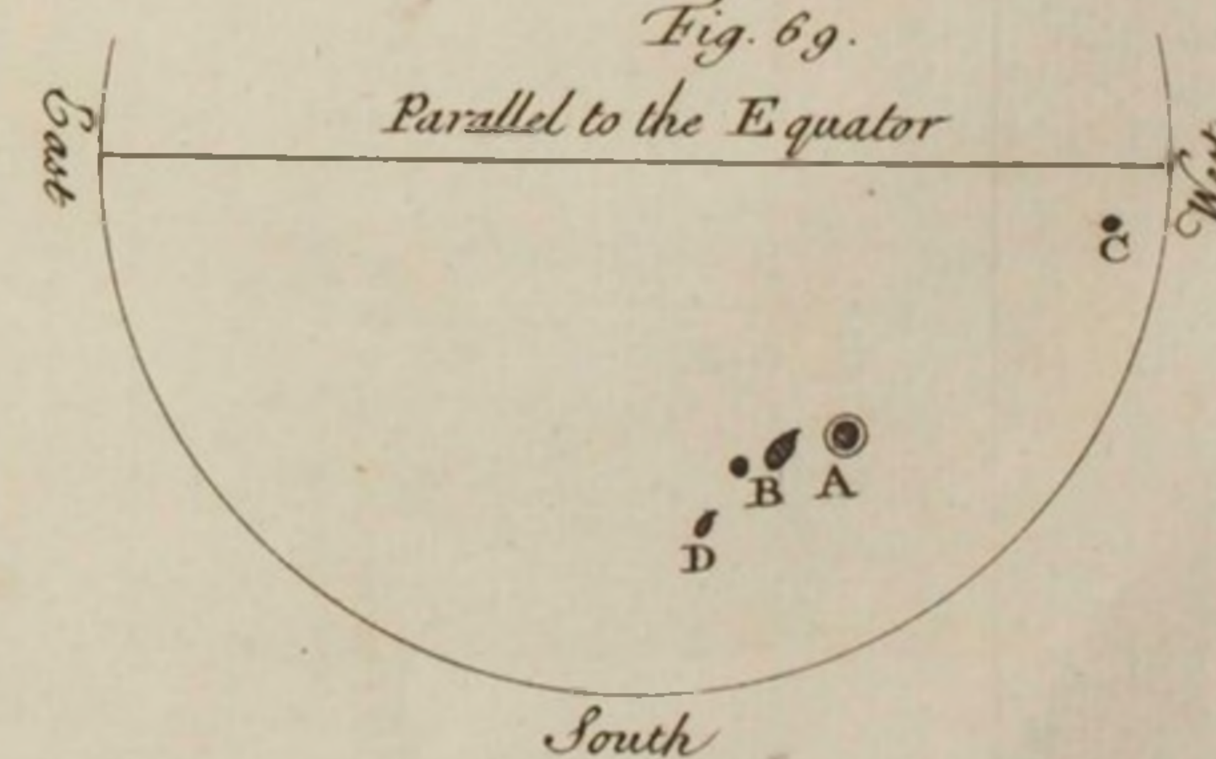


Fig. 69.

In the mean Time, the Spots of the Sun were covered and uncovered after the following Manner.

True Time. h ′ ″	
23 3 50	Spot C covered by the Moon, with a Tube of 8 Feet.
21 3	Spot A begins to be hid, with a Tube of 11 Feet.
21 49	The Centre of the Spot A is hid.
22 41	The whole Spot immerges.
23 54	The firſt of the 2 Spots at B begins to immerge.
25 10	The Centre of the ſame Spot is hid.
25 45	The whole Spot is hid.
26 24	The latter of the 2 Spots at B touches the Limb of the Moon with it's Centre. Hitherto I obſerved with the ſame Teleſcope of 11 Feet.
27 2	The Spot D begins to be hid, with the Tube of 8 Feet.
31 2	The whole Spot is hid with the ſame Tube.
0 31 45	The Spot A begins to appear on the Image of the Sun thrown on the Table.
32 30	The ſame Spot had entirely emerged, with it's Ring with the Tube of 11 Feet.
33 25	Emerſion of the Centre of the firſt of the two at B.
34 59	Total Emerſion of the ſame Spot.
35 51	Total Emerſion of the latter; all theſe with the ſame Tube of 11 Feet.

The Obſervations both of Spots and Digits were made by ſeveral other learned Men beſides *Zanotti*; and all obſerved the Time by the ſame Clock, which was afterwards corrected by Obſervations of the Meridian.

During the Eclipſe, I obſerved the Tranſit of the Moon over the Sun by the Plane of a mural Semicircle ſuſpended at the Meridian.

To determine the Tranſit of the Moon, I noted the Time, when a very ſmall Segment of the Diſk of the Moon, viſible upon the Sun, under the horizontal Thread of the Teleſcope, appeared to be biſected by the vertical Thread; for then the very Centre of the Moon muſt have been on the vertical one. But the Centre of the Moon paſſed over the Centre of the Sun at $23^h\ 59'\ 26''$. p. m. The Meridian Altitude of the Northern Limb of the Moon was $59^\circ\ 36'\ 15''$; of the Northern Limb of the Sun, $59^\circ\ 53'\ 0''$.

Eclipse *of the* Sun *observed at* Wittemberg *in* Saxony, July 24, 1739. *by* Joh. Frid. Weidler. No. 454. p. 226. July, &c. 1739. Fig. 70.

VIII. Increasing Phases.

h	′	″	p. m.
4	15	30	Beginning.
	22	00	dig. 1
	29	30	2
	35	30	3
	40	00	4
	47	30	5
	55	40	6
5	2	00	7
	9	00	8
	24	40	9

Decreasing Phases.

h	′	″	p. m.
5	35	30	dig. 8
	43	40	7
	50	30	6
	56	00	5
6	2	45	4
	8	40	3
	14	00	2
	20	45	1
	27	20	End.

Observation of the Immersion and Emersion of the Spots, which were conspicuous on the Disk of the Sun at the Time of the Eclipse. Fig. 71.

Immersions.

h	′	″	
4	34	35	Appulse of the Moon to the Spot *a*.
	34	45	The whole Spot *a* is covered.
5	1	30	Appulse of the Moon to the Spot *d*.
	5	20	——————— to the Spot *e*.
	7	15	——————— to the Spot *b*.
	10	00	Total Immersion of *b*.
	16	30	Appulse of the Moon to the Spot *c*.
	18	00	Total Immersion of the Spot *c*.

Emersions.

h	′	″	
5	30	50	The Spot *b* begins to emerge.
	32	30	The Middle of the Emersion of *b*.
	34	00	Total Emersion of *b*.
	39	00	The Spot *c* begins to emerge.
	39	50	Middle of the Emersion of *c*.
	40	40	Total Emersion of *c*.
	41	00	The Spot *a* begins to emerge.
	41	40	Total Emersion of *a*.
6	4	30	Emersion of *d*.
	6	15	Emersion of *e*.

Fig. 70. Shews the Disk of the Sun in the Situation in which it appears thro' the Helioscope.

Fig. 71. Represents the Spots of the Sun in that Situation which they had about the Beginning of the Eclipse; of which the Immersion and Emersion were observed during the Eclipse.

The Moon came upon the Sun at about 102° from the Zenith; and went off at about 53° from the same Zenith.

At the Time of the greatest Darkness the Orb of the Moon did not appear

appear quite black thro' the Telescope, but tinged with red; but the Spots of the Moon were not distinguishable.

The Edge of the Moon on the left Side toward the South, about the Time of the greatest Darkness, shewed the Tops of it's Mountains, which were also perceivable in the Image painted by the Telescope. The rest of the Edge appeared even.

During the whole Eclipse, the Circumference of the Moon appeared naked, without any Mist or Cloud, which sometimes hang over it in other Eclipses. But about the End, when one Digit about the Disk of the Sun was still hid, there was a vehement Motion of the Solar Light on the rough Edge of the Moon.

In the last Place, I must not omit, that a Friend of mine very skilful in these Affairs, who viewed the Sun thro' a Telescope of 9 Feet, about $4^h\ 31'$ observed a Light in the dark Disk of the Moon resembling Lightning; and that the same Observer about $5^h\ 50'$, affirmed to all the Company, that he saw again 3 Times such Coruscations breaking out on a sudden.

IX. The Observation was made with a reflecting Telescope of 16 Inches Focus, that magnified about 40 Times. Eclipse *of the* Sun, Dec. 19. 1739. *in the Morning, observed by Mr* Short *in* Surrey-street.

The Beginning could not be seen for Clouds about the Horizon.

About $35'$ after 8 o'Clock, there was an Opening, when the Sun seemed to be about 2 or 3 Digits eclipsed.

End was exactly observed at $9^h\ 1'\ 45''$, Time apparent.

No. 459. p. 633. Jan. &c 1741.

X. A Projection of the Arches and Circles, conceived upon the illuminated Hemisphere of the Earth, upon a Plane, may serve very well to shew any Eclipse of the Sun; and if the Places situated on the Surface of the Earth, as Cities, Shoars, Islands, &c. are inserted in the Projection, and if a Circle is added, to express the Position and Magnitude of the Lunar *Penumbra*, and some smaller Circles concentrical with it, we have then in one View those Places, where the Sun is covered by the Moon, and where any Part of it is withdrawn from our Sight. *An Instrument to represent* Eclipses *of the* Sun, *by* J. And. Segner, *Med. Phys. and Math. Prof.* Goetting. *and F. R. S.* No. 461. p. 781. Aug. &c. 1741.

But such an Image is momentary, and as it shews with great Accuracy what happens at any precise Point of Time; as for Instance, when the Centre of the Lunar *Penumbra* first enters the Disk of the Earth, it cannot exhibit the other *Phænomena*, which depend, partly on the Rotation of the Earth, partly on the Motion of the Moon. Thus if we would exhibit in this Manner all the Appearances of an Eclipse, as they succeed each other, we must delineate a great Number of Projections; which would be an Affair of infinite Labour, and would hardly be recompensed by the Pleasure expected.

Whilst the Earth turns round, the Circles of Latitude indeed, and consequently the Projection of them, remain the same; but the Meridians, or Circles of Longitude, are continually changed, and consequently

sequently the Projection of them, and the Situation of the Places of the Earth, so far as depends upon them.

But the artificial Globe of the Earth, shews the Hemisphere illuminated by the Sun at any Point of Time, with very little Trouble. For the Pole being elevated above the Horizon, or depressed below it, so that the Elevation or Depression may be equal to the Declination of the Sun at that given Time; or, which comes to the same End, the Sun's Place being put in the Ecliptic of the Globe in it's Zenith, the artificial Horizon becomes the Boundary of the Light and Shade; for it distinguishes the illuminated Hemisphere of the Earth from the dark one, and nothing remains to exhibit plainly the illuminated Hemisphere, but to turn the Globe round upon it's *Axis*, till it obtains the Situation which the Hour of the Day requires.

Thus what is very difficult in Projections, is with great Ease performed by the Globe, and also more conformably to Nature. When I considered this, I found we still wanted, in order to represent all the *Phænomena* of any Eclipse of the Sun, to project the Lunar *Penumbra* upon the Globe, and to make an Instrument, to represent the Situation of it at any Time, and to refer it to those Places of the Earth which are marked upon the Globe. By which Facility of doing the Thing, I was induced to think of such an Instrument; and accordingly I have attempted to execute it after the Manner represented
Fig. 72. in *Fig.* 72.

It is a common terrestrial Globe, furnished with it's Horizon, Meridian, and Hour Circle. To the Horizon are fastened two wooden Arms, A B, *a b*, in Length a little exceeding the Semidiameter of the Globe; one End of each of these Arms, is made to embrace the Horizon, and may be fastened to any Part of it by Means of Skrews, one of which is shewn at D.

On the opposite Extremities B *b*, are placed wooden Columns, perpendicular to the Horizon B E, *b e*, of the Height of the Semidiameter of the Globe, and of the Breadth of the Brazen Meridian, so that a right Line being drawn thro' the Tops of the Columns cannot touch the Meridian.

On the Top of each Column is a little Ball of Brass; each of these Balls is perforated by an Iron *Axis*, appearing on both Sides, and firmly joined to the Ball. The lower Parts of the *Axes* were fixed into the Columns, so that the Balls are held fast in a Situation parallel to the Horizon of the Globe.

The upper Parts of the *Axes* are round and polished, as well as the upper Surfaces of the Balls; and receive round Plates of Brass E F G, *e f g*, which rest upon the Balls in such a Manner, that being turned round the *Axes* they always remain parallel to the Plane of the Horizon. The Plates are about 3 Inches in Diameter, and each of them has a Notch in the Circumference, to receive a Thread. The Plate *e f g*, is something less than the Plate E F G; and this Difference

rence in Magnitude is no Injury to the Instrument. Besides it has nothing Particular in it; and therefore it is only fastened with a Ball to keep it from falling off the *Axis*.

But the other Plate E F G has a Circle inscribed upon it, divided into Degrees, and an Index H is added, to shew the Number of those Parts. This is so situated, as to turn round the *Axis* without moving the Plate, or being affected itself by any Motion of the Plate. In order to this, a little immoveable Ball is placed between the Plate and the Index, for the Index to turn round upon it any Way.

Then there are three Rays of Brass, *i k*, *i l*, *i m*, connected in *i*, containing equal Angles *k i l*, *l i m*, *m i k*; and the Plane *i* is perforated with a very small Hole. The Rays are elastick, and as thin as could be made to be firm, and nearly of the Length of ¼ Part of the Globe. The Rays have also little Perforations at *l* and *m*, thro' which a Thread being drawn is brought round the Plates by *m* F G *g f e l*, the Ends being fastened together between *l* and *m*, wherefore the Skeleton of the *Penumbra* is also rendered immoveable at the Part of the Thread *e l m* E, it's third Ray lying freely on the Part of the Thread *g* G; hence the Skeleton is turned either away in a right Line, upon the Turning of the Plate E F G, or *e f g*.

By this Construction might be discovered how many Parts of the Division of the Plate E F G would answer to the Diameter of the Globe, after this Manner. The Arms A B, *a b*, are so placed, that upon the Skeleton's being moved, it's Centre *i* would run thro' the Diameter of the Globe; and to effect this, the Horizon of the Globe is placed in a Situation parallel to the Horizon of the Earth, and a Pendulum *i n* is let fall from that Centre, to shew the Points of the Horizon, over which the Centre would hang. Therefore moving the Centre forward, according to the whole Length of the Diameter of the Globe, we might note the Number of Parts of the Plate E F G, which have passed in the mean Time thro' the Index H; which being carefully observed, must be retained in Memory, since the Use of it, as well as of every Thing that has hitherto been described, will occur in the Representation of all Eclipses. These that follow must be changed according to each particular Eclipse.

The Principal of these is the Disk of the *Penumbræ*, which I have endeavoured to effect after the following Manner. Having found, by the Tables for the Eclipse which I would represent, the Semidiameter of the Lunar *Penumbra* on the Disk of the Earth, as also the horizontal Parallax of the Moon, I argued thus; as the horizontal Parallax of the Moon is to the *Radius* of the penumbrous Disk, so is the Semidiameter of the terrestrial Globe, that I made use of, to the Quadrant, which expressed the *Radius* of the *Penumbra* required by the Magnitude of the Globe.

As the Size of the Instrument seemed not to admit of a Division into 12 Parts, I divided that *Radius* into 6, and described concentri-

cal

cal Circles on a thicker Paper, which I cut into *Armillæ* according to them. I pasted the biggest of these to the Skeleton *k l m*, so that the Centre might agree with the Centre of the Skeleton *i*; then I rejected the second, and pasted the third to the Skeleton in the like Manner, and rejecting the fourth, I pasted on the fifth, rejecting also the inner Circle; so that the Figure might arise, as it is described between *k l m*. The Use of it is to shew, that all the Places marked upon the Globe, which lie under the outer Edge of the greatest Circle, see the Beginning or End of the Eclipse, that those which are situated under the inner Edge of the same Circle, see 2 Digits eclipsed; that those which lie under the outer Edge of the second Circle have an Eclipse of 4 Digits, and so on; but that those which lie under the Centre see the Eclipse total; for I have thought it sufficient to mark the Shadow, because of it's Smallness, thro' the very Centre.

To set every Thing in order for any Moment of a given Eclipse, we must proceed in the following Manner. Having found by Calculation the Points of the Bound of Light and Shadow, by which the Centre of the Moon first enters the Disk of the Earth, and again departs from it, they are to be marked on the Horizon of the Globe, and the Arms A B, *a b*, are to be placed so that the Plate E F G being turned round, the Centre *i* of the Disk of the *Penumbra k l m* may pass over them; and whether this is done or not, will be shewn by the Pendulum *i n*. Then I find the Time when the Centre of the *Penumbra* is in any remarkable Place, as when it first enters the Disk of the Earth, and place the Globe, by means of the Meridian and Equator, without the Help of the Hour Circle, in such a Manner, that the Part above the Horizon may shew the Hemisphere of the Earth at that Time illuminated by the Sun. I then turn the Plate E F G till the Centre of the *Penumbra i*, is perpendicularly over that remarkable Place, as the Bound of Light and Shadow, for Example; which I call the primary Situation of it, and this being obtained, I move the Index H of the Plate to the Beginning of the Division. Thus every Thing is rectified for this Time, and it's *Phænomena* may be collected.

Now the horary Motion of the Moon from the Sun, being taken from the Tables, I infer, that as the horizontal Parallax of the Moon, is to this horary Motion of the Moon; so is the Number of Parts of the Plate E F G, which answers to the Semidiameter of the Globe, found above, to the Quadrant, which shews how many Parts, upon turning the Plate round, are to be drawn thro' the Place of the Index, that the Situation of the Disk of the *Penumbra* may be had, an Hour before or after the Time, which answers to the primary Situation. The Disk therefore being brought to this Place, and the Globe being turned round the Axis, the *Phænomena* of this Time may be had in like Manner.

Now

Now the Situations of the other Times are easily obtained. For the Number of Parts of the Plate just found being divided, namely that which answers to the horary Motion, in order to obtain the Motion of half an Hour, a Quarter of an Hour, and a Minute, a Table may be constructed only by Addition and Subtraction, in which having marked the Times, the Parts are put to the Plate, by which the Disk of the *Penumbræ* ought to be moved forward and backward, that it may receive the Situation accommodated to that Time. When this is done, it remains only to turn the Globe according to the Time, and the Plate in such a Manner, that it's Index may shew the Number ascribed to the Time.

Lastly the Places marked upon the Surface of the Globe, lying perpendicularly under the Disk of the *Penumbra* in any Situation of it, may be found by the Pendulum. But they are seen at one View, if the whole *Apparatus* is exposed to the Rays of the Sun reflected from a plain *Speculum*, in such a Manner, that the Rays may fall perpendicularly upon the Horizon of the Globe. For then such Shadows will be projected from the Disk of the *Penumbræ* upon the Globe, as are like the *Penumbræ* which the Moon casts upon the Earth, by which the Phases of the Eclipse, for any Place may be seen.

This Motion of the Sun is inconvenient; perhaps those who have a large burning Glass, will make Use of a Lamp, the Rays of which may be thrown upon the Globe from the Glass, in a Position perpendicular to it's Horizon. I have thought also of viewing the Globe from a Distance thro' a Perspective Glass, by which Method the Disk *k l m*, being brought upon the Surface of the Globe, exhibits the *Penumbra*. But this requires a very large Telescope; for if the Globe is set at such a Distance, that the whole may be seen thro' a small Telescope, I am afraid the Places marked upon it will not be distinguishable.

I have thought also of giving a Motion to the Machine, by means of two separate Clocks, one of which might turn the Globe, and the other the Plate; and these might be brought to agree exactly by the Help of Pendulums. But I have said enough already on this Subject.

Eclipse of the Moon, Nov. 20, 1732. *observed at* Rome *by the Abbots* Didacus Revillas, *and* Jo. Bottarius, *and by* Eustachius Manfredi. No. 428. p. 85. April, &c. 1733.

XI. 1. The Observations were made with a Telescope of 10 Palms. True Time.

h	′	″	p. m.
8	45	28	The Penumbra begins to be sensible.
	49	14	The Penumbra thicker.
	51	19	Beginning of the Eclipse.
		44	Grimaldus begins to immerge.
	52	47	All Grimaldus hid.
		54	Galilæus.
	53	48	The Shadow at Gassendus.
	56	2	All Gassendus hid.
	57	23	Schikardus.

True Time

h	′	″	p. m.
9	2	43	Kepler.
	4	53	All Ariſtarchus hid.
	5	0	Lanſbergius, and almoſt all the Mare Humorum hid.
	6	13	Bullialdus.
		53	Capnanus.
	7	8	The Shadow at Mare Nubium.
	8	2	Copernicus begins to immerge.
		29	Thro' the Middle of Copernicus.
	10	27	The Shadow at Eratoſthenes ; and all Copernicus hid.
	14	12	Tycho begins to immerge.
		45	*Inſula Sinus medii.*
	15	37	Heraclides.
	16	22	Tycho hid.
	18	12	Tymocharis.
	20	4	Archimedes.
	21	4	Harpalus.
	23	10	Manilius.
		16	Helicon.
		40	Plato.
	26	21	Menelaus.
	28	55	Catharina and Cyrillus.
	30	11	Pliny.
		56	Dionyſius.
	32	31	Ariſtotle.
	33	11	Promontorium acutum.
	34	27	Fernelius.
	34	51	Snellius.
	36	11	Poſſidonius.
		41	Petavius.
	37	45	Promontorium Somnii.
	38	25	Langrenus.
	40	24	Hermes.
	41	0	Proclus.
		30	Mare Criſium begins.
	42	32	Cleomedes.
	45	10	The Shadow thro' the Middle of *Mare Criſium.*
	46	20	Meſſala.
	48	24	The total Immerſion.
0	57	5	Duration of the total Immerſion.
11	31	13	The Emerſion had without Doubt begun.
	33	13	Grimaldus had emerged.
	46	3	The Middle of Copernicus.
	51	17	Tycho.
		52	Plato.
	53	13	Archimedes.

Inſula

True Time.

h ′ ″	p. m.
11 56 36	*Insula Sinus Medii.*
59 57	Eudoxus.
12 2 10	Manilius.
3 26	Aristotle.
4 25	Menelaus.
8 11	Possidonius.
13 6	Pliny.
17 14	Promontorium acutum.
20 38	Langrenus.
23 21	The whole *Mare Crisium.*
26 55	End.
3 35 36	Duration of the whole Eclipse.

Some Phases of the Immersion taken by another Observation with a Newtonian Telescope.

True Time.

h ′ ″	p. m.
8 50 13	The Penumbra thick.
11 28	The certain Beginning of the Obscuration.
54 8	All Grimaldus hid.
32	The Shadow thro' the Middle of Galilæus.
9 0 58	All Kepler hid.
2 18	The Shadow at Aristarchus.
3 37	All Aristarchus hid.
8 3	The Shadow at the Beginning of Copernicus.
9 20	Thro' the Middle of Copernicus.
10 32	All Copernicus is covered.
14 47	The Shadow at the Beginning of Tycho.
23 11	At the Beginning of Manilius.
26	At the Beginning of Plato.
55	The Shadow thro' the Middle of Plato and Manilius.
24 40	All Plato is covered.
39 35	The Shadow at the Beginning of Proclus.
40 18	The Shadow at Hermes.
41 0	All Proclus is covered.
31	At the Beginning of *Mare Crisium.*
44 20	Thro' the Middle of *Mare Crisium.*
46 15	All *Mare Crisium* is shaded.
49 3	The total Immersion of the Moon in the Shadow.

	h ′ ″ apparent Time.
2. The Beginning at — — — —	8 1 30
Immersion — — — — — —	8 59 30
Emersion — — — — — —	10 38 0
End — — — — — — — —	11 37 0

— *Observed in* Fleet-street, London, *by Mr.* Geo. Graham, *F. R. S.* Ibid. p. 82.

Observed with a small Telescope about 18 Inches long, which magnifies about 13 times.

N. B. Mr *Hodgson* at *Christ's-Hospital*, with a 4 Foot Telescope, observed the Beginning at 8h 1' ½ and the End 11h 36' ½.

Eclipse of the Moon, Oct. 2, 1732. Styl. Nov. *observed at* Wittemberg *in* Saxony, *by* Jo. Frider. Weidler, *F. R. S.* No. 443. p. 359. Oct. 1736.

XII.

h	'	''	*Temp-europ. ante Mer.*
0	44	30	The Penumbra near Schikard.
	59	0	Beginning of the Eclipse.
1	1	30	The Shadow touches Schikard. The Edge of it is rough and unequal. Soon after the Clouds hide the Moon.
1	15	0	Tycho is quite shaded. The Moon again covered with Clouds.
1	25	30	The obscured Portion of the Moon is blackened, and the Spots cannot be discerned thro' the Shade by a Telescope of 9 Feet.
1	30	0	The Shadow touches Grimaldus. Now the Spots are seen thro' the Shadow.
1	44	30	The Shadow covers all Grimaldus. Now the shaded Portion is red. The Moon is again covered with Clouds.
2	25	30	The Shadow receding touches Lansbergius. It's Edge is still rough.
2	44	0	The Shadow touches Gassendus.
3	11	0	Tycho begins to emerge.
3	36	0	The End upon Snellius, the Sky being clear round the Moon.

Eclipse of the Moon observed by Mr Geo. Graham *in* Fleetstreet, March 15. 1735-6. No. 445. p. 14. Jan. &c. 1737.

XIII. 1.

h	'	''	
10	13	0	The Beginning.
11	11	0	The total Immersion.
12	49	0	The Emersion.
13	47	0	The End.

— *Observed by Dr* Halley *at* Greenwich, Ibid.

	h	'	''
2. The Beginning — — — — — — — —	10	13	37
The Immersion. — — — — — — —	11	9	42

— *Observed at Mr* Graham's *house in* Fleetstreet, *by Mr* Celsius. Ibid.

3. The Observation was made with a reflecting Telescope of 4 Inches, made at *Edinburgh*, and magnifying 63 times.

h	'	''	
10	22	5	The Shade on the Middle of Kepler.
	23	15	Entering the Mare Humorum.
	28	16	Entering on Copernicus.

Entering

h ′ ″	
10 29 34	Entering the Middle of Copernicus.
30 26	Copernicus entire.
33 28	Enters on Timocharis.
38 44	Enters on Tycho.
39 12	The Middle of Tycho.
40 48	Tycho entire.
46 0	Enters on Menelaus.
49 20	Plinius.
11 0 40	Enters on *Mare Crisium.*
5 36	*Mare Crisium* entire.
9 17	The total Immersion is about to begin.
13 13 55	Tycho is emerged out of the Shade.
29 0	Mare Serenitatis is totally emerged.
40 45	*Mare Crisium* is totally emerged.
45 50	The Eclipse is nearly ended.
46 12	The Eclipse is certainly ended.

4.

True Time.

— Observed in Covent-Garden, by J. Bevis, M. D. Ibid. p. 16. Fig. 73.

h ′ ″	p. m.
6 53 47	Saturn in the Point where the Threads of the Micrometer cross.
7 31 5	First of the Hyades at δ passes the Thread *a*.
7 31 50	It passes over the Thread *b*.
7 32 35	It passes over the Thread *c*.
7 42 39	Saturn again in the Intersection of the Threads.
8 19 57½	First of the Hyades at δ passes the Thread *a*.
8 20 42½	It passes the Thread *b*.
8 21 27½	It passes the Thread *c*.
9 50 0	The Disk of the Moon runs over the horary Thread, 139 horary Seconds.
9 56 0	Again 139″.
10 1 0	Again 139″.
10 9 40	A thin Penumbra seems to cloud the Moon near Hevelius.
10 10 20	Now very sensible.
10 11 40	I reckon the beginning of the Eclipse.
10 14 38	The Edge of the Shadow, as I think, passes thro' Grimaldus and Cavalerius.
10 19 46	Thro' Aristarchus.
10 24 15	The Shade enters the Mare Humorum.
10 32 44	It touches the Sinus Roris.
10 40 18	The Shade divides Tycho.
10 42 26	It touches the Mare Serenitatis.
10 46 1	It touches Menelaus, a black Cloud comes over.

—

True Time.	
h ′ ″	p. m.
10 53 46	On the Departure of the Cloud the whole Mare Nectaris was found covered. Very thick Clouds obscure the Moon again.
11 0 56	The Shadow touches *Mare Crisium.*
11 5 48	*Mare Crisium* and *Mare fœcundum* are immerged.
11 10 0	The total Immersion of the Moon into the Shadow.
12 42 20	The eastern Limb of the Moon grows clear.
12 46 5	It grows still more clear.
12 47 56	A Thread of pure Light is restored in the twinkling of an Eye. Many light Clouds.
12 57 5	The Edge of the Light touches the Mare Humorum.
13 4 3	The whole Mare Humorum is recovered.
13 13 40	Tycho is half covered.
13 14 0	Quite uncovered.
13 17 22	Waltherus emerges. Many dark Clouds, which seem likely to last some Time.
13 43 44	Mare fœcundum is seen out of the Shadow.
13 46 25	The true Shadow ends.
13 48 30	The *Penumbra* no longer sensible.

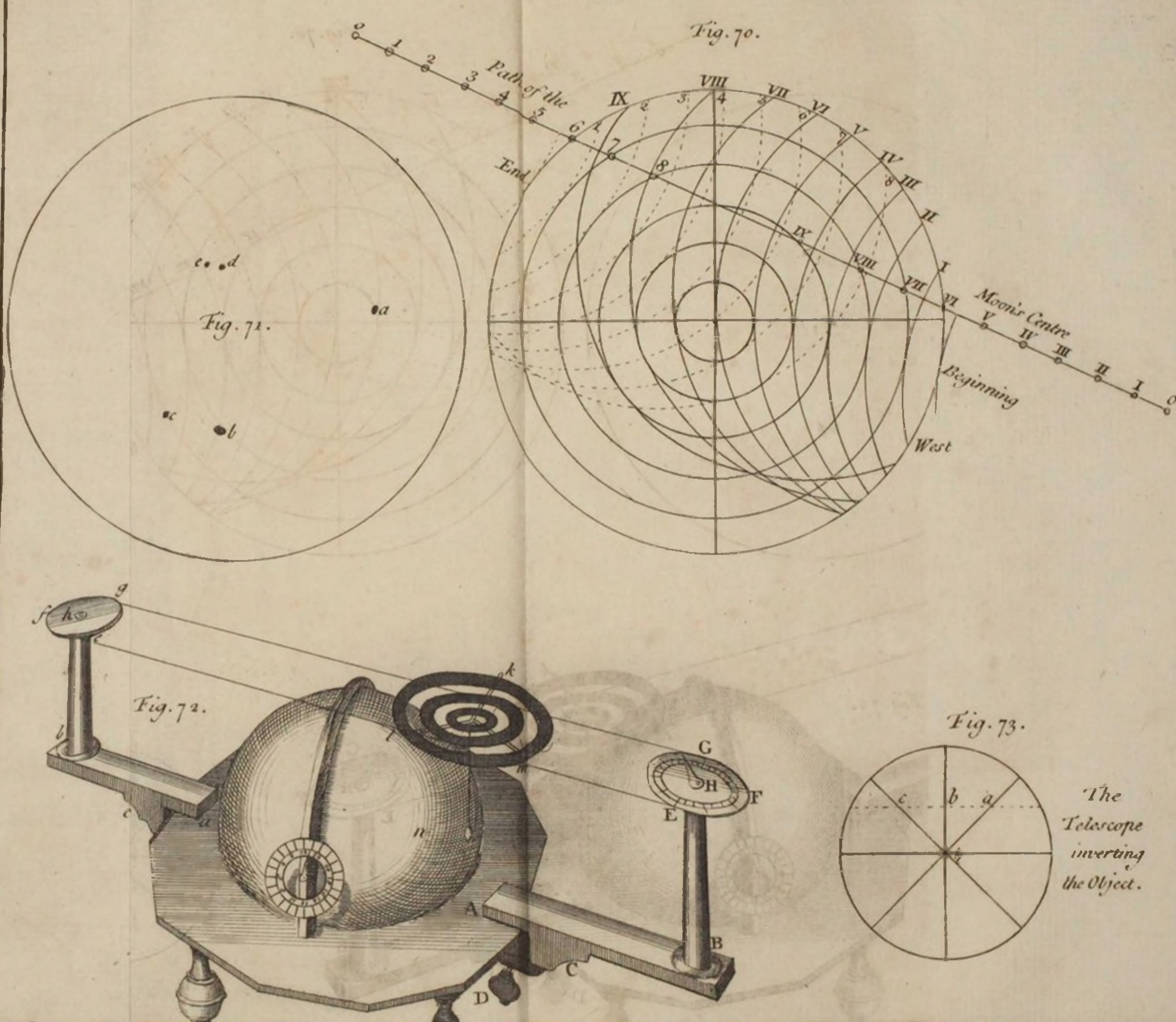
In these Observations I made Use of a very good Clock, corrected by 5 corresponding Altitudes of the Sun this very Day, and several Days before, and a Telescope 6 Feet long. About the middle of the Obscuration, the Moon appeared as thro' a darkish Cloud, but at the Edges it was red like hot Iron. The Limit of the Light and Shade was not well determined thro' the whole Eclipse.

— *Observed at* Yeovil *in* Somersetshire, *by Mr.* John Milner.

5. The Latitude of *Yeovil* is 50° 52′. The Clock was first adjusted by the Equation Table.

	h	′	″	dig.	′
The Beginning — — — — — — —	10	6	0		
The Moon's Altitude then — — — — —				34	29
The Beginning of total Observation — —	11	4	30		
The Altitude then was — — — — —				34	16
The Middle — — — — — — —	11	54	0		
The End of total Obscuration — — — —	12	43	30		
The Altitude then was — — — — —				34	47
The End — — — — — — — —	1	39	15		
The Altitude then was — — — — —				31	47
The Continuation of the total Obscuration was —	1	39	0		
The Duration of the whole Eclipse — — —	3	33	15		

Pla. XII. Vol. VIII. part 1. pa. 166.



XIV. 1.

Eclipse of the Moon, Sept. 8. 1736, observed in Fleetstreet, London, *by Mr* Geo. Graham, *F. R. S. and by Mr.* James Short *of* Edinburgh, *F. R. S.* No. 466. p. 92.

	h	′	″	Appar. Time.
Beginning of the Eclipse — — — —	12	58	0	
The Shadow touched Grimaldi — — —	13	0	0	
touched Kepler — — —		9	30	
touched Copernicus — —		17	10	
touched the East Side of Tycho		25	5	
touched the East Side of Plato		34	30	
touched the East Side of Manilius		36	40	
touched the E. S. of *Mare Crisium*		56	20	
Beginning of total Darkness — — —	14	3	45	

The Observation made with a 5½ Inches reflecting Telescope, magnifying about 38 times.

2. The Observation was made with a Telescope of 5 Feet.

— *In* Covent-Garden, London, *by* J. Bevis, *M. D.* Ibid.

Apparent Time.

h	′	″	
12	53	25	The Penumbra touches the North East Limb. Clear.
	54	25	Now very conspicuous. Clear.
	56	50	The true Shadow, as I judge, touches the Limb. Clear.
	57	30	The Shadow touches Grimaldi. Clear.
13	0	25	Grimaldus covered. Clear.
	7	23	It enters the Mare Humorum thro' thin Clouds. Clear.
	28	39	The Shadow touches the Mare Vaporum. Clear.
	31	19	The dark Part of the Moon is of a reddish Colour. Very clear.
	36	53	The Limit of the Shadow bisects Manilius and touches Mare Serenitatis. Very clear.
	38	48	It touches Mare Tranquillitatis. Clear.
	47	21	Mare Serenitatis is covered. Clear.
	55	26	It touches *Mare Crisium*. Clear.
	58	5	Mare fœcunditatis is covered.
14	2	25	Total Immersion of the Moon. Very thick Clouds come over and hide the Moon.
16	43	0	Mare Tranquillitatis seems quite uncovered thro' a Gap of the Clouds.
	43	30	Clouds again.
17	3	22	The Cloud going off, the Moon seems to be free from all Obscurity.

The Clock was fitted to true Time by equal Altitudes of the Sun; and it's Agreement with Mr *Graham*'s Chronometer was marked by a very good Watch.

3. The

——*At* Wittemberg, *obſerved by* J. Frederick Weidler. *F. R. S.* Ibid. p. 94.

3.

h	′	″	
1	36	0	The Penumbra comes upon the Eaſt Part of the Moon, like a Miſt or Smoak.
1	50	0	Beginning.
1	50	30	The Shadow touches Grimaldi.
1	52	0	——————— Galilæus.
2	0	0	——————— Kepler.
2	1	30	——— covers all Kepler.
2	7	0	A Portion of the Lunar Diſk, immerged deeper into the Shadow, appears clearer than that which was nearer the Edge of the Shadow.
2	8	0	The Shadow touches Copernicus.
	10	50	——— covers all Copernicus.
	16	10	——— touches Tycho.
2	20	0	Half of the Moon darkened.
	25	0	The Shadow touches Mare Serenitatis.
	29	10	——————— Menelaus.
	36	0	All Mare Serenitatis covered. The Moon looks red thro' the Shadow like a Coal of Fire.
	45	30	The Shadow at Mare Criſium. At this Time the Edge of the Shadow is bent inwards about Mare Criſium; and during the whole Eclipſe, the Circumference of the Shadow appeared rough and rugged, and ſeemed in the extreme Part to be ſurrounded with a Sort of light Smoak.
2	50	0	All Mare Criſium ſhaded.
	53	0	Total Darkneſs, Now about $\frac{1}{3}$ of the Lunar Diſk towards the Eaſt appears darker than the Weſt part.
3	43	0	The Shadow darker in the Middle, but paler about the extremities.
4	8	0	The Moon is covered with Clouds.
	44	0	Emerſion of the Moon out of the Shadow.
	45	0	The Shadow leaves Grimaldi. After this the Moon was hid by Clouds, out of which it now and then emerged, but a Miſt or thin Cloud ſhaded it ſo, that the Spots could not be diſtinguiſhed. At Length the whole Moon was hid by thick Clouds.

The Obſervation was made with a Teleſcope, 8 Paris Feet long.

— Obſerved in Hudſon's-Bay, *by Capt.* Chriſtopher Middleton, *F. R. S.* Ibid. p. 96.

4. I made the Obſervation in *Hudſon's-Bay*, in Lat. 55° 34′ N. and on the Meridian of the *North-Bear Iſland*, which lies 30 Miles to the Weſtward of *Charlton*. The Weather was very clear, but the Motion of the Sea rendered my Teleſcope uſeleſs, and I miſſed the Beginning.

I The

	h ′
The total Immersion of the Moon's Body into the Shadow — — — — — —	8 22 by my Watch.
The Emersion — — — — — —	10 8
The End — — — — — — — —	11 16

In order to rectify my Watch, and be certain of the true Time, I took three several Altitudes next Morning, and one in the Afternoon, by Mr *Hadley*'s and Mr *Smith*'s Quadrants; which (having made proper Allowances for the Refraction of the Atmosphere and the Height that I stood above the Surface of the Sea) were as follows.

	° ′		h ′
First Altitude	23 0	Hence the true Time is —	8 49 —
Latitude —	55 45	The Time by my Watch —	8 28 —
		Watch too slow — —	0 21 —
Second Altitude	25 48	The true Time therefore is —	9 15 —
Latitude —	55 45	The Time by my Watch —	8 54 —
		Watch too slow — —	0 21 —
Third Altitude	26 44	The true Time therefore is —	9 24 —
Latitude —	55 45	The Time by my Watch —	9 3 —
		Watch too slow — —	0 21 —
The fourth Altitude taken in the Afternoon the same Day —	21 29	Hence the true Time is — —	3 25 +
Latitude —	55 33	The Time by my Watch —	3 4
		Watch too slow — —	0 21 +

If 21 Minutes therefore be added to the Times above-mentioned, for the Error of the Watch, we shall have the true Times of the several Observations on the Meridian of the *North-Bear Island*, as follows, *viz.*

	h ′
The total Immersion of the Moon's Body into the Shadow —	8 43
The Emersion — — — — — — — — — —	10 29
The End — — — — — — — — — — —	11 37

This same Eclipse was observed by Dr *Bevis* at *London*, and he made the true Time of the total Immersion of the Moon's Body into the Shadow, 14^h, $2'$, $25''$; consequently the Difference of Longitude between *London* and *North-Bear Island* in *Hudson*'s-*Bay*, is 5^h, $19'$, $25''$, or 79°, $51'$.

An Eclipse *of the* Moon, Dec 21, 1740. *at the Island of* St Catharine *on the Coast of* Brasil, *observed by the Hon.* Edw. Legge, *Esq; Captain of his Majesty's Ship the* Severn. No. 462. p. 18. Read Jan. 21, 174½.

XV. 1. It began very nearly at $7^h\ 5'$; but the Horizon being hazy, I could not observe exactly the Beginning: However, it ended exactly to a Moment, at $9^h\ 50'$. I set my Watch by two Observations before, that I might be exact in Time, and confirmed it by one after; so that I believe I may venture to say it was right: And I observed with one Telescope on board, and sent another on Shore, which agreed exactly together.

Remarks on the foregoing account by the Rev. Joseph Atwell, *D. D. F. R. S.* Ibid.

2. The Captain places the Island in Lat. 27° 30′. Mr *Gael Morris* has calculated the said Eclipse; and the Middle of it, apparent Time, at *Greenwich*, was,

	h	′	″
	11	44	50
By the Captain's Observat. supposing the the Beginning exact	8	27	30
Difference of Meridian — — — — — — — —	3	17	20
	= 49° 20′		
	h	′	″
The End of it, by Calculation at *Greenwich* — — —	13	6	57
——— by Capt. *Legge*'s Observation — — —	9	50	0
Difference of Meridian — — — — — — — —	3	16	57
	= 49° 14′		

Capt. *Legge* observes, that in attempting to pass Cape *Horn*, they thought themselves to have been more to the Westward than they really were: By which Mistake, turning too soon to the North, they fell in with high Lands, and met with those Misfortunes, which, if they had kept out more at Sea, might probably have been avoided. By comparing the Longitude at *St Catharine*'s as above settled, with *Senex*'s Maps, the Coasts appear to be placed about 6 Degrees too much Eastward; and if the other Parts of *America* about the Cape are laid down as faultily in the Charts, this Error will probably account for their Misfortunes.

— *At* Cambridge *in* New-England, *by Mr* John Winthrop Hollisian, *Prof. Math. and Astron. at* Cambridge *in* New England. No. 471. p. 577. Read Nov. 3, 1743.

3. *Dec.* 21, 1740.

h	′	
5	24	A plain Penumbra.
	35	The true Shadow seems to enter.
	47	Touches *Palus Maræotis.*
	53	Reaches *Mount Sinai.*

After this the Clouds thickened, and covered the Moon till the End of the Eclipse, which was about 8^h, $30'$, as near as I could guess through the Clouds.

The

The Night before the Eclipse, *viz.* 20 *December*, at 12^{h}, $14'$, I saw the Moon eclipse a fixed Star, which, I think, is in the Heel of *Castor*.

These two Observations were made with an eight Foot Telescope, my Watch being rectified to the apparent Time by correspondent Altitudes of the Sun, taken with the before-mentioned Quadrant for several Days together, before and after the Ecsipse.

XVI. In the Morning.

An Eclipse *of the* Moon, Jan. 2, 1741. *observed at the College at* Pekin, *by the* Jesuits. No. 468. p. 309. Jan. 1742-3.

h	′	″	
6	4	18	The Shadow was observed to have just touched the Edge of the Moon, with a Tube of 3½ Feet.
	5	40	The certain Beginning between *Vieta* and *Schikardus* with a Tube of 6 Feet.
	10	0	The Shadow at *Schikardus*.
	14	0	———————— *Mare Humorum.*
	16	0	———————— Grimaldi.
	16	10	———————— Capnanus.
	17	20	Gassendus begins to be immerged. The Centre of Grimaldi in the Shadow.
	18	30	All Grimaldi immerged.
	19	0	The Shadow at Campanus.
	19	30	———————— Herigonius.
	22	30	———————— Tycho.
	23	10	———————— Bullialdus.
	24	0	Tycho immerged.
	24	20	The Shadow at *Pitatus*.
	32	0	———————— Galilæus.
	42	0	———————— Kepler.
	43	0	Reinholdus.
	55	0	Fracastorious is immerged. The Shadow at Copernicus
7	2	0	All Copernicus seems to be in the Shade.
	6	0	The Shadow at Wendelinus.
	10	20	The Centre of the Moon in the Shade.
	14	0	The Moon is hid behind Mountains at the Setting, before the Middle of the Eclipse.

The *Penumbra* was small and the Shadow very black and distinct, and the Eclipse might very plainly and clearly be seen 'till the Setting of the Moon. The Diameter of the Moon before the Eclipse was in the Micrometer about $30' 20''$, but at the Setting only $30' 00''$. The Centre of the apparent Disk was from the *Sinus Æstuum Occid.* a little towards *Hipparchus*.

An Eclipse *of the* Moon, Jan. 2, 1740. *observed at Mr* Graham's *House in* Fleetstreet, *by Mr* Short. No. 459. p. 633. Jan. &c. 1741.

XVII. The Observation was made with a reflecting Telescope of 9 Inches Focus, that magnified about 40 times.

	h	'	"	
Beginning about — — — — — —	8	25	00	Time appar.
Beginning of total Darkness at — — —	9	31	10	
End of total Darkness — — — — —	11	15	20	
End of the Eclipse at — — — — —	12	22	00	

N. B. The Beginning and End could not be distinctly seen for Clouds.

Eclipse of the Moon, Oct. 22, 1743, *in the Morning, observed at Mr* Graham's *House in* Fleetstreet. No. 471. p. 580. Read Nov. 3, 1743.

XVIII. The Sky was mostly overcast with Clouds, so that the following Observations are the only ones that could be made with any Degree of Certainty.

	h	'	"
Beginning of the Eclipse about — — — — —	1	21	0
The Shade touched Copernicus about — — — —	1	39	0
touched Plato about — — — — — —	1	45	0
touched Tycho about — — — — —	1	51	0
Total Immersion about — — — — — — —	2	17	0

Of the Lunar Atmosphere, by M. Jean Paul Grandjean de Fouchy, *of the R. Acad. at* Paris. No. 455. p. 61. Nov. &c. 1739.

XIX. By the Name of Atmosphere is understood, a certain Congeries of pellucid Matter involving a Planet, and capable of turning the Rays of Light, that pass thro' it, from a right Line; whether this Matter exists in our Air jointly, or separately from it, whatsoever it is, we treat here only of the refracting Matter, and that is what I only take upon me to prove in the Course of this Work, that there is no Matter about the Moon, which is able to turn the Rays of Light sensibly from their strait Course. I would inform the Reader only of this, that I here conceive the Atmosphere to be a homogeneous Fluid, with a spherical Surface, and of the same Density every where, which is equal to the Sum of the decreasing Densities in the real Atmosphere, purposely omitting the Difference of the Density of it's Parts, which cannot disturb our Demonstrations.

Fig. 74.

Now if the Moon is encompassed with an Atmosphere, it's Diameter ought to be found greater than in the naked Planet; but that the Quantity of it's Increase may be known, let AIB be the Body of the Moon, GEF it's Atmosphere, the Angle AHL will be the real Diameter of the Moon; and the Angle EHL comprehended under the *Axis* LH, and the Ray AEH will be the Diameter of the Moon observed. Therefore the Angle EHA will be the Increase of the Diameter of the Moon by it's Atmosphere, but the Angle EHA is opposite to the Side EA of the Triangle EHA; and the Angle AEH the Supplement to 180 of the horizontal

rizontal Refraction in the Lunar Atmosphere, is opposite to the Side A H of the Distance of the Moon from the Earth. Moreover the Side E A, is the Half of a Chord of the Lunar Atmosphere, touching the Body of the Moon itself in A. Therefore the Sine of the Increase E A H of the Diameter of the Moon by it's Atmosphere, will be to the Sine of the Supplement of the horizontal Refraction A E H, as the Half A E of the Chord of the Atmosphere touching the Body of the Moon at the Distance A H of the Moon from the Earth.

Hence it follows evidently, that this Increase of the Lunar Diameter is insensible; for if it arose to $2\frac{1}{2}''$, supposing the horizontal Refraction $5'$, that is, at least 30 Times greater than it can be supposed, as will be proved hereafter, then the Semichord E A would equal 276 French Leagues, and would far exceed a like Chord of the terrestrial Atmosphere. Therefore whether the Moon is covered with an Atmosphere or not, it's Diameter will always be observed the same; and the Observation of the Lunar Diameter can by no Means be equal to the Solution of the Question.

The Eclipses of the Sun by the Moon, give a greater Handle for deciding the Doubt; for the extreme Rays terminating the Cone of the Lunar Shade, as they touch the Body of the Moon, and pass thro' it's Atmosphere, will necessarily be inflected toward the *Axis* of the Cone; therefore the Cone will become shorter and more obtuse; but to shew the Quantity of that Variation, we must observe, that the Ray F A, or it's Parallel E G, which, if there was no Atmosphere, would be the Bound of the Lunar Shade F A C, would be refracted toward the *Axis* C A, at the Ingress of the Atmosphere G, and at the Egress H; whence the Semiangle of the Cone of the Lunar Shade will be increased by double the horizontal Refraction in the Lunar Atmosphere. Fig. 75.

Hence it follows, that if we suppose a lunar Atmosphere, a total Eclipse of the Sun will begin later and end sooner, than if we do not suppose it; moreover in certain Cases, that there will be no total Eclipse; which however the Diameters of the Sun and Moon observed in the same Degreee of Anomaly would require; for in these Cases the Cone of the Lunar Shade is contracted, because of the Atmosphere, and it might be so contracted as not to touch the Disk of the Earth even with it's Point.

After the same Manner exactly, the Duration and Quantity of the partial Eclipses would be diminished; for the Beginning of a partial Eclipse is observed, when the Cone of the Penumbra G D I enters upon the Habitation of the Observer; but a double Refraction F C E, E V H being supposed in the Atmosphere of the Moon, the Semiangle of the Cone of the Penumbra is diminished, and the Semidiameter of the Base G I is contracted into I H; therefore that the Beginning of an Eclipse may be observed in a given Fig. 76.

Place, a Space GH equal to the Centre I of the *Penumbra*, must be run over; the same must be said of the Emersion. Therefore a partial Eclipse will begin later and end sooner, supposing a Lunar Atmosphere, than if the Moon is naked; and it will also be observed to be less, for the Habitation T being immerged into the *Penumbra* by the Quantity T N, supposing a Lunar Atmosphere, will enter it only by the Distance T K. It may also be, that no Eclipse may be observed in that Place, where it would be observed, if no Atmosphere is supposed to be about the Moon; for the Disk of the *Penumbra* being altered, the Place R, which if the Moon is naked, would be immerged into it, will become free from it by the Quantity R N. But they who shall live in the Space YH, comprehended between the direct Ray X Y touching the Atmosphere, and the refracted Ray E H, terminating the *Penumbra*, will see the Sun free indeed from the Body of the Moon, but obscured by it's Atmosphere; and therefore a certain pale *Penumbra*, which, by what has been already demonstrated, must precede and follow the Disk of the Moon; moreover this Obscuration may be observed without any Eclipse.

These *Phænomena* must principally be observed in the Solar Eclipses, if there is any Atmosphere about the Moon; now let us see what is really observed.

In the first Place, as the *Axis* of the Lunar Shade is extended to 55 Semidiameters of the Earth, when greatest, and to 52½ when it is least, and as the least Distance of the Moon from the Earth is 54 Semidiameters of the Earth, if the lunar Atmosphere was capable of a horizontal Refraction of 8″, the Semiangle of the shady Cone will be increased by double the Quantity, that is, 16′, and therefore it will be equal to 16′ 41″, when it is most open, and to 16′ 5″ when it is narrowest. Moreover the least Semiangle of the Cone being supposed equal to 16′ 5″; it's *Axis* will be less than the least Distance of the Moon from the Earth, of 54 Semidiameters of the Earth, and therefore the Point of the Lunar Shade, will reach to the Earth. If therefore there is an Atmosphere about the Moon, in which the horizontal Refraction is 8″, there will be no total Eclipse upon the Earth. Therefore either there is no Atmosphere about the Moon, or if there is any, it produces a horizontal Refraction less than 8″.

But there are total Eclipses of the Sun observed with a Duration of the total Darkness. For Instance, in the Eclipse of 1724, the the Duration of the total Darkness amounted to 2′ 16″. The Moon at that Time ran over 1′ 52″ in it's horary Motion, and it's Shade always parallel to it in Degrees of the Disk of the Earth went over a Space 54 Times greater, that is, equal to 1° 7′ 30″; from which, if we take away the diurnal Motion of a Habitation equal to 20′, which may prolong the Duration of the Eclipse, we shall

ſhall have the Diameter of the Shadow equal to $47' \ 30''$, or 45173 Toiſes, or 22 Paris Leagues. Whence, by Calculation, we find that the *Axis* of the Cone of the Lunar Shade, is greater by at leaſt one Diameter of the Earth, than the Diſtance of the Moon from the Earth, which was then the leaſt, the Moon being about the *Perigæum*. Moreover from the given Diameters of the Luminaries, obſerved in the ſame Degree of Anomaly, the *Axis* of the Cone of the Lunar Shade is found to be equal at leaſt to 55 Semidiameters: Whence it follows, that the Spot of the Lunar Shade on the Diſk of the Earth, and the *Axis* of the Cone are found to be exactly the ſame, as the Diſtances of the Moon and the obſerved Diameters of the Lunaries ſeems to require. There is therefore no Atmoſphere about the Moon, or, if there is any, it cannot produce any ſenſible Refraction. But that there may be no Room left for Doubt, I ſhall give a Reaſon for thoſe *Phænomena*, which being obſerved in the Solar Eclipſes, have given Room to imagine a Lunar Atmoſphere.

Firſt indeed, that diminutive Light, which is obſerved in total Eclipſes, does not prove any Refraction in the Fluid, which encompaſſes the Moon; for by *M. Maraldy*'s Experiments, which have been repeated by me with the greateſt Care, and with the ſame Succeſs, it is manifeſt, that the Shadow of Bodies not covered with any Atmoſphere, if they are expoſed to the Sun, are bright about the *Axis* of the Cone; and the more ſo as it is the farther from the Body itſelf. Moreover the Habitation of the Obſerver in a total Eclipſe is about the *Axis* of the Cone of the Lunar Shade, and in the Neighbourhood of it's Point. It is no wonder, therefore, that the Middle of the Shadow is covered with a malignant Kind of Light, which may otherwiſe be increaſed by the Rays being reflected by an illuminated Air ſurrounding the Shadow about the Middle.

Secondly, the lucid *Annulus* ſurrounding the Moon in total Eclipſes, by no Means proves the Exiſtence of the Lunar Atmoſphere, as will appear to any one that hides the Sun from him by Balls of Wood or any opake Matter. Wherefore it is to be aſcribed not to a Lunar, but a Solar Atmoſphere, as has abundantly been proved, by *M. Mairan* *, in his Treatiſe of the *Aurora Borealis*.

Thirdly, the Diminution of the lunar Diameter, which in the Solar Eclipſes is obſerved to be about $30''$ leſs than when the Moon ſhines with a full Orb in the ſame Degree of Anomaly, by no means proves the Lunar Atmoſphere; tho' ſome Inequalities of Mountains are obſerved in the Circumference of the Diſk of the Moon, which quite diſappear in the Full-Moon; for lucid Objects ſtrike the Fibres of the Eye ſo ſtrongly, that the Motion of them is communicated to the neighbouring Fibres, and ſo the Image of the lucid Body is increaſed beyond the due Quantity, which is known by common Experience; for if a Stick is placed between the Moon and the

* Sect. I. cap. i. pag. 14.

the Eye, the Diameter of the Stick over againſt the Moon will ſeem to be diminiſhed; but if at that Time any Cloud comes over the Luminary, the Diminution of the Stick will appear leſs, but if the Cloud take away the Sight of it there will be no Diminution; and laſtly it will be various according to the various Intenſeneſs of the lunar Light.

As for the Inequalities of the Mountains, they are leaſt obſerved for the ſame Reaſon in the Full-Moon; for the lunar Mountains obſcure of themſelves, and ſeen in the bright Orb of the Sun, eſcape the Eye much leſs, than when ſhining in the Full-Moon, they are extinguiſhed in the neighbouring Splendor of that Luminary; eſpecially as the lunar Light is ſo intenſe, that a Star of the third Magnitude can hardly be ſeen when near it. But, to take away all Doubt in this Affair, if the Limb of the Moon oppoſed to the Sun was the Bound of it's Atmoſphere, and not of it's very Body, the Mountains in the Circumference of the Moon would never be obſerved by the longer Teleſcopes with narrower objective Apertures. I have often obſerved ſeveral Inequalities of Mountains in the Diſk of the Full-Moon with a Teleſcope of 36 Paris Feet, and an objective Aperture of one Inch; whence it follows, that the Diſk of the Full-Moon is terminated by the Circumference of it's Body and not of it's Atmoſphere.

This Obſervation was really made by Dr Halley in the Preſence of M. Delouville. *See* Vol. IV. p 258, 259.

Fourthly, I muſt ſpeak a little of that wonderful Obſervation, in 1715, of the lunar Coruſcations, which was made by *M. Delouville*, in the Preſence of many Aſtronomers of the Royal Society. We may ſuppoſe, that the viſible Limb of the Moon is compoſed of the Tops of Mountains; which, in a total Eclipſe, hide the Sun from the Obſerver in the ſame Manner as the Trees of great Woods obſtruct the Sight. Whence if ſome Rows of Mountains on the Surface of the Moon afford a free Paſſage in a right Line to the ſolar Rays, they muſt imitate a Sort of Coruſcations, in the ſame Manner, as when in a *Camera Obſcura*, a Ray of the Sun by Means of a *Speculum* is ſuddenly admitted, and the Picture of external Objects drawn on the *Focus* of the *Lens* is taken away, it will be illuminated with luminous Tracts very much reſembling Lightning; which I think is the more eaſily to be allowed, becauſe thoſe ſudden Coruſcations have always been obſerved near the Limb of the Moon, as appears from the Scheme of this Eclipſe in Sir *Hans Sloane*'s *Muſeum*, drawn by his Daughter.

As for that pale Ring accompanying the Limb of the Moon in this Eclipſe, as nothing like it appeared either to me, or to any other Aſtronomer in the Solar Eclipſes hitherto obſerved, which however, according to the Hypotheſis of the Lunar Atmoſphere, muſt always and every where be obſerved, we ſhall make no Mention of it here.

From

From all this it is manifest, that there is nothing like a Lunar Atmosphere in the Eclipses of the Sun. I shall now speak of the Eclipses of the Fixed Stars and Planets by the Moon.

If the Moon is surrounded by an Atmosphere, the Planets and fixed Stars will be seen by an Observer placed on the Surface of the Earth, to be hid later behind the Moon, and to emerge sooner from it's Disk, than if the Moon is supposed to have no Atmosphere; nay, and in some Places, where an Eclipse of a fixed Star or a Planet ought to be seen, there will be none. To make this plain, let A B C be the Body of the Moon, and let a Star be placed as it were at an infinite Distance in S; the parallel Rays L V, M X, touching the Body of the Moon on all Sides, constitute a cylindrical Surface, of which Cylinder the Base V Z X comprehends in it's Compass all the Habitations on the Disk of the Earth, in which the Star or Planet is covered by the Moon. The Observer therefore will see the Beginning of the Eclipse at V, and the End of it at X, and will measure the Duration of the Time, in which the Moon may run thro' it's Diameter, or rather a Space equal to it. But if we suppose an Atmosphere of the Moon, the Ray I W will not remain parallel to the *Axis* of the Cylinder, and the Cylinder itself will become a Cone, of which the Section Y T U will mark the Habitations where the Eclipse must be. And the Base Y T U being contracted, the Point Y will come upon the Habitation later than the Point V; and the Limit U will forsake it sooner than X: Therefore, the Eclipse of a Star or Planet by the Moon will begin later and end sooner, if we suppose an Atmosphere about the Moon, than if there is none: And there will be no Eclipse in that Place where it ought to be observed without an Atmosphere; for the Place C being covered by the Circumference V Z X of the former Cylinder, will be free from the Section of a Cone Y T U. Besides, supposing the horizontal Refraction in the Atmosphere of the Moon equal to 8'', V Y will be equal to 1384 Toises, or $\frac{1}{2}$ of a Paris League; whence it follows, that no Eclipse must be observed in the Places pointed out in the Calculation, as often as they are immerged into a cylindrical Area not exceeding $\frac{1}{2}$ of a League. Fig. 77.

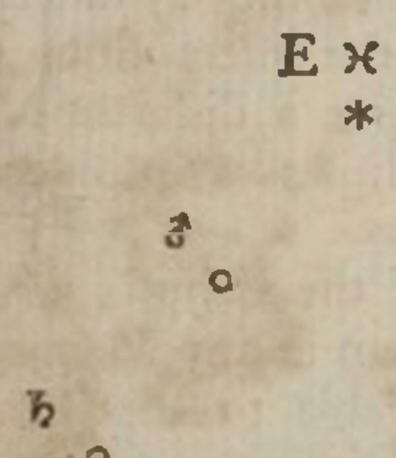
Another *Phænomenon* also arises from the Supposition of a Lunar Atmosphere; in the Part of the Cylinder Y R, the Star indeed will always be seen, but thro' the Interposition of the Lunar Atmosphere; and therefore it will acquire a different Motion and Colour from the true; and that in all Eclipses whatsoever, whether the Star is one of the biggest or least.

Besides, the Duration of the Eclipses of the Fixed Stars and Planets by the Moon, does not seem in any Manner diminished, but is always found to be exactly agreeable to the Diameter of the Moon and it's Motion. As for those Observations, in which the Star after the Contact is seen to proceed a little in the Disk of the Moon before the Occultation, we shall refer the whole Cause of them to the increaſed Diameter of the Moon and Star; for if the Lunar Atmoſphere was the Cauſe of this Appearance, it would always be obſerved the ſame in all Stars, and in any Apertures of Objectives. Beſides, I have not as yet obſerved the Progreſſion of any Star in the Diſk of the Moon, unleſs it was of the firſt, or at leaſt of the ſecond Magnitude, and that by half of it at moſt, and the true Diameter of the Fixed Stars, as is manifeſt to any Obſerver, becomes inſenſible, and is increaſed only by ſpurious Rays; whence the adventitious Rays both of the Star and the Moon, are mixed in the Bottom of the Eye before the true Conjunction of the Bodies of the Sun and of the Moon; and if the viſible Limb of the Moon was the Bound of the Atmoſphere, and not of the Body, no Mountains would be obſerved on it's Circumference with greater Tubes and narrower objective Apertures; which however, as has been ſaid, are ſeen plainly enough.

From all this it is manifeſt, therefore, that the Moon is not ſurrounded with a refracting Atmoſphere, the Refraction of which is capable of being obſerved; for there might be an Atmoſphere about the Moon, in which the horizontal Refraction amounted to $1''$ or $2''$; for this Opinion ſeems to be countenanced by the greater Spots in the Moon, which cannot by any Means be taken for Woods, as *Hartſoeker* and others have imagined. For the Shadows of the Edges are always obſerved nearer to the bright Limb of the Moon; whence it is rightly concluded, that they are Cavities and not Woods, which would project a Shadow from the other Side. Moreover ſome Fluid may be ſuppoſed to be in them, in which caſe it would be very agreeable to Philoſophy, that ſome Vapours ſhould be raiſed from them, the Congeries of which would repreſent a Sort of an Atmoſphere about the Moon, which Atmoſphere would not be found to be very thick; for by Sir *I. Newton*'s Demonſtrations, it would hardly equal $\frac{1}{3}$ of the Denſity of the terreſtrial Vapours, nor would be alike at different Times, thoſe Vapours being deſtitute of any other Addition.

A Conjunction of Saturn *and* Mars, *obſerved at* Wittemberg *by* Joh. Frid. Weidler, *F. R. S.* No. 441. p. 238. April, *&c.* 1736.

XX. Saturn and Mars were ſeen *Feb.* 5. $7^h\ 30'$ p. m. in the ſame right Line with the Star E χ Bayeri.



♄ ♂ $2^{\circ}\ 15'$

♂ E χ 1 12 $30''$

Feb.

Pla. XIII. *Vol. VIII. part 1. pa. 178.*

Fig. 74.

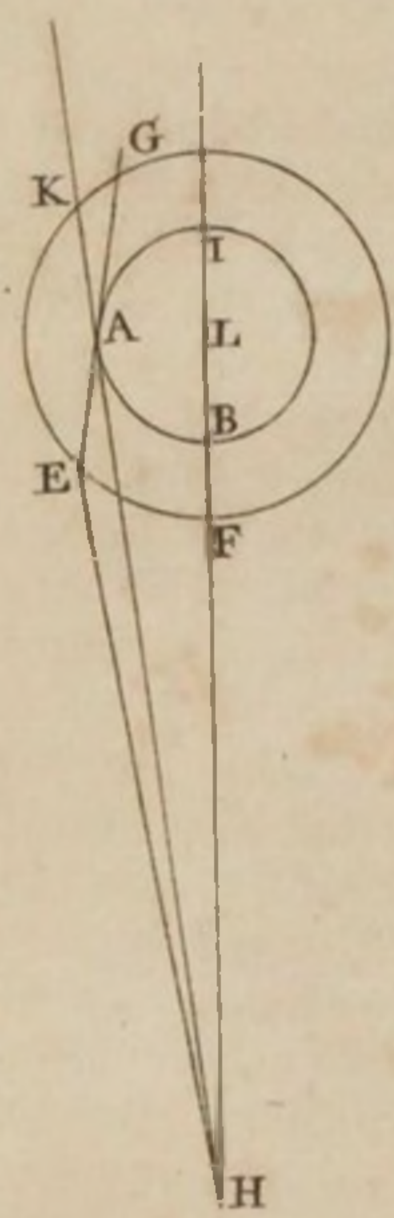
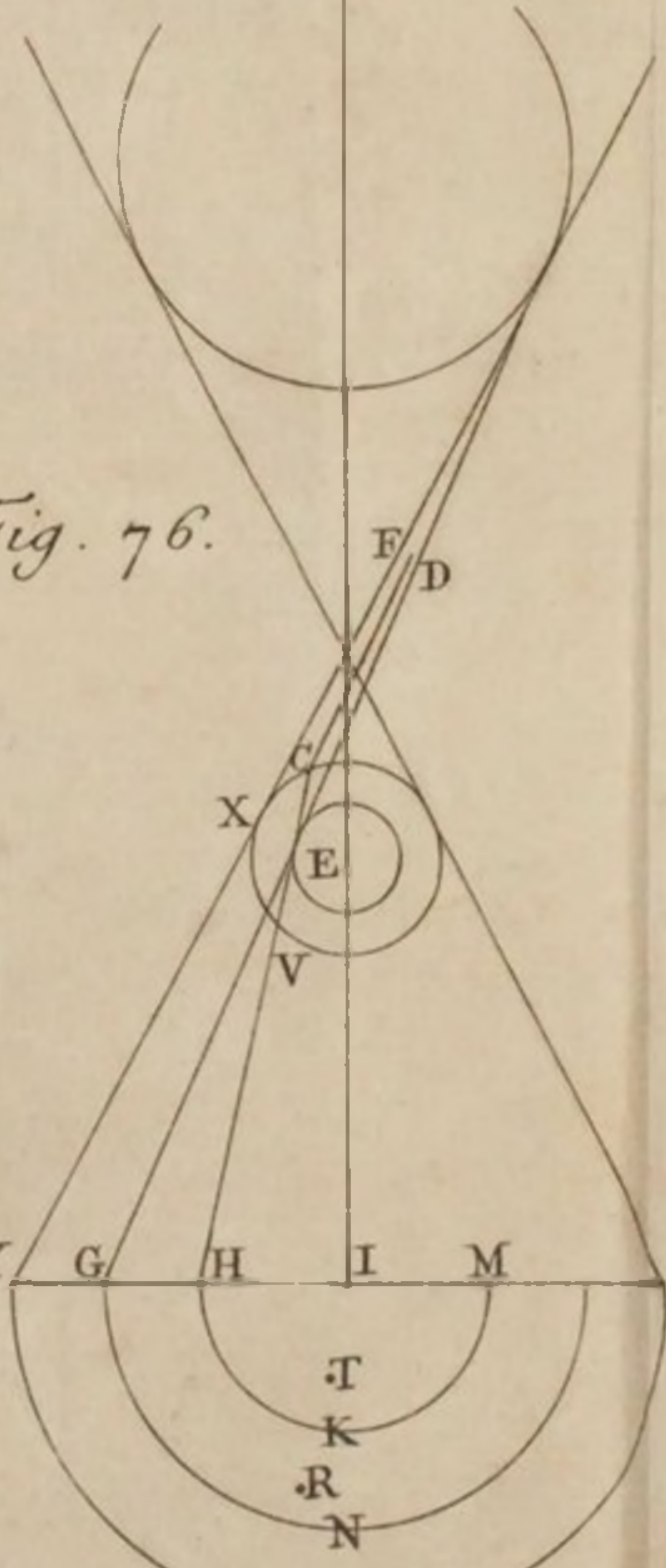


Fig. 75.

Fig. 76.



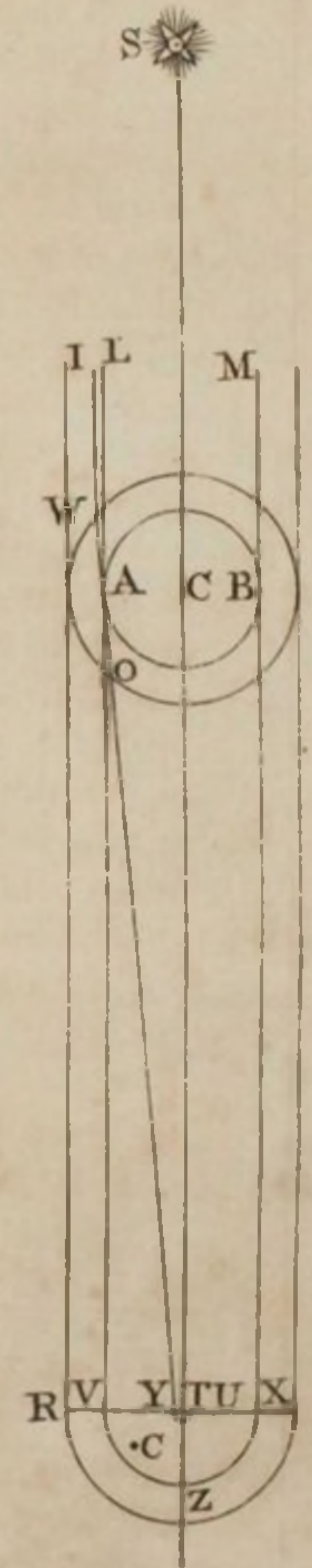


Fig. 77.

Feb. 19. 7^h $15'$ p. m. the Diſtance of ♂ from ο♓ was obſerved, namely ο♓ ♂ 1° $17'$ $30''$. Mars was diſtant from the Star toward the North.

XXI. 1. *Eclipſes of the Satellites of Jupiter obſerved by Euſtachius Manfredi. No. 429. p. 117. July, &c. 1733.*

h	′	″	True Time. *April* 2, 1732.
10	56	3	Emerſion of the ſecond Satellite of Jupiter out of the Shadow. Sky clear. Teleſcope of 22 Feet.
13	2	36	Emerſion of the fourth Satellite, Sky clear, Teleſcope 22 Feet.
7	31	40	*April* 3. Emerſion of the inner Satellite from the Shadow, Sky clear, Teleſcope 22 Feet.
13	32	4	*April* 9. Emerſion of the ſecond Satellite, Sky clear, Teleſcope 22 Feet, a little doubtful.
9	44	41	*May* 3. Emerſion of the inner Satellite, Sky clear, Teleſcope 22 Feet.
			May 4. Emerſion of the ſecond Satellite, Sky cloudy, Wind.
10	35	32	Teleſcope 14 Feet.
10	35	41	Teleſcope 11 Feet.
			May 26. Emerſion of the inner Satellite, Sky clear.
9	58	4	Teleſcope 22 Feet.
9	58	21	Teleſcope 11 Feet.
9	43	47	*June* 2. Emerſion of the third Satellite, Sky cloudy, Teleſcope 22 Feet.
11	7	24	*June* 9. Immerſion of the third Satellite into the Shadow, Sky clear, Teleſcope 22 Feet.
10	8	25	*June* 18. Emerſion of the inner Satellite, Sky clear, Teleſcope 22 Feet.
7	36	5	*July* 27. Emerſion of the inner Satellite, Sky clear, Teleſcope 11 Feet, doubtful.
			Jan. 17, 1733. Immerſion of the third Satellite, Sky clear.
14	8	45	Teleſcope 22 Feet.
14	8	33	Teleſcope 14 Feet.
16	13	29	Emerſion of the third Satellite, Sky clear, Teleſcope 22 Feet.
			March 12. Immerſion of the inner Satellite, Sky clear.
13	23	34	Teleſcope 22 Feet.
13	23	22	Teleſcope 11 Feet.

— Observed by Geo. Lynn, *Esq; at* Southwick, *near* Oundle *in* Northamptonshire. No. 440 p. 196. Jan. *&c.* 1736.

2. The Telescope I made use of is the same as formerly, having a 13 Foot Object-Glass, with an Aperture of 2 $\frac{4}{10}$ Inches, and an Eye-Glass of 2 $\frac{1}{2}$ Inches. By apparent Time, at *Southwick*, near *Oundle* in *Northamptonshire*, Longitude West from *London*, 00° 30′, as follows:

		Month.	D.	h	′	″
1730.	The 2d Satellite began to emerge —	*April*	29	10	19	20
	It seemed in a Minute after to be as bright as the 4th Satellite — —				20	20
	And at full Brightness about — —				21	20
17$\frac{29}{30}$.	The 4th Satellite emerged — — —	*Jan.*	25	9	45 nearly	
1730.	The 2d began to immerge — — —	*Nov.*	28	13	17	46
	And was quite out of Sight at — —				19	46
1731.	The 2d began to emerge — — —	*March*	29	11	33	8
	And seemed at full Brightness about— —				36	30
	The 1st Satellite began to emerge —	*April*	18	11	45	10
	And was at full Brightness about —				46	10
	Again the first began to emerge — —	*May*	4	10	4	30
	Was at full Brightness about — —				5	45
	And passed by the 3d Satellite at — —			11	49	30
17$\frac{30}{31}$.	The 1st Satellite began to immerge —	*Jan.*	7	12	2	55
	And was quite out of Sight about —				4	25
17$\frac{31}{32}$.	The 3d Satellite began to immerge —	*Jan.*	28	14	16	0
	Was but equal in Light to the 2d at —				19	0
	Quite gone at — — — — —				24	0
1732.	The 2d Satellite began to emerge — —	*April*	30	12	23	57
	The 1st Satellite began to emerge —	*May*	6	12	48	44
17$\frac{32}{33}$.	The 3d Satellite quite disappeared —	*Febr.*	18	13	5	30
1733.	Again the 3d Satellite disappeared —	*April*	2	13	3	30
	But it began to fail of it's Light about 5 or 6 Minutes before.					
1735.	The 2d Satellite immerged — — —	*May*	28	10	45	0
	The 3d began to emerge — — —	*August*	3	9	10	30

And was 4 or 5 Minutes before it came to it's full Brightness.

— Observed at Petersburg, *by* M. Jos. Nic. De l'Isle. *F. R. S.* No. 441. p. 225. Ap. *&c.* 1736.

3. It was a great Pleasure to me to see that Mr *James Hodgson* has been at the Pains to calculate the Eclipses of the four Satellites of *Jupiter*, which were to happen in 1732. It was to be wished he would continue to do so for the Years following; but I would advise him, to do it a long while beforehand, that People in Foreign Countries might have Time to be informed of it. He says, he has made use of Tables of the Satellites, which have not been corrected these 50 Years*. Probably he means the Tables of M. *Cassini*, published at the Royal Printing-House at *Paris*, in 1693, at the End of the Observations of the Gentlemen of the Academy made in several

* *He means Tables of the late Mr* Flamstead.

ſeveral Voyages. However, the late M. *Caſſini*, has from Time to Time made divers Corrections to thoſe Tables, though they never were made publick. M. *Maraldi* has alſo much worked at it after the Death of M. *Caſſini*, and has communicated to me his Corrections, on which I have taken Pains to calculate new Tables; but having in the Year 1724, received of Dr *Halley* a Copy of his Aſtronomical Tables, among which, are thoſe of the four Satellites of *Jupiter* by Mr *Bradley*, I judged there could not be any better, till ſome Method ſhall be found and explained geometrically to deduce from the Laws of Gravity, the Effect of the mutual Attraction of theſe Satellites on one another, and with relation to *Jupiter*: But as I could not hope this could be done ſo ſoon, I took the Pains again to calculate new Tables upon thoſe of Mr *Bradley*, by reducing the Tables of the four Satellites into the ſame Form with thoſe Mr *Pound* has made of the firſt Satellite only. Theſe Tables being thus made eaſy, I have uſed them hitherto for comparing Obſervations; and my Brother has taken the Pains, ſince the drawing up thoſe Tables, in the ſaid Manner, to calculate a Year beforehand all the Eclipſes of the four Satellites. I commonly ſent thoſe Calculations to my Correſpondents, to prepare them for Obſervations, and ſome Years of thoſe Ephemerides have been publiſhed in the *little Gazette of Literature* of *Leipzig*, printed in *High Dutch*. My Brother lately prolonged theſe Calculations to the Month of *January*, 1737.

Herewith follow the laſt Obſervations on the Satellites of *Jupiter*, which were made at *Petersburg*, ſince thoſe inſerted in the third Volume of the Memoirs of the Academy of *Petersburg*, to the preſent Time.

N. St.			True Time h	′	″	
1731.	*Dec.*	6	17	3	5	Immerſion of the firſt Satellite difficultly obſerved with a reflecting Teleſcope of 5 Foot. The true Time was found only by Means of two Clocks.
1732.	*Jan.*	4	13	30	56	Immerſion of the ſecond by the Reflecter, doubtful to a few Seconds. *Jupiter* not being well defined nor ſufficiently high. The true Time adjuſted by two Clocks.
		9	18	33	7	Immerſion of the fourth by the Reflecter. The Sky not very ſerene, and the true Time adjuſted only by two Clocks.

The

N. St.			True Time h	′	″	
1732.	*Jan.*	9	20	25	0	The other Satellites disappearing by the Day-light, the fourth was not yet come out of the Shadow. Telescope the same.
	Feb.	22	13	25	34	The first Satellite, just entering the Shadow, was yet visible when a Mist covered *Jupiter.*
			13	26	34	*Jupiter* being uncovered, the first Satellite did not now appear through the reflecting Telescope. The true Time was adjusted only by two Clocks.
	March	8	8	22	20	Immersion of the third by the reflecting Telescope. The Wind was somewhat troublesome, the true Time was adjusted by two Clocks.
	April	3	8	46	23	Emersion of the first by the reflecting Telescope. Doubtful to a few Seconds, by reason of the Nearness of the Satellite to *Jupiter.*
		13	7	20	30	Immediately after Sunset, *Jupiter* becoming visible to the Eye, the third Satellite appeared to be out of the Shadow, and entirely clear by the reflecting Telescope.
		20	11	6	52	Emersion of the third Satellite, by the reflecting Telescope. The Sky serene.
		27	15	13	0	The third Satellite had been come out of the Shadow perhaps for several Minutes; for the other Satellites did not appear better than this which was seen with the reflecting Telescope through the Mist, *Jupiter* being low, and the *Crepusculum* strong.
	May	10	12	55	54	Emersion of the first by the reflecting Telescope. Sky serene. Observation certain.
		26	11	14	5	Emersion of the first by a Telescope of 13 Foot. Cloudy.
	Dec.	24	18	4	30	Immersion of the 1st by a Telescope of 13 Foot. A good Observation.

4. Full

4.

— *Obſerved by the Jeſuit's at the College at* Pekin. No 468. p. 306. Jan. 1742-3.

		h	′	″			
1740.							
Nov.	4.	5	55	15	*a. m.*	Full Immerſion of the firſt Satellite, Teleſcope 13 Feet.	
	8.	12	16	5	*p. m.*	Full Immerſion of the 2d Satellite.	
	12.	14	16	52	*p. m.*	Full Immerſion of the 1ſt Satellite.	
	15.	14	49	24	*p. m.*	Full Immerſion of the 2d Sattellite.	
	16.	9	55	0	*p. m.*	As the 3d Satellite was going to immerge into the Shadow, it diſappeared in a Cloud, ſo that the Immerſion neither of this nor of the 4th could be ſeen.	
	19.	16	9	10	*p. m.*	Full Immerſion of the 1ſt Satellite.	
	21.	10	37	33	*p. m.*	Full Immerſion of the ſame.	
	22.	17	21	48	*p. m.*	Full Immerſion of the 2d Satellite.	
	26.	18	1	25	*p. m.*	Full Immerſion of the 1ſt Satellite.	
	30.	17	47	40	*p. m.*	Full Immerſion of the 3d Satellite.	
Dec.	3.	9	10	57	*p. m.*	Full Immerſion of the 2d Satellite.	
	10.	11	42	32	*p. m.*	Full Immerſion of the 2d Satellite, Teleſcope 10 Feet.	
	14.	10	38	20	*p. m.*	Full Immerſion of the 1ſt Satellite, Teleſcope 10 Feet.	
	17.	14	15	20	*p. m.*	Full Immerſion of the 2d Satellite, Teleſcope 10 Feet.	
	19.	18	3	0	*p. m.*	Immerſion of the 1ſt Satellite, Teleſcope 10 Feet.	
1741.							
Jan.	1.	5	30	6	*p. m.*	Emerſion of the 1ſt Satellite, Teleſcope 10 Feet.	
	6.	12	52	25	*p. m.*	Emerſion of the 1ſt Satellite, Teleſcope 18 Feet.	
	8.	7	20	30	*p. m.*	Emerſion of the ſame, ſame Teleſcope, doubtful.	
	11.	13	51	22	*p. m.*	Emerſion of the 2d Satellite. Teleſcope 10 Feet.	
	14.	2	42	40	*a. m.*	Emerſion of the 1ſt Satellite. Teleſcope 18 Feet.	
	15.	9	12	36	*p. m.*	Emerſion of the ſame. Teleſcope 13 Feet.	
	18.	16	26	15	*p. m.*	Emerſion of the 2d, Teleſcope 10 Feet.	
	22.	5	41	57	*p. m.*	Emerſion of the 2d, Teleſcope 18 Feet.	
		11	3	19	*p. m.*	Emerſion of the 1ſt, Teleſcope 18 Feet.	
	24.	5	33	24	*p. m.*	Emerſion of the 1ſt, Teleſcope 10 Feet.	
	29.	8	18	16	*p. m.*	Emerſion of the 2d, Teleſcope 13 Feet.	
		12	57	10	*p. m.*	Emerſion of the 1ſt,	Teleſcope 8 Feet.
Feb.	3.	8	18	0	*p. m.*	Emerſion of the 3d,	
	5.	10	53	20	*p. m.*	Emerſion of the 2d,	
	6.	2	53	0	*p. m.*	Emerſion of the 1ſt,	

1741.	h	'	''		
Feb. 7.	9	20	35	*p. m.*	Emersion of the 1st, Telescope 8 Feet.
8.	10	43	0	*p. m.*	Full Immersion of the 4th, Telescope 13 Feet.
	14	6	30	*p. m.*	The 4th began to immerge. Same Telescope.
10.	9	16	30	*p. m.*	Full Immersion of the 3d. Same Telescope.
12.	13	32	0	*p. m.*	The 2d immerged, Telescope 8 Feet.
14.	11	14	15	*p. m.*	Emersion of the 1st, Telescope 18 Feet.
16.	5	43	45	*p. m.*	Emersion of the same, same Telescope.
23.	7	39	29	*p. m.*	First emerged, Telescope 18 Feet.
25.	8	26	30	*p. m.*	Emersion of the 4th, Telescope 13 Feet.
Mar. 2.	0	36	11	*p. m.*	Emersion of the 1st, Telescope 18 Feet.
11.	6	2	45	*p. m.*	Emersion of the same, same Telescope.
Apr. 3.	6	26	35	*p. m.*	Emersion of the same, same Telescope.
	8	14	0		Emersion of the 2d, Telescope 8 Feet.
10.	8	20	37	*p. m.*	First emerged, Telescope 13 Feet.
May 3.	8	40	6	*p. m.*	Emersion of the 1st, same Telescope.
	9	36	0		Emersion of the 4th, same Telescope.
Sept. 8.	4	41	48	*a. m.*	Immersion of the 1st, same Telescope.
Oct. 1.	5	0	8	*a. m.*	Immersion of the 1st, Telescope 13 Feet.
15.	17	8	49	*p. m.*	Immersion of the 2d, Sky a little cloudy, Telescope 13 Feet.

An Occultation of Jupiter *and his* Satellites *by the* Moon, October 27, 1740, *in the Morning; observed at Mr* George Graham's, *F.R.S. House in* Fleet-Street, London, *by Dr* Bevis, *and Mr* James Short, *F.R.S.* No. 459. p. 647.

XXII.

Times by the Clock, *October* 26.			Apparent Times. *October* 27.			
Clock above Stairs.						
h	'	''	h	'	''	
23	46	38	0	0	0	The Sun's Centre passed the Meridian in the Transitory.
Clock below.						
14	49	4	15	2	25	The Moon's illuminate Limb preceded *Jupiter* in right Ascension 1' 38'' in Time.
14	52	32	15	5	53	The same Limb preceded *Jupiter* 1' 31''. These were taken with a reflecting Telescope, 9 Inches Focus, fitted with Wires at half right Angles, and which magnified 30 times.
Clock above.						
15	26	1	15	39	20	*Sirius* passed the Meridian.
15	37	43	15	51	2	The Moon's Centre passed the Meridian.
15	39	9	15	52	28	*Jupiter*'s Centre passed the Meridian.

Clock

Times by the Clock, *October* 26. Clock below Stairs.			Apparent Times. *October* 27.			
h	′	″	h	′	″	
15	41	15	15	54	36	*Jupiter*'s third Satellite eclipſed by the Moon.
15	47	10	16	0	31	*Jupiter*'s ſecond Satellite eclipſed by the Moon.
15	55	4	16	8	25	*Jupiter*'s preceding Limb immerged.
15	57	20	16	10	41	*Jupiter*'s ſubſequent Limb immerged.
16	0	54	16	14	15	*Jupiter*'s firſt Satellite eclipſed by the Moon.
						Theſe Immerſions were taken with a Reflecting Teleſcope, of 16.5 Inches Focus, that magnified 120 times.
Clock above.						
16	17	49	16	31	8	*Procyon* paſſed the Meridian.
Oct. 27.			*Oct.* 28.			
23	46	42	0	0	0	The Sun's Centre paſſed the Meridian.

N. B. The Clock in the lower Room was all along 2″ ſlower than the Clock in the upper Room.

None of the Emerſions could be ſeen for Clouds. Whilſt *Jupiter* was immerging, the Sky was perfectly ſerene; and, at his neareſt Approach to the Moon, he did not appear to alter his Figure in the leaſt, nor to be tinged with any priſmatic Colours; neither did he (as is ſaid to have been ſometimes obſerved through Refracting Teleſcopes) ſeem to enter at all upon the Moon's Body.

That Part on the Moon's Limb where *Jupiter* entered, was a Hollow; and though ſome are of Opinion, that the Circumference of the Moon, as it is bounded to our Eye, is a perfectly ſmooth Circle, and that no Hills or Hollows appear there, as on the Surface of the Moon; yet if it be looked at in a clear Night with a good Teleſcope, that magnifies about 100 times, or even leſs, it will be ſeen rugged and uneven all round.

Notwithſtanding *Jupiter*'s Light ſeems to be more vivid than that of the Moon, when he is ſeen at a good Diſtance from her, and far more ſo when the Moon is away; yet the contrary is plainly diſcerned when they are near one another: And in this Obſervation, whilſt *Jupiter* was immerging behind the Moon, his Diſk appeared much dimmer, and of a more faint and duſky Complexion, than the Diſk of the Moon.

An Occultation of Mars *by the* Moon, Oct. 7. 1736, *observed by Mr* Geo. Graham, *F. R. S. in* Fleet-street, London, No. 446. p. 100. July, &c. 1737.

XXIII. 1. The first Contact could not be seen for Clouds.

Apparent Time. h ′ ″	
At 14 24 44	*Mars* appeared about half covered, but a distinct View could not be had for flying Clouds.
14 25 21	*Mars* totally covered, the last Ray of Light being then lost.
15 11 22	The *Moon* appeared, but *Mars* was not seen, no Part being yet emerged.
15 15 11	I judged it was quite emerged, but Clouds prevented the *Moon*'s Limb from being distinctly seen.

The Observation was made with a Refracting Telescope of 12 Feet.

—Observed in Covent-Garden, *by* J. Bevis, *M. D.* Ibid. p. 101.

2. Before the Eclipse, I took several Differences of Right Ascension and Declination between ♂ and μ *Piscium*, for ascertaining the true Place of *Mars*: As also several Differences of right Ascension and Declination between the *Moon* and *Mars*, before and after the Eclipse, which I shall give another Time.

Apparent Time. h ′ ″	p. m.
14 24 10	I was surprized to see *Mars* continue quite round, though hardly, to Appearance, disjoined from the scabrous Edge of the *Moon*; but that Instant I thought it began to lose it's Figure. Clouds.
14 25 26	The *Moon* shone out bright again, but *Mars* was entirely vanished.
15 14 46	The *Moon* being just clear of a Cloud, I saw *Mars* partly emerged.
15 14 49	He seemed just half out; then Clouds came on again, so that I saw not the final Contact.

The *Moon*'s Diameter was 21,157 Parts of the Micrometer and it's illuminated Part passed over the horary Thread in 2 Minutes, 3 Seconds.

I am certain of the Time to 2 or 3 Seconds.

Occultation of Mars *by the* Moon, *observed by the Jesuits at* Pekin, No. 468. p. 306. Jan. 1742-3.

3. *Nov.* 9, 1740. $11^h\ 2'\ 15''$ *Mars* emerged from the *Moon*, in a right Line drawn through Menelaus and Kepler, the Immersion could not be seen because of Clouds in the eastern Horizon.

XXIV. § 1.

XXIV. §. 1. *October* 10, 11, and 12, when ♂ passed near μ ♓, a Star of the 5th Magnitude, I observed the following Distances of the Centre of Mars from that Star.

Observations on Mars in the Autumn of 1736, *at Berlin, by* Christ. Kirch, *Astronomer of the R Society there.* No. 459. p. 573. Jan. &c. 1741.

Conjunction of Mars & μ. ♓.

St. N.	True Time.			Parts of the Micromet.	Value of Parts of the Mic.
	h ′				′ ″
Oct. 10	9 41 *Vesp.*	♂ μ ♓	Telesc. 7 F.	48½	19 24
	9 46		Telesc. 9 F.	65	19 21
Oct. 11	10 1	♂ μ ♓	Telesc. 9 F.	22½	6 42
	10 4		Telesc. 7 F.	16	6 24
	10 9		Telesc. 7 F.	16½	6 36
	10 12		Telesc. 9 F.	22	6 33
Oct. 12	8 55 *Vesp.*	♂ μ ♓	Telesc. 9 F.	71½	21 18
	8 59		Telesc. 7 F.	53	21 12
	9 5		Telesc. 7 F.	53½	21 24

§ 2. In order to obtain from these Distances the Time of the Conjunction of Mars with the Star μ ♓, I chose the three following Distances.

	h ′		′ ″
1. *Oct.* 10.	9 43 *Vesp.*	Distance of the Centre of ♂ from μ ♓ — — — — — —	19 22
2. *Oct.* 11.	10 6 *Vesp.*	— — — — — — —	6 34
3. *Oct.* 12.	9 0 *Vesp.*	— — — — — — —	21 18

I supposed from the Ephemerides, the diurnal Motion of Mars in Longitude 19′ 30″, in Latitude 3′ 40″: Therefore the diurnal Motion of Mars in his proper Orbit, is 19′ 51″, and the Angle of the Orbit of Mars and the Ecliptick (or rather with the Parallel of the Ecliptick) is 10° 39′.

§ 3. In the oblique-angled Triangle *a* μ *b*, three Sides being given namely, Fig. 78.

	′ ″
a b, the Motion of Mars, which answers to $24^h\ 23'$ (the Time between Obs. 1 and 2) — — — — — —	20 10
a μ, the Distance first observed — — — — — —	19 22
b μ, the Distance secondly observed — — — — —	6 34
From μ I drew a Perpendicular to the apparent Orbit of Mars μ χ, and in the rectangular Triangle *b* χ μ sought the Particle of the Orbit of Mars *b* χ, and found it to be —	1 51
and the least Distance of ♂ and μ, χ μ, which I found to be —	6 18

 The

	h	′
The Particles of the Orbit of Mars χb anſwer to —	2	14
Which being ſubtracted from the ſecond Time of Obſervation, *Oct.* 11. — — — — — — —	10	6 *Veſp.*
Leave the true Time ☌ ♂ μ ♓ in the Orbit, *Oct.* 11. —	7	52 *Veſp.*

§. 4. In the oblique angled Triangle $b\mu c$ the Motion of Mars between Obſ. 2, and 3, bc, is — — — — — 18′ 55″
The Diſtance of ♂ from μ ♓ by the Obſ. 2. $b\mu$ — — — 6 34
The Diſtance of ♂ from μ ♓ by Obſ. 3. $c\mu$ — — — 21 18

Theſe three Sides being given, I ſought the Angle c, and found it to be 17° 35′. Then I drew a Perpendicular from μ to the Orbit of Mars $\mu\chi$, and in the rectangular Triangle $c\chi\mu$, the Hypothenuſe $c\mu$ being given, I ſought the Sides $\mu\chi$, and $c\chi$, and found $\mu\chi$, the leaſt Diſtance 6′ 26″ and $c\chi$ — — — — — 20′ 18″
From which the Side bc being ſubtracted — — — — 18 55
Leaves $b\chi$ — — — — — — — — — — 1 23

	h	′
To which anſwers in Time — — — — —	1	40
Which being ſubtracted from the Time of Obſ. 2. *Oct.* 11.	10	6 *Veſp.*
Gives the true Time of the leaſt Diſtance, or of the Conjunction of ♂ and μ ♓ in the Orbit, *Oct.* 11.	8	26 *Veſp.*

§. 5. The Deductions in the two preceding Paragraphs, as uſual, differ but little. If I had taken the diurnal Motion of Mars about ¼ of a Minute leſs, the Difference would have been ſmaller. In the mean Time, if I chooſe a mean between the 2 Deductions, I can err but very little from the Truth; and thus I gather the true Time of ☌ ♂ μ ♓ in the Orbit of Mars, *Oct.* 11. 8ʰ 9′ the leaſt Diſtance of ♂ from μ ♓ 6′ 22″ North.

§ 6. Tho' this might have been ſufficient, yet I ſet about a new Calculation, ſuppoſing the diurnal Motion of ♂ in Longitude — — — — — — — — — — 19′ 15″
In Latitude — — — — — — — — — — 3 40
Therefore the diurnal Motion of ♂ in the Orbit was — — 19 36
And the Angle of the Orbit of Mars with the Parallel of the Ecliptick 10° 47′ the Spaces of Time between Obſ. 1, and 2, and between 2 and 3, this diurnal Motion of Mars in the Orbit 19′ 36″ being given, make ab 19′ 55″, and bc 18′ 42″; the Diſtances, $a\mu$, $b\mu$ and $c\mu$ remain the ſame as in the former Calculations. Theſe being granted, I found in the firſt Place by the Triangle $ab\mu$, $\mu\chi$ 6′ 22″, and $b\chi$ 1′ 37½″.

	h	′
To which anſwers — — — — — — — — —	1	59
Which being ſubtracted from *Oct.* 11. — — —	10	6 *Veſp.*
Leaves the Time of the leaſt Diſtance, *Oct.* 11. — —	8	7 *Veſp.*

Then by the Triangle $bc\mu$, I found $\mu\chi$ — — — — 6′ 21″
And

	h	′
And $b\chi$ 1′ 38″, which anſwer in Time to — — —	2	0
Which being ſubtracted from the Time of Obſ. 2. *Oct.* 11. — — — — — — — — —	10	6 *Veſp.*
Leave the Time of the leaſt Diſtance, *Oct.* 11. — —	8	6 *Veſp.*

Thus theſe Calculations agree very well together, and with the mean of the former Calculations.

§. 7. If from μ a Right Line $\mu\, d$ be drawn, which with the Line $\chi\,\mu$, perpendicular to the Orbit of Mars, makes an Angle at μ equal to the Angle of the Orbit of Mars with the parallel of the Ecliptick, $d\,\mu$ will be perpendicular to the Ecliptick. I found this Angle at firſt to be 10° 39′ (§. 2.); and then the Diurnal Motion of Mars being corrected, I found it to be 10° 47′ (§. 6.). Now in the Rectangular Triangle $d\,\chi\,\mu$, beſides the Angles, the Side $\chi\,\mu$ is known to be 6′ 22″, and the other Sides are ſought. Aſſuming therefore the Angle $\chi\,\mu\,d$ 10° 39′, the Side $\chi\,d$ is found to be 1′ 12″. But if I make uſe of the more correct Angle 10° 47′, the Side $\chi\,d$ will be 1′ 13″.

		h	′	″
To which is anſwerable in Time — — — — —		1	29	0
Which being added to the time of the leaſt Diſtance *Oct.* 11. — — — — — — — — — —		8	7	0
Gives the true time of ☌ ♂ and μ ♓ in the Ecliptick *Oct.* 11. — — — — — — — —		9	36	0
$d\,\mu$, or the difference of the Latitude of Mars, from the Latitude of the Star in ☌ in the Ecliptick is found — — — — — — — — — —		0	6	29
	°	′	″	
Which being ſubtracted from the Latitude of the Star — — — — — — — — —	3	4	25	*S.*
Leaves the Latitude of Mars — — — — —	2	57	56	*S.*
The Longitude of Mars is equal to the Longitude of the Star according to the accurate *Britannick Catalogue* — — — — — — — —	♈ 19	25	40	

		h	′	″
§. 8. At the Time of the Conjunction of Mars and μ ♓ in the Ecliptick, at *Berlin*, true time *October* 11. — — — — — — — — —		9	36	0
And at *Bologna*, mean Time *Oct.* 11 — — — —		9	14	0
By *Manfredi*'s Ephemerides the Longitude of Mars is found to be — — — — — — —	♈	19°	14	40
Which falls ſhort of the Obſervation — — — —			11	0
Ghiſler's Ephemerides make the Longitude of ♂ —	♈	19	4	0
Almoſt 22′ ſhort of the Obſervation, and the Ephem. of *Deſplaces* make it — — — — — —	♈	19	25	0

Agreeable enough to the Obſervation.

Manfredi's

	°	′	″
Manfredi's Ephemerides make the S. Latitude of ♂	2	57	0
that is, about 1′ less than the Latitude observed;			
According to *Chisler*'s it is — — — — — —	2	57	30
And according to those of *Desplaces* — — — —	2	59	30

The Place of Mars in opposition to the Sun.

		°	′	″
§. 1. At the Time of the Conjunction of Mars and μ ♓ in the Ecliptick, the Place of the Sun is found by *Manfredi*'s Tables to be — — — — —	6 ♎	18	46	21
At which time the Longitude of Mars was to —	0 ♈	19	25	40
And therefore ♂ was almost in opposition to the Sun, and only 39′ 19″ from the opposite Place to the Sun.				
The diurnal Motion of the Sun was — — — —		0	59	34
And the diurnal Motion of ♂ retrograde in the Eclipt. —		0	19	15
The Sum gives the diurnal Motion of ☉ from ♂ — —		1	18	49

	h	′	″
§. 2. As 1° 18′ 49″ the diurnal Motion of ☉ from ♂ is to 24 hours, so is 39′ 19″, the Distance of ♂ from the opposite Place to ☉ to — — — — — —	11	58	0
Which being added to the true Time of ☌ ♂ μ ♓ in the Ecliptick *Oct.* 11. — — — — — —	9	36	0
Makes the Time of the Opposition of Mars and the Sun, at *Berlin*, *Oct.* 11. true time — — — —	21	34	0
Subtract the Equation — — — — — — —	0	13 ½	
There will remain mean Time at *Berlin*, *Oct.* 11. —	21	20 ½	
For the Difference of Meridians, between *Bologna* and *Berlin*, subtract — — — — — — —	0	8 ½	
Remains mean Time at *Bologna*, *Oct.* 11. — — —	21	12	0

		°	′	″
§. 3. As 24^h to 19′ 15″ the diurnal Motion of Mars in Longitude, so 11^h 58′, the time between ☌ ♂ and μ ♓ in the Ecliptick, and the Opposition of ☉ and ♂, to		0	9	36
Which being subtracted from Longitude ♂ in ☌ ♂ and μ ♓ — — — — — — — —	0 ♈	19	25	40
Leaves Longitude of ♂ in ☍ ☉ — — — —	0 ♈	19	16	4
The Place of ☉ by *Manfredi*'s Tables *Oct.* 11. 21^h 12′ mean Time at *Bologna* is found — —	6 ♎	19	16	3

A Difference of only 1″ (besides the Semi-circle) from the Place of Mars, which may safely be neglected.

	°	′	″
§. 4. As 24^h to 3′ 40″, the diurnal Motion of Mars in Latitude, so 11^h 58′ to — — — —	0	1	50
Which being subtracted from Latitude ♂ in ☌ ♂ and μ ♓ in the Ecliptick — — — — — —	2	57	56 S.
Leaves Latitude ♂ in ☍ ☉ — — — — — — —	2	56	6 S.

Observations on Mars about

Mars was among the Stars *e*, ε and ζ of *Pisces*, and other smaller Stars; from which I often measured the distances of Mars, with 3 different

different Telescopes, of 7, 9 and 2 feet, and once with a Telescope of 18 feet. By the longer Telescopes more accurate Distances may be taken; but because they do not comprehend any great Space, I could measure only the smaller Distances by them. Large Distances might indeed be observed by the Telescope of 2 feet, but sometimes a Doubt of 1 or 2 Minutes may creep in, especially if the Distances are too large for the Capacity of the Telescopes Such Errors are most seen when the Situation of the Stars is drawn upon Paper, and the Distances of a Planet from different Stars, do not intersect each other in one Point. I have taken out the Stars, from which I measured Mars, from the *Britannick Catalogue*, and by the Distances of Mars from these Stars, I have traced out the Place of the Planet by means of a Circle. I shall first exhibit the Distances taken, and then the Places of Mars found thereby. Where it is to be observed, that I have made use of a Delineation, in which the Magnitudes of Degrees and Distances of the Stars were double of those in the Scheme.

his second Station November 1736.

Fig. 79.

Styl. nov.	True time Vesp.			Parts of the Micrometer	Value of parts of the Microm.
	h '				° ' ''
Oct. 27.	8. 58.	♂ *e* ♓	Telef. 7 feet	121.	0. 48. 24.
Oct. 29.	8. 30	♂ *e* ♓	Telef. 7 feet	6.	24. 48.
	8. 38.		Telef. 9 feet	83.	24. 43.
Nov. 1.	11. 6.	♂ *e* ♓	Telef. 9 feet	38.	11. 18.
	11. 10.		Telef. 7 feet	28.	11. 12.
Nov. 5.	7. 22	♂ *a.*	Telef. 7 feet	34 ½.	13. 48.
	7. 26.	♂ *e* ♓	. . .	100 ½.	40. 12.
	8. 14.	♂ *c.*	. . .	105.	42. 0.
	8. 21.	♂ *a.*	Telef. 9 feet	44.	13. 6.
Nov. 6.	7. 28	♂ *a.*	Telef. 7 feet	17.	6. 48.
	7. 34.	♂ *e* ♓	. . .	116.	46. 24.
	7. 40.	♂ *c.*	. . .	110.	44. 0.
		or		110 ½.	44 12.
	7. 44.	♂ *a.*	Telef. 9 feet	23 ½.	6 59.
Nov. 7.	6 4	♂ *a.*	Telef. 18 feet	16.	2. 17.
	7 47.	♂ *a* diffic.	Telef. 7 feet	5 ½.	2. 12.
	7. 50.	♂ *e* ♓	. . .	129.	51. 36.
	7. 53.	♂ *c.*	. . .	118.	47. 12.
Nov. 12.	9. 19.	♂ *a*	Telef. 7 feet	52.	0. 20. 48.
	9. 27.	♂ *e* ♓	. . .	172.	1. 8. 48.
	9. 38.	♂ *c.*	. . .	165	1. 6. 0.

Styl.

Styl. nov.	True time Veſp. h	'			Parts of the Micrometer	Value of parts of the Microm. ° ' ''
Nov. 13.	7.	32.	♂ a.	Teleſ. 9 feet	77.	0. 22. 56.
	7.	36.	♂ a.	Teleſ. 7 feet	58.	0. 23. 12.
	7.	40.	♂ e ♓.	. . .	175.	1. 10. 0.
	7.	44.	♂ c.	. . .	171.	1. 8. 24.
Nov. 15.	7.	2.	♂ a.	Teleſ. 7 feet	72.	0. 28. 48.
	7.	9.	♂ e ♓.	. . .	179.	1. 11. 36.
	7.	13.	♂ c.	. . .	186 ½.	1. 14. 36.
	7.	18.	♂ a.	Teleſ. 9 feet	96.	0. 28. 35.
Nov. 26.	6.	11.	♂ e ♓.	Teleſ. 2 feet	91.	1. 22. 6.
			♂ ζ ♓.	. . .	106.	1. 35. 38.
			♂ ε ♓.	. . .	94.	1. 24. 48.
			♂ c.	. . .	143.	2. 9. 2.
			♂ a.	. . ♓	113.	1. 41. 57.
			♂ e ♓.	. . .	92.	1. 23. 0.
	6.	32.	♂ a.	. . .	103.	1. 32. 55.
Nov. 28.	6.	43.	♂ e ♓.	Teleſ. 2 feet	104.	1. 33. 50.
	6.	46.	♂ ζ ♓.	. . .	82.	1. 13. 59.
	9.	34.	♂ ε ♓.	. . .	103.	1. 32. 55.
	9.	37.	♂ e ♓.	. . .	105.	1. 34. 44.
	9.	41.	♂ ζ ♓.	. . .	82.	1. 13. 59.
			better		81.	1. 13. 5.
Dec. 3.	9.	41.	♂ e ♓.	Teleſ. 2 feet	160.	2. 24. 23.
			♂ ε ♓.	. . .	157.	2. 21. 40.
	9.	52.	♂ ζ ♓.	Teleſ. 7 feet	56.	0. 22. 24.
	10.	1.	♂ ζ ♓.	Teleſ. 9 feet	75. ½	22. 29.
			or		76.	22. 38.
Dec. 6.	5.	33.	♂ ε ♓.	Teleſ. 2 feet	201.	3. 1. 22.
	5.	39.	♂ e ♓.	. . .	204.	3. 4. 4.
	5.	44.	♂ ζ ♓.	. . .	50.	0. 45. 8.
			♂ ζ ♓.	Teleſ. 7 feet	113 ½.	0. 45. 24.
	5.	57.	♂ ζ ♓.	Teleſ. 9 feet	153.	0. 45. 34.

Theſe Diſtances are always to be underſtood from the Centre of Mars, eſpecially by the longer Teleſcopes.

Now follow the Places of Mars deduced from the Diſtances enumerated, and his Places taken from the different Ephemerides, to ſhew the Agreement or Diſagreement between the Calculations and the Obſervation.

Styl.

Styl. nov.	True Time Vesp. h	'		Longitude of Mars. °	'	"	Latitude of Mars. °	'	"
Nov. 5.	8.	18.	Observation.	♈ 13.	37.	0.	1.	17.	0. S.
			Manfredi.	13.	26.	—	1.	16.	30.
			Ghisler.	13.	42.	—	1.	20.	30.
			Desplaces.	13.	35.	30.	1.	17.	30.
Nov. 6.	7.	26.	Observation.	♈ 13.	32.	0.	1.	13.	15. S.
			Manfredi.	13.	22.	—	1.	13.	—
			Ghisler.	13.	37.	30.	1.	16.	—
			Desplaces.	13.	31.	—	1.	14.	—
Nov. 7.	7.	50.	Observation.	♈ 13.	27.	40.	1.	9.	30. S.
			Manfredi.	13.	17.	40.	1.	9.	20.
			Ghisler.	13.	32.	40.	1.	12.	—
			Desplaces.	13.	27.	—	1.	10.	—
Nov. 12.	9.	28.	Observation.	♈ 13.	20.	0.	0.	50.	40. S.
			Manfredi.	13.	8.	—	0.	51.	—
			Ghisler.	13.	19.	30.	0.	53.	—
			Desplaces.	13.	18.	—	0.	51,	30.
Nov. 13.	7.	38.	Observation.	♈ 13.	20.	30.	0.	48.	0. S.
			Manfredi.	13.	9.	15.	0.	48.	—
			Ghisler.	13.	19.	—	0.	49.	30.
			Desplaces.	13.	19.	—	0.	48.	—
Nov. 15.	7.	12.	Observation.	♈ 13.	23.	30.	0.	42.	0. S.
			Manfredi.	13.	13.	—	0.	41.	—
			Ghisler.	13.	21.	30.	0.	43.	—
			Desplaces.	13.	21.	30.	0.	41.	—
Nov. 26.	6.	20.	Observation.	♈ 14.	35.	0.	0.	9.	0. S.
			Manfredi.	14.	26.	—	0.	9.	—
			Ghisler.	14.	37.	—	0.	12.	—
			Desplaces.	14.	34.	—	0.	9.	30.
Nov. 28.	9.	35.	Observation.	♈ 14.	57.	0.	0.	3.	30. S.
			Manfredi.	14.	49.	30.	0.	4.	—
			Ghisler.	14.	59.	—	0.	7.	—
			Desplaces.	14.	57.	30.	0.	5.	—
Dec. 3.	9.	48.	Observation.	♈ 16.	1.	0.	0.	6.	30. N.
			Manfredi.	15.	57.	—	0.	7.	40.
			Ghisler.	16.	3.	—	0.	5.	—
			Desplaces.	16.	2.	—	0.	7.	—
Dec. 6.	5.	46.	Observation.	♈ 16.	46.	—	0.	16.	— N.
			Manfredi.	16.	40.	30.	3.	13.	30.
			Ghisler.	16.	47.	30.	0.	10.	30.
			Desplaces.	16.	50.	33.	0.	13.	30.

On the 2 last Days, namely the third and especially the sixth of *December*, the Places of Mars, deduced from Observation, are doubtful; these therefore may be rejected.

The Places of the fixt Stars in the Scheme annexed, from the *Britannic Catalogue*, to the Beginning of the Year 1690, are taken without any Reduction: therefore to the Longitudes of Mars, exhibited by the Figure, must be added 39′ or 39′ 5″ for the Motion of the fixt Stars in 46 Years and about 10 or 11 Months.

The Observations of *November* 9 are omitted above, and I shall add them here, with the Place of Mars deduced from them.

Styl. nov.		True time Vesp.					Parts of the Micrometer	Value of Parts of the Microm.		
		h	′							
Nov.	9.	9.	28.	♂ a.	Telef.	7 feet	25.	0.	10.	0.
		9.	30.	♂ e ♓	. .	.	152.	1.	0.	48.
		9.	34.	♂ c.	. .	.	136.	0.	54.	24.
		9.	41.	♂ a.	Telef.	9 feet	32 ½.	0.	9.	40.

Styl. nov.		True time Vesp.			Longitude of Mars.				Latitude of Mars.			
						°	′	″	°	′	″	
Nov.	9.	9.	34.	Observation.	♈	13.	22.	20.	1.	1.	30.	S.
				Manfredi.		13.	22.	—	1.	0.	30.	
				Ghisler.		13.	11.	—	1.	3.	30.	
				Desplaces.		13.	25.	—	1.	2.	30.	

An Observation of the Transit of Mercury *over the* Sun, Oct. 31. 1736, *by Mr* George Graham, *F. R. S. made in* Fleetstreet, London. No. 446. p. 102. July, &c. 1737.

XXV. 1.

Apparent Time.

h	′	″	
9	22	00	*Mercury* not yet seen, then Clouds.
9	25	37	I first saw *Mercury* for a few Seconds, and judged he was got entirely within the *Sun*'s Disk, or perhaps a little more; then Clouds again, with some Intervals of a few Moments between, which allowed us a Sight of *Mercury* about three or four several times; then quite cloudy till near 12, when we had a Sight of the *Sun* for a few Minutes, and took his *Transit* upon the Meridian; at which time we judged

Mercury

h	′	″	
			Mercury to be about two of his Diameters, or a little more, within the *Sun*'s Disk, and a little past the vertical Line.
12	10	27	We had again a Sight of the *Sun*, but *Mercury* was gone off.

2. The Sky was very clear, and the Air not disturbed by any Wind. *Roversius* happened to be the first who perceived the Planet at the edge of the Sun at $22^h\ 8'\ 37''$ *p. m.* and it's inner Contact with the Sun at $22^h\ 11'\ 12''$ we made use of Clocks regulated by a Meridian Line drawn by *Zanotti*, by equal Altitudes of the Sun in the Morning and Evening, taken several times.

—*Observed at* Bologna, *by* Eustachius Manfredi, *F. R. S.* Ibid. p. 103.

The Planet was perceived something later by other Observers in the Limb of the *Sun*. For my own part, I did not perceive it, with a Telescope of 11 feet, till $22^h\ 9'\ 5''$ when it had plainly entered the *Sun*, and I estimated it's inner Contact to be at $22^h\ 10'\ 53''$ But the former Observation is far more certain, as being made with a better Instrument. But since from the times of the Egress of the Planet, which will afterwards be mentioned, it is manifest that it's Body spent $3'\ 16''$ in going out, if we subduct so much from the time of the inner Contact observed by *Roversius*, the exterior Contact, or first Appulse of *Mercury* to the *Sun* will be still more certain, at $22^h\ 7'\ 56''$.

The subsequent Observations tended to find some Points of the Path, which the Planet was seen to describe in the *Sun*. We referred each of those Points to a Horary Circle, and also to a Parallel drawn through the Centre of the *Sun*, according to *Cassini*'s Method, marking the times by the Clock, at which the Limbs of the *Sun* and *Mercury* passed over the Horary Thread of the Micrometer. *Zanotti* obtained many of these Points with a Telescope of 8 feet; and I obtained 1 or 2 with a Tube of 6 feet, to which an excellent Micrometer was fitted, made by *Jo. Jacobus Marinonius*. *Roversius* and *Thomas Perellus, M. D.* determined some other Points with the same. Hither also belongs the Observation made by *Perellus* on the Meridian, with a mural Semi-circle, by which Observation the right Ascension of the Planet was found to be $11''\frac{1}{2}$ of Time greater, and the Declination $58''\frac{1}{2}$ of Time less than of the Centre of the *Sun*. Besides *Zanotti* took upon himself to describe the Positions of the more remarkable Spots, many of which were seen that Day in the *Sun*. It was easy to distinguish between the Planet and those Spots, because it was exactly round, and very black, and surrounded with no Ring.

Franciscus Algarottus, F. R. S. observed the Beginning of the Egress with a Telescope of 8 feet at $50'\ 1''$ *p. m.* the End at $53'\ 6''$. I observed the Beginning with a Telescope of 11 feet at $51'\ 7''$ the End at $53'\ 44''$. *Roversius* observed only the End with a Telescope of 14 feet at $54'\ 1''$; but these Observations are not very certain, because the Telescopes were but indifferent, and the Wind rising about that Time shook them a little. Therefore we must prefer the Observation which was made with a Telescope of 22 feet, by *Franciscus Vandellius*, Professor of Military Architecture. He determined the inner Contact at 50′ 50″, and the outer at 54′ 6″, whence the Stay of the Planet in the Limb was 3′ 16″, and the Time of the Egress of the Centre 52′ 28″, which, according to my Observation, should be 52′ 25″.

Thus much for the Observations themselves; I shall now mention what I have deduced from comparing them with *Zanotti*. Assuming the Diameter of the *Sun* to be 32′ 34″, and the Time of it's passing thro' the Horary Circles 2′ 17″ (which Numbers are set down in the Tables of the Modern Astronomers, and confirmed by the Observations themselves) we have set down those Points by observing the Bounds of the planetary Path; and as an Account of the small Fallacies of the Observations they would all fall exactly upon the same Right Line, we thought none more proper to reconcile them, than if we determined a Perpendicular Line drawn from the Centre of the *Sun* to the Path of the Planet, to contain an Angle of 23° 40′ to the East with the Horary Circle; and if we settled the Length of that Perpendicular from the Centre to the Path to be 13′ 58″ to the North. From these we have calculated all the rest after the following manner.

Fig. 80.

h	′	″	
27	7	56	Ingress of *Mercury* into the Disk of the *Sun*.
22	9	34	Ingress of the Centre.
22	11	12	Total Ingress.
0	50	50	Beginning of the Egress.
0	52	28	Egress of the Centre.
0	54	6	Total Egress.
2	42	54	Stay of the Centre of *Mercury* in the Disk of the *Sun*.
1	21	27	Half the Stay.
23	31	1	Time of the middle Transit.
°	′	″	
23	40	0	Angle of the Perpendicular Line to the Path of the Planet with the Horary Circle determined by the Observations, to the East.
105	48	0	Angle of the Ecliptick with the Horary Circle from the Astronomical Tables, to the East.
82	8	0	Is therefore the Angle of the Ecliptick with the Perpendicular to the apparent Path of *Mercury*.
7	52	0	Is also the Angle of the apparent Path with the Ecliptick.

Distance

°	′	″	
0	13	58	Distance of the Path from the Centre of the *Sun* to the North, found by Observations.
0	16	17	Semi-Diameter of the *Sun*.
0	16	45	Length of the Path within the *Sun*'s Disk.
0	8	22	Half of it's Length.
0	6	10	Horary Motion of *Mercury* in the apparent Path.
0	6	6	Apparent Horary Motion in the Eclipticк.

0	1	58	Portion of the Path between the middle of the Transit and the Conjunction.
0	10	20	Portion of the Path from the Ingress to the Conjunction.
0	6	24	Portion of the same from the Conjunction to the Egress.
0	10	15	Difference of Longitude of *Mercury* and the *Sun* in the Ingress.
0	6	21	Difference of Longitude in the Egress.

h	′	″	
0	19	2	Time from the middle of the Transit to the Conjunction.
True Time			Time of the Conjunction at *Bologna*
23	50	3	
Mean Time			
23	34	25	

°	′	″	
			of *Scorpio*.
19	23	30	Longitude of the *Sun* and *Mercury* at the very Conjunction, by *Cassini*'s Tables.

This Longitude agrees within 4″ with the Observation made by *Petrus Lilius*, *J. V. D.* the same Day by the Meridian Gnomon of *St Petronius*.

0	12	37	Latitude of *Mercury* in the Northern Ingress.
0	14	54	Latitude in the Northern Egress.
0	0	50½	Horary Motion into the Latitude.
0	14	1	Latitude in the Northern Conjunction.

h	′	″	
16	39	0	Space of Time from the Transit of *Mercury* thro' the ascending Node to the Conjunction.
7	11	0	True Time } Time of the Transit thro' the Node.
6	55	0	Mean Time }

Motion

°	′	″	
4	15	47	Motion of *Mercury* ſeen in the Orbit out of the *Sun* at the Diſtance of 16^h 39′ about this Time, or Argument of Latitude in Conjunction.
4	13	56	The ſame Motion reduced to the Eclíptick.

°	′	″	of *Taurus*
15	9	34	Place of the aſcending Node of *Mercury* ſeen out of the *Sun*.

Log.	449301	Diſtance of *Mercury* from the *Sun* at the Time of Conjunction by *Caſſini*'s Tables.
Log.	499503	Diſtance of the Earth from the *Sun* by the ſame Tables.

°	′	″	
0	30	31	Latitude of *Mercury* in Conjunction ſeen out of the *Sun* North.
6	51	0	Inclination of the Orbit of *Mercury* to the Ecliptick.

h	′	″	
0	3	16	Time from the inner Contact of *Mercury* to the outer one in the Egreſs, by Obſervation.
°	′	″	
0	0	20	Portion of the Path gone through at this Time by *Mercury*.
58	50	0	Angle of the Path with the Semi-Diameter of the *Sun* in the Egreſs.
0	0	10	Apparent Diameter of *Mercury* as nearly as poſſible.

—Obſerved at Wittemburg, Nov 11. 1736. *by* J. Frid. Weidler, *F. R. S.* Ibid. p. 110. Fig. 81.

3. *Mercury* appeared within the *Sun*'s eaſtern Limb (as in the Scheme)

h	′	″		
10	49	20	- at -	1
11	36	0	- ☿ abt. -	2
11	52	20	- at -	3
12	2	30	- - - -	4
	4	30	- - -	5
	44	20	- - - -	6
	52	45	- - -	7

A Tranſit of Mercury *under the* Sun Oct. 31. 1736, *by* J. Bevis, *M.D.* No. 471. p. 622. *Read* December 15, 1743.

XXVI. I went to *Greenwich*, *Oct.* 31. 1736, early in the Morning, to obſerve the Conjunction of *Mercury* with the *Sun*, being invited by Dr *Halley*, who condeſcended to aſſiſt me. The Sky was very clear at the riſing of the *Sun*, but the Wind was very briſk. Dr *Halley* was in the ſame Room with me, and was pleaſed to attend the Clock, whilſt I took the care of a Teleſcope of 24 feet. I began to obſerve

 about

about 8, being afraid of missing the Ingress, if there should be any Error in the Calculation: But I could see nothing in the *Sun* besides Spots. The Sky was presently after covered with Clouds. About ten the Clouds opened a little, and gave me the first Opportunity of seeing *Mercury* under the *Sun*, which was taken away in a Moment by very thick Clouds. I had not waited long before I saw him again, and shewed him to Dr *Halley* on the Face of the *Sun*. Then came a long Succession of dark Clouds. About Noon it began to be clear, and Dr *Halley* observed the *Sun* culminating with his great Mural Quadrant. I had now great Hopes of seeing the Egress of *Mercury*, and renewing my Application, made the following Observations.

	h	′	″	
Oct. 30.	23	50	45	The Centre of *Mercury* was 1′ 8″ distant from the *Sun* by the Micrometer.
31.	0	2	39	*Mercury* was distant from the *Sun*'s Limb about his own Diameter.
		7	4	The Centre was judged by the Eye to be gone out.
		8	33	The exterior Contact, the Sky being very clear.

XXVII. I have sent you a Scheme of the Phase of the *Sun*, *Oct.* 31st, $11^h\ 5'\ 12''$ as taken by my Telescope, which is a very good one of 10 feet; but as I had neither Cross-Hairs, Micrometer, or other exact Instruments, the Observation may not be very exact: Besides, I had only a Glimpse of the *Sun* for 7 or 8 Minutes.

A Transit of Mercury *over the* Sun, Oct. 31. 1738, *by* John Huxham *M. D. F. R. S* Fig. 82.

No. 459. p. 645.

XXVIII. *April* 21, 1740, I had an Opportunity to observe *Mercury*, then near his descending Node, transiting the *Sun*'s Disk. Being advertised by Dr *Halley*'s Calculations, that the former Part of this Transit would be visible in our Horizon, I was resolved to observe it in the best manner I could, with those few Instruments I was furnished with; which were only those I had received from my Predecessor Mr *If. Greenwood*, and are the same that are mentioned by the late Mr *Thomas Robie* * being a 24 Foot Telescope, another of 8 Foot, and a brass Quadrant of 2 Foot *Radius*, fitted with telescopic Sights, and having Cross-Hairs fixed in the *Focus* of the Glasses. All these I got in Readiness, being the more desirous to make this Observation, because *Mercury* had never as yet been seen entering upon or going off the *Sun*'s Limb at his descending Node, and this *Transit* ought to be invisible to *Europe*. The better to observe *Mercury*'s Ingress on the *Sun*, I determined to make use of my 24 Foot Tube, while an Assistant I had with me used that of Eight Foot: After which I proposed, in order to find out his Path in the *Sun*, to observe the Passages of *Mercury* and the *Sun*'s Limbs by an horizontal and vertical Hair in the Telescope of the Quadrant; and

A Transit of Mercury *over the* Sun, April 21. 1740. *by Mr* John Winthorp, *Hollisian Prof. Math. and Astron. at* Cambridge *in* New-England. No. 471. p. 572. *Read* Nov. 3. 1743.

* *See* Vol. VI. p. 173.

I chose

I chose rather to deduce *Mercury*'s Right Ascensions and Declinations by Calculation from hence, than to observe them immediately in the common way of placing one of the Cross-Hairs parallel to the Equator, *&c.* because, as the *Sun* was likely to be low before *Mercury* made his Entrance, Refraction would have caused considerable Errors in the Places of *Mercury* determined in this Manner. Having no Clock, I was obliged to make use of my Pocket-Watch, which I know to be a good one; and by this it was easy to distinguish Time to a Quarter of a Minute, which would have served pretty well for the Ingress of the Planet. But as it was by no means sufficient for those other Observations I designed to make, I procured another Watch, which shewed Seconds; and both these Watches I adjusted to the apparent Time, by several Altitudes of the *Sun* taken with the Quadrant before the *Transit* began; and by Altitudes taken the next Day, I found that the Watches had kept time exactly enough. I expected that the Centre of the Planet would enter upon the *Sun* at $5^h\ 2'$; but being apprehensive that he might be earlier than the Calculation, I, for some time before that, with my 24 Foot Tube directed to the *Sun*, kept my Eye fixed on that Part of his Limb where the Planet was to enter, as steadily as I could for the Wind, which then blew fresh. This Precaution was not needless; for, at $4^h\ 54'\ 59''$, I perceived that *Mercury* had made a Impression on the *Sun*'s Limb; by the Quantity of which I concluded, that almost $\frac{1}{4}$ of his Diameter might be entered. After I had beheld this very plainly about a Minute, a small Cloud covered the *Sun* near $3'$; which then clearing off, and the Sun shining very bright, as before, I had again a distinct View of the Planet, and saw much more than half his Body on the *Sun*. I continued to see him till $5^h\ 0'\ 40''$, at which Time he seemed to be gotten almost wholly within the *Sun*; for he appeared now very near round, though I could not yet discern the *Sun*'s Light behind him. By the shaking of the Tube, I unfortunately missed the Moment of his interior Contact with the *Sun*'s Limb, but am certain it could be but very little later than this; for I presently after saw him fairly within the *Sun*. Upon which, I repaired to my Quadrant; but this being at my Lodgings, at some Distance from the long Telescope with which I observed the Ingress, and which I had no Convenience for raising nearer Home, almost half an Hour slipped away before it was possible for me to begin my Observations. I began them as soon as I could, and continued them till Sun-set, excepting when I was interrupted by the Clouds; and I observed sometimes one and sometimes the other Limb of the *Sun*, as I found it most convenient. It will be needless, I suppose, to give a Detail of all the Observations I made; I shall therefore select Two or Three, which I look upon as most exact, and most suitable to my present Purpose. One was as follows:

The

	h	′	″
The *Sun* at the Horizontal — — — —	5	37	59
The *Sun* at the Vertical — — — — —	0	39	1
Mercury at the Vertical — — — — —	0	39	16
Mercury at the Horizontal — — — —	0	40	1

This Observation gave me the *Azimuth* and Altitude of *Mercury* at his Passage by the vertical Hair; from whence I computed his Right Ascension and Declination, and from thence his Longitude and Latitude. The Method of obtaining which being sufficiently known, I shall say nothing upon it, but only mention the Result of the Numbers, which was, that at $5^h\ 59'\ 16''$, when *Mercury* passed the Vertical, his Longitude was $12^\circ\ 43'\ 5''$ ♉; and the *Sun* being then in $12^\circ\ 42'\ 27''$ of that Sign, *Mercury* was in consequence of the *Sun*'s Centre, $38''$, his Latitude at the same time being $15'\ 2''$ North. Another Observation was thus:

	h	′	″
The *Sun* at the Horizontal — — — —	6	47	37
The *Sun* at the Vertical — — — — —	0	48	17
Mercury at the Vertical — — — — —	0	48	25
Mercury at the Horizontal — — — —	0	49	24

From hence I concluded, that at $6^h\ 48'\ 25''$ *Mercury* was in Antecedence of the *Sun* $3'\ 57''$ with $14'\ 20''$ North Latitude. I made another Observation after this; but the *Sun* being then very near the Horizon, his Limbs were not well defined, so that I look upon this Observation as much preferable to that. I shall set down only two more, which were made about the middle between these two; and were made by the *Sun*'s upper Limb.

	h	′	″
The *Sun* at the Vertical — — — — —	6	6	56
Mercury at the Vertical — — — — —	0	7	8
Mercury at the Horizontal — — — —	0	8	42
The *Sun* at the Horizontal — — — —	0	9	45

The *Sun* at the Vertical — — — — —	6	17	18
Mercury at the Vertical — — — — —	0	17	29
Mercury at the Horizontal — — — —	0	18	26
The *Sun* at the Horizontal — — — —	0	19	32

At the former of these Observations, *viz.* $6^h\ 7'\ 8''$ I computed the Longitude of *Mercury* to be in $12^\circ\ 42'\ 17''$ ♉, which being taken from the *Sun*'s Place in $12^\circ\ 43'\ 35''$ ♉, leaves $1'$, $18''$ for the Difference of Longitude between the *Sun* and *Mercury*; and his Latitude was then

14′ 47″. At the latter Obſervation, the Difference of Longitude was 1′ 55″, and the Latitude of *Mercury* 14′ 42″.

From theſe Places of *Mercury* it appears, that his Horary Motion in Longitude from the *Sun* was now 3′ 58″; according to which, if we ſuppoſe the central Ingreſs to have been at 4^h 57′, we ſhall find the Difference of Longitude at that time 3′, 20″; and the Semi-diameter of the *Sun* being 15′ 57″, the Latitude of *Mercury* muſt be 15′ 36″. Now the Angle of *Mercury*'s viſible Way with the Ecliptic being, by the Theory of his Motion, 10° 23′, we muſt conclude the former of the obſerved Latitudes about 4″ too ſmall, and the latter as much too large;——an Error very inconſiderable in this kind of Obſervations. From theſe things we may gather by an obvious Computation, that *Mercury* was in Conjunction with the *Sun*, in reſpect of Longitude, at 5^h 47′ with 14′ 59″ North Latitude; and that his neareſt Diſtance to the Centre of the *Sun* was 14′ 44″; and when he was at his neareſt Diſtance, the Difference of his Longitude from the *Sun*'s was 2′ 39″ which he paſſed over in 40′ of Time, and conſequently arrived at the middle of his Courſe in the *Sun* at 6^h 27′: Whence the Semi-duration of the central *Tranſit* was 1^h 30′, and the End at 7^h 57′, an Hour after Sun-ſet.

As to the Place of *Mercury*'s Nodes, the Inclination of his Orbit to the Ecliptic, and the other Elements of his Theory, I pretend not to determine any thing from ſo ſhort a Series of Obſervations as this. I content myſelf with the foregoing Determinations, which, I hope, are not far from the Truth, having taken all the Care I could, both in the Obſervations and Calculations.

Tranſit *of* Mercury *over the Sun*, Oct. 25. 1743. *in the Morning, obſerved at Mr* Geo. Graham's *Houſe in* Fleet-ſtreet. No. 471. p. 578. *Read* Nov. 3. 1743.

XXIX. 1. The Beginning could not be ſeen by reaſon of Clouds, but about 8^h 45′ *Mercury* was ſeen (through a Reflecting Teleſcope three Foot *Focus*, magnifying about 50 times) about four or five of his Diameters within the *Sun*'s Limb.

At Mr *Short*'s Houſe in *Surrey-ſtreet*, *Mercury* was ſeen juſt paſt the interior Contact 8^h 30′ 59″ through a Reflecting Teleſcope two Foot *Focus*, magnifying about 70 times; the Perſon who obſerved it ſays, that the Thread of Light between *Mercury* and the *Sun*'s Limb was ſo ſmall, as ſcarcely to amount to the 20th or 30th Part of *Mercury*'s Diameter.

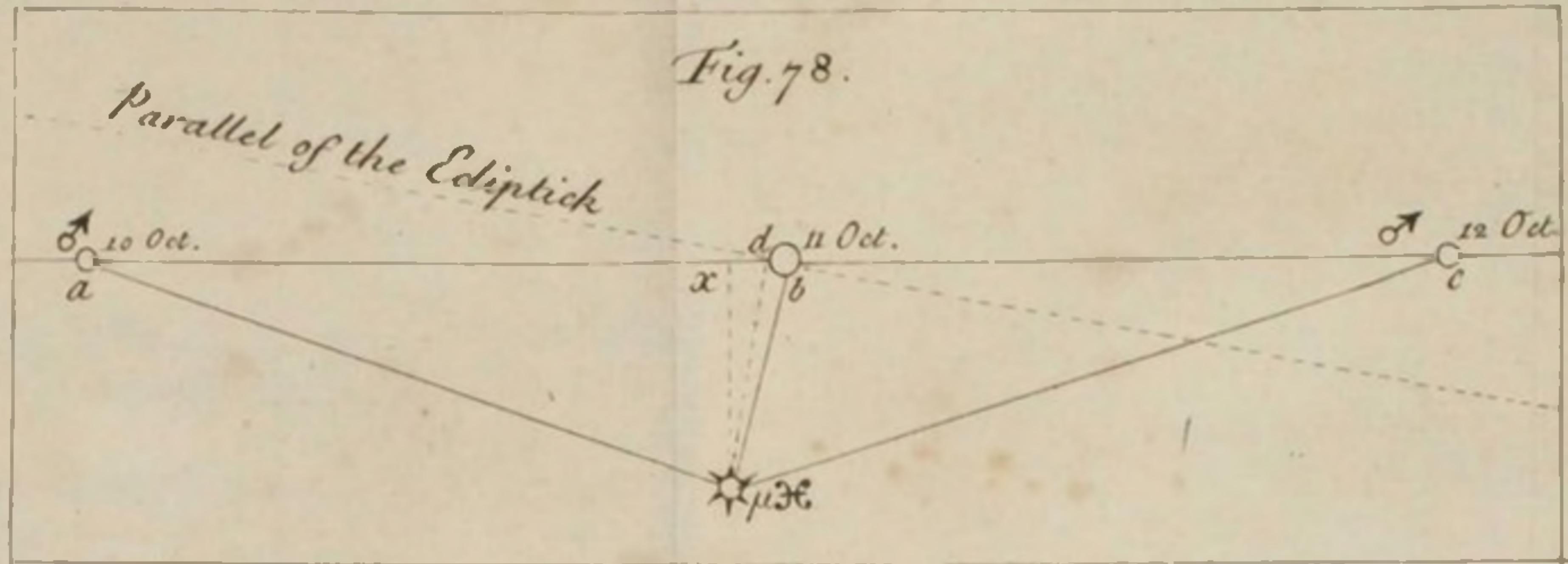
The following Differences of Right Aſcenſion between the Sun's preceding Limb and Mercury, *were taken at Mr* Short's *Houſe.*

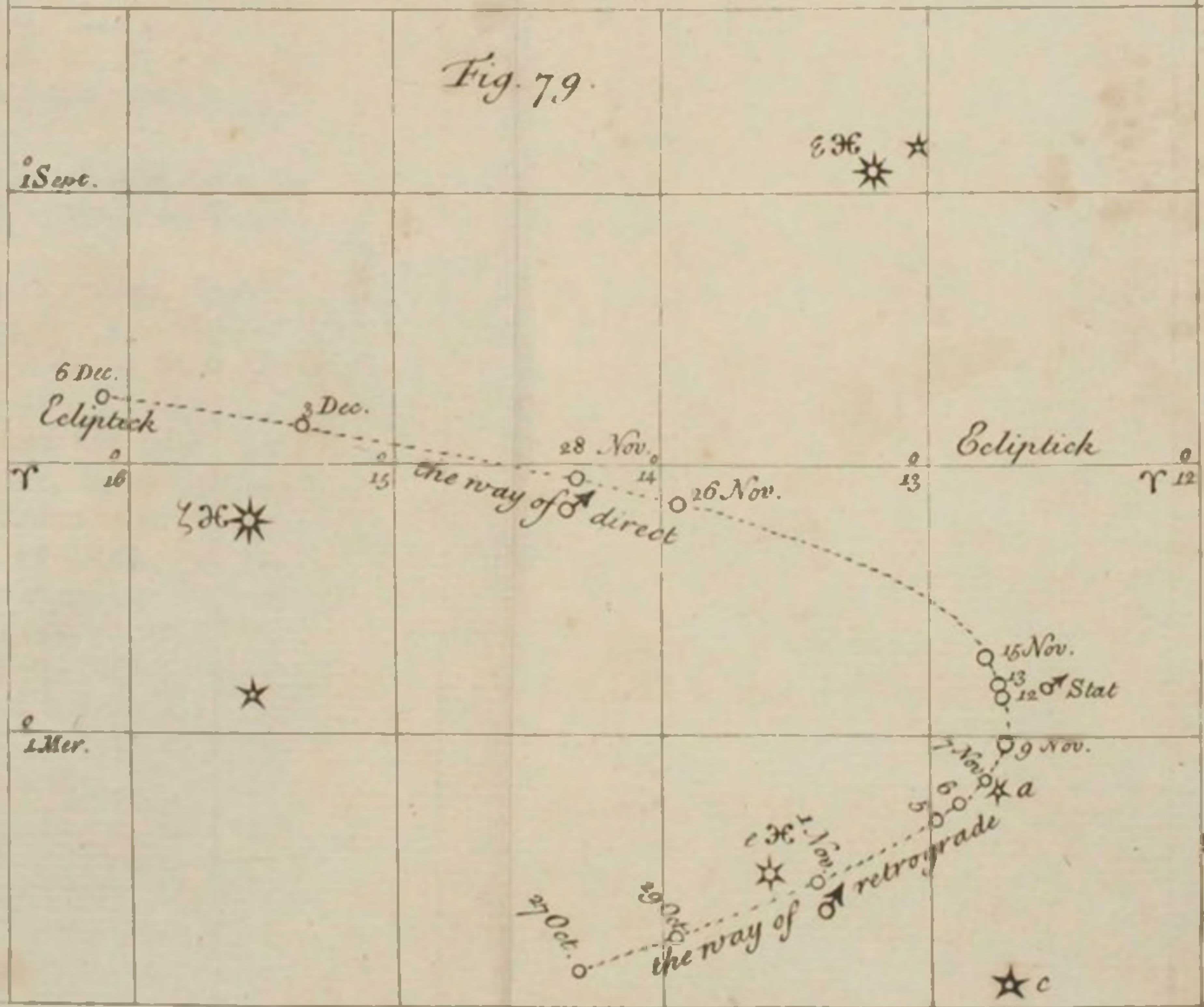
	h	′	″
Sun's preceding Limb touched the Wire at — —	10	58	55
Mercury touched the ſame Wire at — — — —	10	59	40

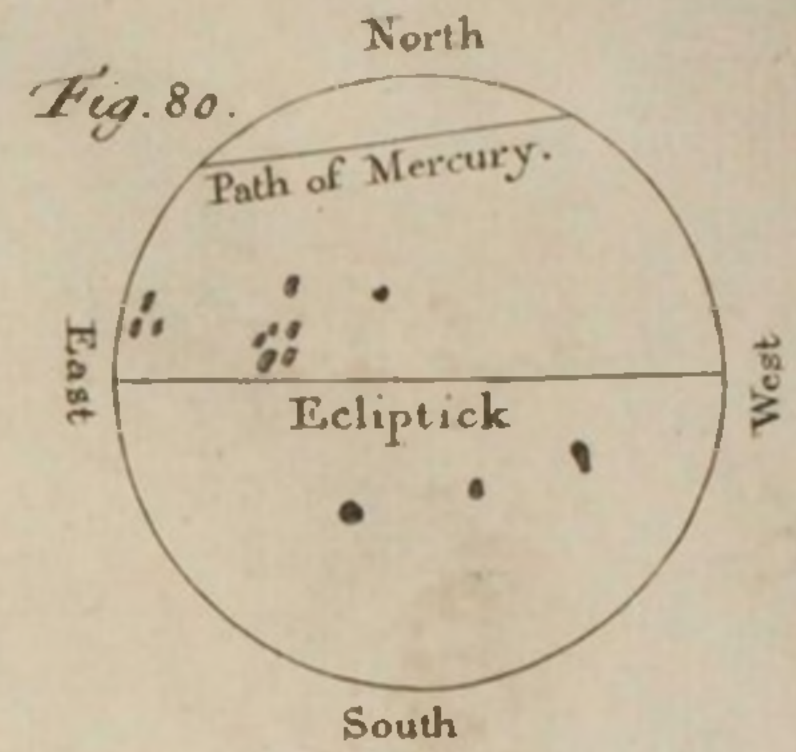
	h	′	″
Sun's preceding Limb touched the Wire at — — —	11	48	4
Mercury touched the ſame Wire at — — — —	11	48	32
Sun's ſubſequent Limb touched the ſame Wire at — —	11	50	20

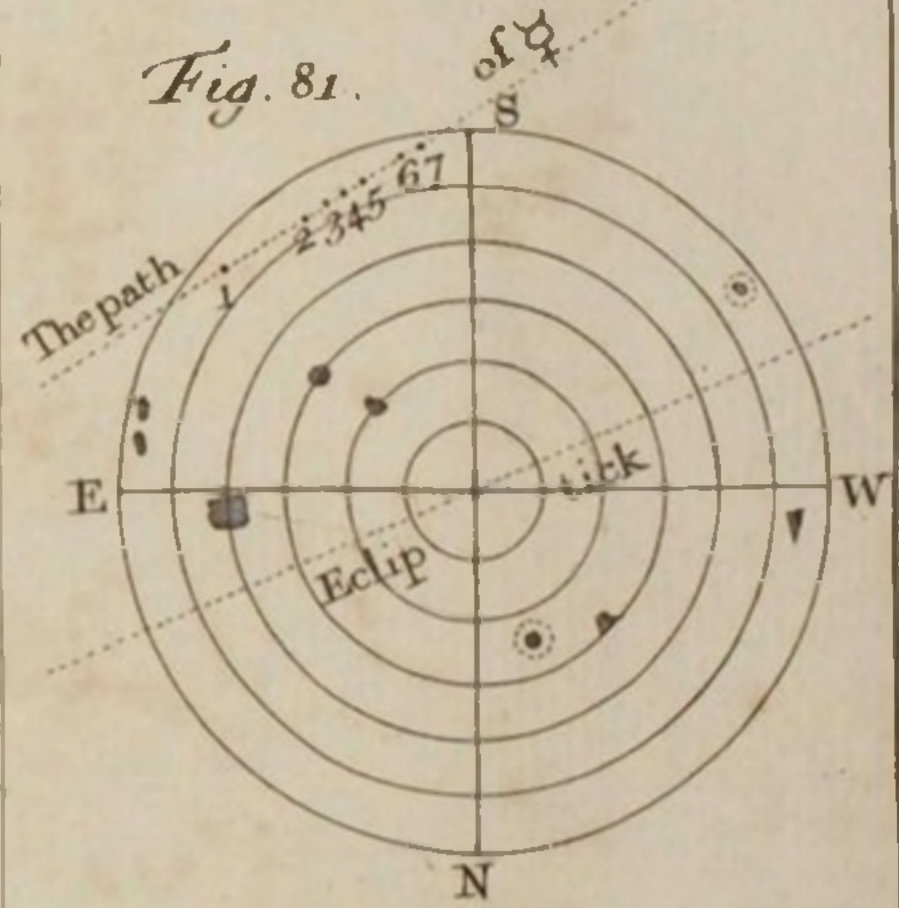
Sun's

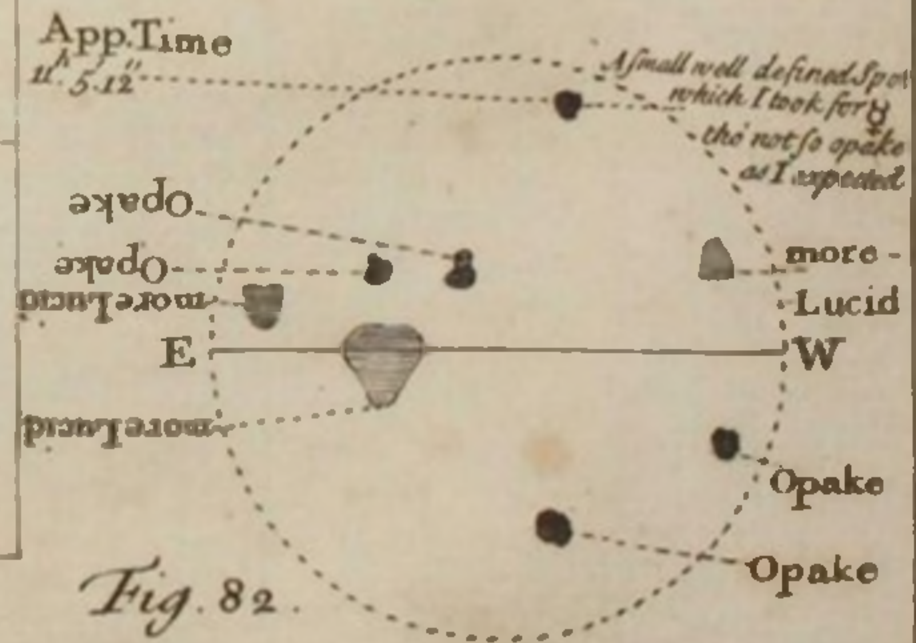
Pl. XIV. Vol. VIII. part 1. pa. 202.











	h	′	″
Sun's preceding Limb touched the Wire at — —	11	49	20
Mercury touched the same Wire at — — — —	11	49	46
Sun's preceding Limb touched the Wire at — —	11	51	9
Mercury touched the same Wire at — — — —	11	51	36
Sun's preceding Limb touched the Wire at — —	12	1	33
Mercury touched the same Wire at — — — —	12	1	57

Mr *Graham* got an Observation madeby a Person in his Neighbourhood, by which it appears, that at $11^h\ 59'\ 50''$, *Mercury* preceded the Sun's Centre $42''$ in Right Ascension.

The Sky clearing up towards one o'Clock, the following Times were observed at Mr *Graham*'s House with great Accuracy.

	h	′	″
Last interior Contact at — — — — — — —	1	0	42
End, or *Mercury* just leaving the Sun's Limb at — —	1	2	16

This last Observation agrees to a Second with the same Observation made by Dr *Bevis* at Mr *Sisson*'s House in the *Strand*.

During the Time of these Observations it blew a violent Gale of Wind, so that both Observers and Instruments were somewhat disturbed.

2. I made this Observation at *London*, in *Beaufort-Buildings*, situated about ½ a Minute West from the Royal Observatory. The Weather was the same as in my former Observation*, only the Wind blew harder, which caused a little shaking of the Telescope, tho' strongly Supported. I could not easily apply the Micrometer, and the single Observation made with it was found to be so inaccurate, by comparing it with a contemporary Observation made in a close Room at *Greenwich*, that I shall not mention it. Mr *Jer. Sisson* counted the Clock, whilst I observed the Sun. At 8 in the Morning nothing appeared in the Sun, and it was soon after obscured by many Clouds. At about 10 ½ I first discovered *Mercury*, having then finished almost half his Passage. The Sun was then covered with Clouds again, but at Noon grew bright, when the ascensional Difference of the Sun and *Mercury* appeared: for having placed three vertical Threads in the Focus of the meridional Passage. *Oct*, 24. $23^h\ 57'\ 48''$ T. App. the preceding Limb was come to the first of them, and in $25''$ the Centre of *Mercury* came thither. It then grew very cloudy with Rain, so that I thought of giving over the Observation, but the Clouds breaking again, I proceeded, and had the Pleasure to see *Mercury* exceeding black upon the bright Body of the Sun, and set down the following Phases exactly.

—*by* John Bevis, *M. D*, No. 471. p. 624. *Read* Dec. 15. 1743.

* *Vide Supra*, §. XXVI.

Temp. Ap.	h	′	″	
Oct. 25.	0	58	34	The Distance between the Limbs of the Sun and *Mercury* nearly equal to the Diameter of *Mercury*.
	1	0	33	The last interior Contact.
		1	25	The Egress of the Centre, judged by the Eye.
		2	16	The last exterior Contact.

The Day before, a little before Noon the Diameter of the Sun was measured 32′ 27″ with an excellent Telescope of 12 Feet, armed with a Micrometer.

Mr *Bird* made a good Observation, about the Beginning of the Transit, with a catadioptrical Telescope that magnified much, in *Surrey-street* about 1″ ½ of Time East from the Place of my Observation. He perceived a very small Thread of Light between the Limbs of the Sun and *Mercury*, which had just entered, scarce equal, as he said to $\frac{1}{10}$ of the Diameter of *Mercury*, at 8h 30′ 56″, that is, at 8h 30′ 54″ ½ in *Beaufort-Buildings*, as appeared by an exact comparison of the Clocks; wherefore I may venture to refer the total Ingress at *Beaufort-Buildings* to 8h 30′ 40″ as near as possible.

I may therefore, from what has been said, set down the whole Transit, as seen at *Beaufort-Buildings*, in the following manner.

Temp. Ap.	h	′	″	
Oct. 24.	20	28	57	The first exterior Contact.
		29	48 ½	The Ingress of the Centre.
		30	40	The first interior Contact.
25.	1	0	33	The last interior Contact.
		1	24 ½	The Egress of the Centre.
		2	16	The last exterior Contact.

—*by Mr* John Catlyn, No. 466. p. 235. *Read* Nov. 25. 1742.

3.

		h	′	″
The Equal Time of the true ☌ at *Greenwich* — *Oct.*	24.	22	15	58
The Equation of Natural Days add — —			16	11
Apparent Time of the true ☌ — — — *Oct.*	24.	22	32	9

At which Time the true Place of the Sun and of *Mercury* seen from the Earth — — —	m,	12	36	44
The Geocentric Latitude of *Mercury* — — —	South.		9	37
The Elongation in 5 Hours (*i. e.*) the 2 ½ immediately preceding and following the ☌			29	16
The Difference of Latitude in the same time — —			4	24
Therefore the Angle of the apparent Way of ☿ with the Ecliptic — — — — —		8	33	0
And the Distance of their Centres at the Time of their nearest Approach — — — —			9	31
And the Motion of Interval between that and the ☌			1	26

And

	h	′	″
And the hourly Motion of *Mercury* in his Path over the Disk of the Sun — — — — —		5	55 $\frac{1}{12}$
And the Motion of the ½ Duration from the first to the last exterior Contacts of the Limbs —		13	15
And the Motion of the same for the interior Contacts — — — — — — —		13	4
Hence the Time of the Interval from the 6 to the Middle — — — — — — — —		14	32
of ½ the exterior *Transit* — — — — —	2	14	22
of ½ the interior *Transit* — — — — —	2	12	30

Hence

	h	′	″	
The first exterior Contact of the Limbs —	8	32	19	*Oct.* 25. Morning.
The first interior Contact — — —	8	34	11	
The nearest Approach of the Centres, or Middle — — — — — —	10	46	41	
The last interior Contact — — — —	0	59	11	Afternoon.
The last exterior Contact, or End of the *Transit* — — — — — —	1	1	3	

This Computation is made from Tables * which give the ascending Node of *Mercury* at the Time of this *Transit* 6′ 17″ too forward, according to the Result of very accurate Observations made of that in the Year 1723, by Dr *Halley*, Dr *Bradley*, and Mr *Graham*. Therefore making the Calculation with this Correction of the Place of the Node, the Times of the several Circumstances of the *Transit* will be as follows:

	h	′	″	
The first exterior Contact — — — —	8	29	21	*Oct.* 25. in the Morning.
The first interior Contact — — — —	8	31	5	
The nearest Approach of the Centres — —	10	46	6	
The last interior Contact — — — —	1	1	7	Afternoon
The last exterior Contact — — — —	1	2	51	

This *Transit* may be very aptly compared with that which happened on the 24th Day of *October* 1697 †; as happening at the End of a remarkable Period in *Mercury*'s Motion, by which he is nearly in the same Situation, with respect to the Sun, at every Completion of it. Dr *Halley* in his Series of Moments, in which *Mercury* is joined to the Sun, *&c* ‖. makes the Middle of this *Transit* at 11′ past Six in the Morning the 24th Day, or the 23d Day at 18^h 11′ *p. m.* and the Distance of the Centres of the Sun and *Mercury* 10′ 4″.

It may not be amiss to examine and compare these Numbers by such Observations as were made of this *Transit*, and may be depended on, and thereby to collect the Difference between Computation and Observation; and whatever Error arises in Excess or Defect by a proper

* Vol. VI. Chap. III. §. 39. † Mean Period 46 Years 1^d 5^h 43′ 42″.
‖ Vol. I. Chap. IV. §. 100.

2 Application

Application to the *Transit* of 1743, it is imagined, will foretel it with a greater Degree of Exactness, than a Calculus from any Theory whatsoever.

There was only the Egress of *Mercury* in the *Transit* of 1697, capable of being observed in *Europe* *; which was done at *Nuremberg* in *Germany*, by Mr *Wurtzelbaur*, and at *Paris* by M. *Cassini*; at *Greenwich* Clouds prevented it. At *Nuremberg* Mr *Wurtzelbaur* observed *Mercury* to go off of the Disk of the Sun † at $8^h\ 45'\ \frac{1}{2}$ *mane* about $73\ \frac{1}{2}$ Degrees from the Vertex of the Sun to the Right-Hand; and M. *Cassini* observed the same accurately at $8^h\ 10'\ 24''$ *mane*; therefore from the known Difference of Meridians of these Places, the Egress must have happened at *Greenwich* at $8^h\ 1'$ *mane*.

The Observation of Mr *Wurtzelbaur* will greatly avail at coming at the Duration of the *Transit*. It is mentioned, that *Mercury* left the Limb of the Sun $73^\circ\ 30'$ from his Vertex to the Right. Now at that time at *Nuremburg*, the Angle of the Ecliptic with the Vertical passing through the Sun's Centre, was $42^\circ\ 3'\ 5''$; therefore the last Point of Contact on the Sun's Limb was observed $31^\circ\ 26'\ 55''$ from the Ecliptic to the South, and consequently his Latitude was $8'\ 28''$ South at that time.

To find the Point on the Sun's Limb of the Ingress, in order to come at the Duration of the *Transit*, we must be beholden to Computation, and the Theory of *Mercury*'s Motion: I have therefore, from the Tables from which the above Times of the *Transit* of 1743 are drawn, carefully computed his Motion along his Path crossing the Disk of the Sun, and find that he moved along it after the Rate of $5'\ 53''\ \frac{1}{4}$ in an Hour, and the Difference of Latitude in 5 Hours $4'\ 21''$, and his Elongation $29'\ 7''$: Therefore the Angle of his visible Way was $8^\circ\ 29'\ 50''$, which, doubled, and added to $31^\circ\ 26'\ 55''$, gives $48^\circ\ 26'\ 35''$, his Distance, on the Limb of the Sun from the Ecliptic also to the Southward at his Ingress on it; therefore the nearest Approach of his Centre to that of the Sun was $10'\ 19''$, and the Length of the Path run during the *Transit* $25'\ 14''$, and consequently the time of running it $4^h\ 17'$ the half of which $2^h\ 8'\ \frac{1}{2}$, subtracted from $20^h\ 1'$, the End of the *Transit* at *Greenwich*, gives the Middle there at $17^h\ 52'\ 30''$ earlier by $18'\ \frac{1}{2}$ than the Series of Moments, *&c.* give it.

Now as the said Series makes the Middle of the *Transit* of 1743, at $11^h\ 2'$ *mane*, and as it corresponds with that of 1697; and the Computation of that is $18'\ \frac{1}{2}$ too late by the Series of Moments, *&c.* it may be reasonably expected, that the same Computation for this of 1743 will be so much too late too; and if so, the Middle may be put down at $43'\ \frac{1}{2}$ past 10, or $44'$ at farthest, *October* 25th in the Forenoon.

* *Flamstead's Hist. Cœlest.* Lib. II. Fol. 32.

† Vertex to the Right, it says, *a Nadir Solis ad dextras*; but it is a manifest Mistake, as any one upon Trial may find.

By

By Computation from the Tables above-mentioned, with the Correction of the Node, I make the Distance of the Centres at the nearest Approach in 1697, to be 10′ 33″, but by the Observations of Mr *Wurtzelbaur* it turns out only 10′ 19″, less by 14″. Should therefore their Distance in 1743 computed in the same manner at 9′ 10″ be as much diminished, the Duration of the *Transit* will be protracted no less than 5′ 24″, and the first Contact will be 2′ 42″ earlier, and the last so much later, than the Times abovementioned for them.

N. B. In the Computation of the *Transit* of 1743, the Semidiameter of the Sun is supposed 16′ 14″ ½ and that of *Mercury* 4″ ½; but in that of 1697, have taken *Mercury*'s only 3″ ½, imagining the precise Moments, of the first and last exterior Contacts are not observable; but that the Ingress is seen some little Time later, and the Egress sooner, than the true Times thereof. I have all along spoke of the Motion of *Mercury* without mentioning that of the Sun, whereas, in Reality, it is that of them both jointly; but as we may suppose the Sun to stand still during the *Transit* it will then be considered as the apparent Motion of *Mercury* alone for that Time.

XXX.

An Occultation of Mercury *by* Venus, May 17 1737. *at the Observatory at* Greenwich, *by* J. Bevis *M.D.* No. 450. p. 394. Oct. &c. 1738. Fig. 83.

Apparent Time.

h	′	″	
1	37	3	The preceding Limb of *Venus* passes the Meridian, the Centre from the *Vertex* 25° 46′ 35″; but I could not see *Mercury* within the Telescope.
9	4	9	The Centre of *Mercury* preceded the preceding Limb of *Venus* 12″ of Time.
	6	20	The same preceded, as before, the same Quantity of Time.
	28	0	As *Mercury* ran along the parallel Thread of the Micrometer, the southern Cusp of *Mercury* was cut by the same Thread ☽ ☿, whence I gathered that *Venus* would cover *Mercury*, or at least touch him; therefore I drew out the Micrometer, that I might discern the inner Contact the better, with a Tube of 24 Feet.
	43	4	*Mercury* is not distant from *Venus* more than $\frac{1}{10}$ or $\frac{1}{12}$ of the Diameter of *Venus*: then interposing Clouds.
	51	10	*Venus* shines out again very bright, but all *Mercury* lies hid under *Venus*: The Clouds now cover *Venus* again, hindering any farther Contemplation of so rare a Spectacle.

May 18. *p. m.* Meridian Distance of the Sun from the *Vertex* 30° 4′.

h	′	″	
1	31	53	The preceding Limb of *Venus* passes the Meridian. The Centre distant from the *Vertex* 25° 57′ 15″.

I could not ſee *Mercury* culminating this Day, tho' the Sky was very clear.

N. B. The Diſtances from the *Vertex* are not cleared from the Refractions.

An Obſervation on the Planet Venus, *(with regard to her having a Satellite) made by Mr* James Short, *F.R.S. at Sunriſe,* Oct. 23. 1740. No. 459. p. 646. Jan. &c. 1741.

XXXI. Directing a Reflecting Teleſcope of 16.5 Inches Focus, (with an Apparatus to follow the diurnal Motion) towards *Venus*, I perceived a ſmall Star pretty nigh her; upon which I took another Teleſcope of the ſame focal Diſtance, which magnified about 50 or 60 times, and which was fitted with a Micrometer, in order to meaſure it's Diſtance from *Venus*; and found it's Diſtance to be about 10°. Finding *Venus* very diſtinct, and conſequently the Air very clear, I put on a magnifying Power of 240 times, and, to my great Surprize, found this Star put on the ſame Phaſis with *Venus*. I tried another magnifying Power of 140 times, and even then found the Star under the ſame Phaſis. It's Diameter ſeemed about ⅓, or ſomewhat leſs, of the Diameter of *Venus*; it's Light was not ſo bright or vivid, but exceeding ſharp and well defined. A Line, paſſing through the Centre of *Venus* and it, made an Angle with the Equator of about 18 or 20 Degrees.

I ſaw it for the Space of an Hour ſeveral times that Morning; but the Light of the Sun increaſing, I loſt it altogether about a Quarter of an Hour after Eight. I have looked for it every clear Morning ſince, but never had the good Fortune to ſee it again.

Caſſini, in his Aſtronomy, mentions much ſuch another Obſervation.

I likewiſe obſerved Two darkiſh Spots upon the Body of *Venus*; for the Air was exceeding clear and ſerene.

Several Aſtronomical Obſervations made at Pekin, *by the Jeſuits.* No. 468. p. 306 Jan. 1742-3.

XXXII.

1740.		h	′	″	
Nov.	8.	18	34	0 *p. m.*	♀ preceding the Star *η* in ♍ was more Weſt in the right Aſcenſion 2′ 54″ of Time, and more North in the Declination 6′ 30″.
	9.	18	34	0 *p. m.*	♀ following yeſterday's Star, was more Eaſt in the right Aſcenſion 1′ of Time, and more South in the Declination 15′ 40″ Diſtance 21′ 53″.
	22.	18	43	0 *p. m.*	♀ followed the Star *ϑ* in ♍ in right Aſcenſion 4′ 27″ of Time, and was more ſouthern in the Declination 13′ 20″.
	26.	7	45	0 *Veſp.*	The Star *τ* in ♒ ſtood in the Line of Dichotomy of ☽, from the ſouthern Cuſp in the Declination more ſouthern 13′ 0″.

Dec.

		h	′	″		
Dec.	2.	17	20	0	*p.m.*	The Star ε in ♉ was above ☽ in the same right Ascension with the Centre of Plato, more North in the Declination 12′ 20″, Plato was distant from the northern Limb of ☽ 4′ 0″.
	4.	12	26	0	*p.m.*	☽ covered the Star η in ♊, which immerged against *Byrgius*; the Emersion was not observed because of a Cloud.
1741 *Jan.*	1.	7			*mane*	♀ distant from the Star ν in ♏ ♐ 34′. It followed it in right Ascension 1′ 50″ of Time: more North in the Declination 19′.
		5	59	30	*p.m.*	The western Limb of ☽ at the horary Thread in the Telescope.
		6	0	24	——	♃ at the Day-Thread, was distant from the northern Limb of ☽ 13′.
			1	15	——	The eastern Limb of ☽ at the same Thread.
		6	5	36	——	The western Limb of ☽ again at the same horary Thread.
			6	18	——	♃ at the same Thread, distant from the North Limb 12′ 30″.
			7	55	——	Eastern Limb of ☽ at the same Thread.
		11	34	0	——	♃ culminated, Altitude 73° 26′.
			43	0	——	☽ culminated, Altitude of the Centre 73° 15′.
	21.	5	30	0	*p.m.*	♂ preceded the Star of yesterday ε in ♊ 1′ 8″ of Time in right Ascension, it was more southern in declination 5′.
	22.	5	15	0	*p.m.*	♂ preceding the Star of yesterday 2′ 45″ of Time in right Ascension, more southern in Declination 2′.
	28	7	18		*Vesp.*	♃ was distant from the Edge of ☽ 19′ 50″.
Feb.	22.	11	44	26	*p.m.*	The Moon covered the Star η in ♉ standing in a right Line with *Manilius* and *Censorinus*. The Emersion could not be seen.
	24.	9	27	0	*p.m.*	The Star η in ♊ below the ☽ stood in a Right Line with *Tycho* and *Plato*, being distant from this to the South 11′ 20″.
		13	38	45	*p.m.*	The Star μ in ♊ was covered by ☽ in a right Line thro' Tycho and Posidonius; which did not emerge before $13^h 55'$ when ☽ set behind a House.

	h	′	″		
Apr. 20.	10	50	42	*p. m.*	☽ covered the third Satellite, which was to the West of ♃.
	10	56	45		☽ touched the Limb of ♃ $10^h\ 57'\ 35''$ was the full immersion of him in the middle between each Cusp of ☽ directly toward the Centre. The other Satellites were not very discernable, because of the Atmosphere, and the Moon hid itself soon after behind the Houses.
Sept. 24.	8	7	15	*p. m.*	☽ covered the preceding Star of the Quadrangle before the southern Tail of the Whale, which just emerged at the rising of *Cleostratus.*
	9	0	13		The same emerged very near *Berosus.*

Observations upon the Comet that appeared in Jan. Feb. *and* March 1737, *made at* Oxford, *by* J. Bradley, *F. R. S. Sav. Prof. Astron.* No. 446. p. 111. July &c. 1737.

XXXIII. 1. I made several Observations on the late Comet, during the last 5 Weeks of it's Appearance, which enabled me to find out the Elements of a Parabolic Trajectory, upon which a *Calculus* might be founded, that would correspond with each of my Observations within about 1′ of a Degree: But the first of them being taken many Days after the Time of the *Perihelion*, and the whole Series comprehending but a very small Portion of the Trajectory; I was sensible, that a little Error, either in the Observations themselves, or in the Places of the Fixt Stars with which the Comet was compared, might occasion a considerable Difference in the Situation and Magnitude, &c. of the Orbit deduced from them alone; and therefore I was desirous of having some earlier and accurate Observations, in order to determine those Elements with more Certainty: But I have not yet been able to procure them.

I first saw the Comet *Feb.* 15th 1737, between 6 and 7 in the Evening, when it's *Nucleus* appeared small and indistinct, and it's Tail (extending above a Degree from the Body) pointed towards the Star *in Lino Austral. Piscium*, marked ξ by *Bayer.* Applying my Micrometer to a good 7 Foot Tube, I observed, that at $7^h\ 32'$ *Temp. Æquat.* the Comet preceded the said Star $1°\ 1'\ 40''$ in Right Ascension, and was $20'\ 20''$ more Southerly than the Star. *Note*, That the equal Time is likewise made use of in all the following Observations.

Assuming the Place of this Star, as it is settled in *the British Catalogue*, (as I shall likewise the Places of others hereafter mentioned) it follows, that the Comet's Right Ascension was $23°\ 58'$, and it's Declination $1°\ 31'\ 55''$, North.

Feb. 17. $7^h\ 33'$ the Comet followed α *in Nodo Lin. Piscium* $31'\ 25''$ in Right Ascension, and was $52'\ 30''$ more Northerly. Hence the Comet's Right Ascension was $27°\ 38'\ 20''$ and it's Declination $2°\ 21'\ 10''$ North.

Feb.

Feb. 18. 7^h 14′ a ſmall Star (whoſe Right Aſcenſion was afterwards found to be 29° 0′ 5″ and Declination 2° 58′ 30″ North) preceded the Comet 24′ in Right Aſcenſion, and was 15′ 30″ more Northerly. Hence the Comet's Right Aſcenſion was 29° 24′ 5″, and it's Declination 2° 34′ North.

Feb. 21. 7^h 25′ the Comet preceded *ν Ceti* 1° 6′ in Right Aſcenſion, and was 38′ 20″ more Southerly. Hence it's Right Aſcenſion was 34° 25′ 10″, and it's Declination 3° 47′ 20″ North.

Feb. 22. 7^h 45′ the Comet followed *ν Ceti* 30′ 5″ in Right Aſcenſion, and was 18′ 45″ more Southerly. Hence the Comet's Right Aſcenſion was 36° 1′ 15″, and it's Declination 4° 6′ 55″ North.

Feb. 25. 7^h 45′ a ſmall Star (whoſe Right Aſcenſion was afterwards found to be 40° 34′, and Declination 5° 5′ 30″ North) followed the Comet 2′ 30″ in Right Aſcenſion, and was 2′ 30″ more Northerly than the Comet. Hence the Comet's Right Aſcenſion was 40° 31′ 30″, and it's Declination 5° 3′ North.

The Difference of Right Aſcenſion and Declination between this Star and the Comet was taken with a 15 Foot Teleſcope; but the Place of the Star was determined by one Obſervation made with the 7 Foot Tube.

Feb. 27 8^h 45′ the Comet preceded a ſmall Star 1° 16′ in Right Aſcenſion, and was 2′ 15″ more Southerly. The Right Aſcenſion of this Star was afterwards (by a ſingle Obſervation) found to be 44° 37′ 40″, and it's Declination 5° 38′ 30″ North. Hence the Comet's Right Aſcenſion was 43° 21′ 40″, and it's Declination 5° 36′ 15″ North.

March 4. 8^h a ſmall Star (whoſe Right Aſcenſion was found to be 49° 30′ 30″, and it's Declination 6° 38′ 30″ North) preceded the Comet 7′ 30″ in Right Aſcenſion, and was 10′ more Southerly. Hence the Right Aſcenſion of the Comet was 49° 38′, and it's Declination 6° 48′ 30″.

March 12. 8^h 25′ the Comet preceded *μ Tauri* 2° 5′ 50″ in Right Aſcenſion, and was 4′ 25″ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 58° 12′ 40″, and it's Declination 8° 16′ 50″ North.

March 14. 9^h the Comet followed the *47th Star of* Taurus *in the* Britiſh *Catalogue* 12′ 50″ in Right Aſcenſion, and was 15″ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 60° 8′ 5″, and it's Declination 8° 34′ 5″ North. This, and all the following Obſervations, were made with a good 15 Foot Teleſcope, the Comet now appearing too faint to be well obſerved with the 7 Foot Tube.

March 17. 8^h 40′ the Comet followed *ν Tauri* 25′ 5″ in Right Aſcenſion, and was 9′ 40″ more Northerly. Hence it's Right Aſcenſion, was 62° 47′ 55″, and it's Declination 8° 58′ 45″ North.

March 19. 7^h 50′ the Comet followed the ſame Star 2° 4′ 50″ in Right Aſcenſion, being 23′ 55″ more Northerly. Hence it's Right Aſcenſion was 64° 27′ 40″, and Declination 9° 13′ North.

 The

The ſame Night, at 9ʰ the Comet preceded *d Tauri* 47′ 40″ in Right Aſcenſion, and was 22′ 50″ more Southerly. Hence it's Right Aſcenſion was 64° 30′ 20″, and Declination 9° 12′ 35″ North.

March 20. 8ʰ 5′ the Comet preceeded *d Tauri* 30″ in Right Aſcenſion was 16′ 35″ more Southerly than the Star. Hence it's Right Aſcenſion was 65° 17′ 30″, and Declination 9° 18′ 50″ North.

March 22. 8ʰ 15′ the Comet followed the ſame Star 1° 36′ 10″ in Right Aſcenſion, and was 3′ 50″ more Southerly. Hence it's Right Aſcenſion was 66° 54′ 10″, and Declination 9° 31′ 35″ North.

This was the laſt Night that I ſaw the Comet; for the Moon being then in her Increaſe, entirely obſtructed it's further Appearance. The Light of the Comet was indeed (even in the Moon's Abſence) ſo very weak, that I found it difficult, in ſome of the latter Obſervations, to take it's Place with any tolerable Certainty; which is, in part, the Cauſe of ſome little Diſagreement obſervable in the Comet's Places taken from the ſame Stars on different Nights; though there are likewiſe other Irregularities that occur in this Series of Obſervations, which ſeem to ariſe from ſmall Errors in the aſſumed Places of the Fixt Stars.

Suppoſing the Trajectory deſcribed by this Comet to be nearly *Parabolical*, conformable to what Sir *I. Newton* has delivered*. I collect from the foregoing Obſervations, that the Motion of this Comet in it's own Orbit was *direct*, and that it was in it's *Perihelion*, *Jan.* 19. 8ʰ 20′ *Temp. Æquat. Lond.* That the Inclination of the Plane of the Trajectory to the Ecliptick was 18° 20′ 45″. The Place of the Deſcending Node ♉ 16° 22′. The Place of the *Perihelion* ♒ 25° 55′. The Diſtance of the *Perihelion* from the Deſcending Node 80° 27′. The Logarithm of the *Perihelion Diſtance* from the Sun 9.347960. The Logarithm of the Diurnal Motion 0.938188.

From theſe Elements (by the Help of Dr *Halley*'s general Table for Comets, to which they are adapted) I computed the Places in the following Table; which alſo contains the Longitudes and Latitudes of the Comet, calculated from the obſerved Right Aſcenſions and Declinations above-mentioned, together with the Differences between the obſerved and computed Places.

* Princip. Math. Lib. iii.

Oxon.

Oxon. 1737. *Temp. Æquat.*	*Com. Longit. Observat.*	*Lat. Aust. Observat.*	*Com. Longit. Computat.*	*Lat. Aust. Comput.*	*Diff. Long.*	*Diff. Lat.*
Day h '	s ° ' ''	° ' ''	s ° ' ''	° ' ''	''	''
Febr. 15 7 32	♈ 22 45 7	7 53 27	♈ 22 45 00	7 53 1	+ 7	+ 26
17 7 33	26 30 30	8 27 21	26 30 44	8 28 6	− 14	− 45
18 7 14	28 18 14	8 44 20	28 17 46	8 43 57	+ 28	+ 23
21 7 25	♉ 3 26 34	9 26 50	♉ 3 26 53	9 26 46	− 19	+ 4
22 7 45	5 4 53	9 40 00	5 5 28	9 39 27	− 35	+ 33
25 7 45	9 42 18	10 12 21	9 41 19	10 12 22	+ 59	− 1
27 8 45	12 36 43	10 31 42	12 36 16	10 31 13	+ 27	+ 29
Mar. 4 8 00	19 3 00	11 6 46	19 3 5	11 7 8	− 5	− 22
12 8 25	27 49 58	11 43 2	27 49 53	11 43 19	+ 5	− 16
14 9 00	29 47 42	11 49 59	29 47 19	11 49 26	+ 23	+ 33
17 8 40	♊ 2 30 57	11 56 31	♊ 2 30 50	11 56 49	+ 7	− 18
19 7 50	4 12 36	12 00 19	4 12 45	12 00 47	− 9	− 28
9 00	4 15 11	12 1 12	4 15 13	12 00 52	− 2	+ 20
20 8 5	5 3 10	12 3 5	5 3 32	12 2 33	− 22	+ 32
22 8 15	6 41 30	12 6 15	6 41 19	12 5 42	+ 11	+ 33

From the ſmall Differences between the Comet's obſerved and computed Places, exhibited in the two laſt Columns of this Table, we may reaſonably conclude, that the Orbit, as above determined, cannot differ much from the Truth, and muſt therefore be near enough to enable future Aſtronomers to diſtinguiſh this Comet upon another Return, and thereby to ſettle it's Period; which I cannot at preſent pretend to do, not having met with an Account of any former Comet that ſeems likely to have been the ſame with this, whereof a Deſcription has been given particular enough to determine this Point.

On Mount Aventine, *at* Rome, *by the Abbot* Didacus de Revillas, *F. R. S.* Ibid. p. 118.

2. *Feb.* 16. about 7^h *p. m.* the Comet firſt appeared to us in the weſtern Part of the Heavens, 8° or 9° lower than *Venus*; and declining a little from her vertical Circle toward the South. With the naked Eye we ſaw only a whitiſh Line, ſhining with a doubtful Light. But with an excellent Teleſcope of 6 Feet, beſides the Tail, which was extended into the Part turned from the Sun, and appeared like a Line without the Teleſcope, we ſaw the *Nucleus*, though covered on all Sides with a thin Atmoſphere. As no Quadrant was at Hand, and not only a Cloud intercepted the View of the neareſt fixed Star, but the Twilight alſo concealed them, the apparent Place of the Comet could not be determined that Night.

From the 16th to the 19th, and alſo after the 25th there were many other Obſtructions, which hindred us from obſerving. But in in the Nights between the 19th and 26th, we could not accurately determine the apparent Place of the Comet, any otherwise than by comparing it's *Phænomenon* with *Venus*, because we used only a small Quadrant, of which the Optical Tube was scarce equal to an English Foot. Therefore from the vertical Altitudes both of the Comet and of *Venus*, observed at the same Time, we collected the vertical Differences of both, as follows.

Day	*p. m.*		*Vert. diff.*	
	h	′	°	′
20	7	59	5	22
22	7	00	3	56
23	7	20	3	13
24	6	15	2	30
25	7	30	1	47

The Tail of the Comet on the 22d, passing over the vertical Thread of the Micrometer, impended 1′ 7″. The Micrometer was fitted to the abovementioned Telescope.

24 8 03 Venus and the Comet appeared under an Angle of 7° 35′
25 7 50 They appeared under an Angle of — — — 8 05

—*at* Philadelphia *in* Pensylvania, *by* *Dr* Kearsly, *Ibid.* p. 119.

3. *January* 27, about Six in the Evening, I saw a dull Star about 3 or 4 Degrees above *Mercury*, and a little to the Southward of a Vertical passing through ☿, but took little Notice of it then, not thinking of a Comet; but by comparing ☿'s Place with the Fixt Stars, I afterwards thought it might be a Comet.——On the 31st, about 6ʰ 30′ *p. m.* I took it's Distance from *Venus*, by a Reflecting Instrument of Mr *Hadley*'s Make, 14° 40′, but by a Forestaff, 14° 50′, and a Right Line passed over the Comet, *Venus*, and the *Pleiades.* The Night following, about 6ʰ 20′ it's Distance from *Venus* was, by Mr *Hadley*'s Instrument, 13° 25′. The rest of my Observations, by such Instruments as I had, being none of the best, and the Comet's growing very dull, are as follow:

	h ′	*p. m.*
Feb. 7. — — —	6 47	Comet from *Venus* 7° 40′
	7 3	———from *Aldebaran* 59° 40′.
		———from *Algeneb* 17° 45′ by a Forestaff.
		A Right Line from the Comet over *Venus* passed over the bright Star in the Side of *Perseus*.
11. — — —	7 14	Comet from *Venus* 7° 12′.
	7 20	A Right Line over the Comet, *Venus*, and Head of *Cassiopeia*.
17. — — —	7 20	The Comet was in a Right Line, and to the Northward of two Stars; Distance of the Stars I

supposed

	h	′	
			ſuppoſed to be about 40′, and the Comet from the leaſt 30′. Theſe Stars, I think, were the South Node of *Piſces*, brighteſt from *Venus* 10° 20′ from *Aldebaran* 50° 30′ as I found it ſet down, but muſt be very falſe.
Febr. 20.	—	—	No Star within Sight of the Comet by the Teleſcope.
	30	7	Comet from *Aldebaran* 34° from *Lucida Cap.* ♈ 19½.
21. about	——	30 8	Wanted about a Degree of *Oculus Ceti.*—— Which was the laſt Sight I had of it.

4. The Comet was firſt perceived about *Jan.* 26, but muſt, by it's Plainneſs then, have been viſible for ſome Time before. It was in the Weſt firſt of all, ſome Degrees below and directly under *Venus* : Every Night it appeared nearer to that Star, but inclined Northerly. In about a Fortnight, it was parallel to it, and in a Week after, was no more to be ſeen.

—*at* Spaniſh Town *in* Jamaica, *by* Roſe Fuller, *M. D. F. R. S. Ibid.* p. 122.

5. *Feb.* 9. for 7 Days laſt paſt, about 7ʰ *Veſp.* there hath appeared a dim Comet, as we took it to be : It is ſeen in the Weſt, under *Venus* towards the S. W. It looks through a Tube of 10 or 11 Feet long, like a dim or pale Planet ; it's Tail tends upwards.

—*at* Madras, *by Mr* Sartorius, *a Miſſionary, ibid.*

6. *Jan.* 29. 1736-7, at 6ʰ 49′, *p. m.* we ſaw a Comet with a long bruſh Tail, at which Time it's Altitude was found 5° 15′, it's Diſtance from *Venus* 18° 5′ ; and *Venus*'s Altitude was obſerved 20° 40′. It bore due Weſt.

—*at* Liſbon, *by* G. R. Vanbrugh, *Eſq; on board the* Burford *Man of War.*

XXXIV. The Motion in it's own proper Orbit was retrograde.

The parabolic Orbit *for the* Comet *of* 1739, *obſerved by Seignor* Euſtachio Zanotti *at* Bologna. No. 461. p. 809. Aug. &c. 1741.

		°	′	
The *Perihelion* was in — — — —	♋	5	11	
The deſcending Node in — — —	♈	25	18	
The *Perihelion* from the Node — — —		69	53	

	d	h	′
The Comet was in the *Perihelion* — — *June*	9	9	59
in the deſc. Node *July*	18	4	57

The *Perihelion* of the Comet's Orbit was within the Sphere of the Orbit of *Veuns*, and without that of the Orbit of *Mercury* ; being diſtant from the Sun 0,69614 Parts of the Earth's mean Diſtance from the Sun.

The Plane of the Orbit ſtood inclined to the Plane of the Ecliptic in an Angle of 53° 25′.

The Diurnal mean Motion, according as it is interpreted by Dr *Halley* in his Elements of Cometical Aſtronomy, was 1°,5707.

Observations on a Comet, by F. Frantz, *a Jesuit at* Austria, Feb. 1743. No. 470. p. 457. *Read* April 21, 1743.

XXXV.

Styl. Nov.

Feb. 11. *Vesp.* The Comet in a right Line with { ε & ζ of Ursa α & δ Major

12. — — — — { γ of Ursa Major & λ of Draco, δ & χ of Ursa Major.

13. — { in a right Line with χ of Ursa Major & λ of Draco, in a rectangular Triangle with ψ & ω of Ursa Major.

14. almost in a right Line with { α of Leo & ν of Ursa Major, β of Leo & β of Virgo.

15. { almost in a right Line with β of Leo & β of Virgo in a rect. Triangle with ν & ξ of Ursa Major.

1[illegible]. — — almost in a right Line with β of Leo & β of Virgo; at which Time the Comet & β of Virgo were almost equally distant from β of Leo.

21. — In the Tail of Leo near a little Star of the 6th Magnitude, which constitutes { δ & β of Leo, β of almost a right Line with { Leo, & π of Virgo.

On the other Days it could not be observed with any Telescope, therefore the Comet was last seen at Vienna, near the abovementioned Star, of which the Long. $13^{\circ}\ 16'\ 28''$ ♍ and N. Lat. $17^{\circ}\ 30'$ the Comet declined from this Star *Feb.* 21. $8^{h}\ 8'\ 22''$ towards the N. $28'\ 16''$ more to the W. $1^{\circ}\ 15'\ 12''$.

Some Conjectures concerning the Position of the Colure *in the ancient* Sphere; *by the Rev.* Ebenezer Latham, *M. D.* No. 466. p. 221. Nov. 1742. Fig. 84.

XXXVI. I send you a Draught of the Constellation *Aries*, as it was exactly copied by Dr *White*, from a Book in the fine Library of *Samuel Saunders*, Esq; *F. R. S.* I do not know whether it may not be esteemed of some Moment towards the determining the famous Controversy with respect to Sir *I. Newton*'s Chronology. Dr *Halley* observes *, 'That the Dispute is chiefly, Over what Part of the 'Back of *Aries* the *Colure* passed. Sir *I. Newton* takes it to be over 'the Middle of the Constellation; *P. Souciet* will have it, that it 'passed over the Middle of the *Dodecatemorion* of *Aries*, which by 'Consequence would make it pass about Mid-way between the Rump 'and first of the Tail;" which Situation could never be said to be over the Back: Whereas, if the Ring in this Cut was designed, as I apprehend, to image the *Colure* in the *antient Sphere*, it exactly answers *Hipparchus*'s Description— ἐν δὲ τῷ ἑτέρῳ κολύρῳ φησὶ κεῖσθαι τὰ κριοῦ τὰ νῶτα κατὰ πλάτος. and justifies the Construction Sir *Isaac* put on those Words beyond Exception. The Sculptures from whence this was taken, have the Title of *Aratæa, sive Signa Cœlestia, in quibus Astronomicæ Speculationes Veterum ad Archetypa vetustissimi Aratæorum Cæsaris Germanici Codicis* (44) *ob oculos ponuntur a Jacobo de Geyn ex Biblioth. Acad. Lugd. Bat. Amstel.* 1652.†

* Vol. VII. Part IV. § I. 2.

† *Hug. Grotii Batavi Syntagma Arateorum: ex offic. Plantin.* 4to. See *Germanicus*'s Interpretation, p. 35, the Figure of the Constellation *Aries*.

I will

I will beg Leave to obferye farther, that as this Catalogue begins with the *Draco*, which the Ancients feem to place at the Head of their Conftellations; perhaps it may give fome Light into the Time of the Book of *Job*, as well as into the Senfe of that Place. For when he fays, *By his Spirit he has garnifhed the Heavens; his Hand has formed the crooked Serpent*; I fubmit it to the Judgment of the Critics, whether it is not highly probable the Writer muft have lived within that Period of Time wherein a Star of that Conftellation might pafs for the Polar Star: And then, if the Afterifms are fuppofed to be placed in fome fuch Order as here, the exprefs Mention that he only makes of this was fufficient to refer us to the whole Syftem or Furniture of the Heavens.

Chap xxvi. 13.

XXXVII. 1. As we now have the Globes of the Heavens, they are only formed for the prefent Age, and do not ferve the Purpofes of Chronology and Hiftory, as they might, if the Poles, whereon they turn, were contrived to move in a Circle round thofe of the Ecliptic, according to the prefent Obliquity of this. By this Means we might have a View of the Heavens fuited to every Period, and that would anfwer the ancient Defcriptions, thofe of *Eudoxus*, for Inftance, who is fuppofed to borrow his from the moft early Obfervations; and of *Hipparchus*, *&c.* Nor could any Contrivance better enable the loweft Reader to judge of the Merits of the Controverfy about the *Argonautic* Expedition, as far as it depends on this: For it will verify to the Sight the Path of the Colures, *&c.* at any Time.

A Propofal to make the Poles *of a* Globe *of the Heavens move in a Circle round the Poles of the* Ecliptic; *by the fame.* No. 447. p. 201. April 1738.

2. The Poles of the Diurnal Motion do not enter into the Globe, but are affixed at one End, to two Shoulders or Arms of Brafs, at the Diftance of 23° ½ from the Poles of the Ecliptic. Thefe Shoulders at the other End are ftrongly faftened on to an Iron Axis, which paffeth through the Poles of the Ecliptic, and is made to move round, but with a very ftiff Motion; fo that when it is adjufted to any Point of the Ecliptic, which you defire the Equator may interfect, the Diurnal Motion of the Globe on it's Axis will not be able to difturb it.

When it is to be adjufted for any Time, paft or to come, bring one of the brafen Shoulders under the Meridian, and holding it faft to the Meridian with one Hand, turn the Globe fo about with the other, that the Point of the Ecliptic, which you would have the Equator to interfect, may pafs under 0 Degrees of the brafen Meridian: Then holding a Pencil perpendicular to that Point, and turning the Globe about, it will defcribe the Equator as it was pofited at that Time; and transferring the Pencil to 23° ½, and 66° ½ on the brafen Meridian, the Tropics and Polar Circles will be defcribed for the fame Time.

A Contrivance to make the Poles *of the Diurnal Motion in a* Celeftial Globe *pafs round the Poles of the Ecliptic. Invented by* John Senex, *F. R. S.* Ibid. p. 203. May 1738.

By this Contrivance, the Celeftial Globe may be fo adjufted as to exhibit not only the Rifings and Settings of the Stars, in all Ages, and in all Latitudes, but the other *Phænomena* likewife, that depend upon the Motion of the Diurnal Axis round the Annual Axis.

Fig. 85. *a a a a.* A Section of the Celestial Globe.

E E. A strong Iron Axis, passing through the Poles of the Ecliptic.

b c. Two strong Arms of Brass, screwed on to the Ends of the Iron Axis, at *d.*

P P. The Axis or Poles of the Diurnal Motion, (by which the Globe is hung in the brasen Meridian) rivetted on to the other Ends of the brass Arms, and which may be carried round the Poles of the Ecliptic, by the Iron Axis, but with so stiff a Motion, as not to disturb the Diurnal Rotation on the Poles *P P.*

The true Delineation *of the* Asterisms *in the ancient Sphere By the Rev.*Ebenezar Latham,*M.D.* No. 460. p. 730. April &c. 1741.

XXXVIII. I never heard of Mr *Senex*'s Invention, till I saw the Transaction N° 447 *, and am pleased with the Opportunity I had of producing it to the World. It is many Years since I first thought of this Method, and have often suggested it to some Students. The Dispute that arose about Sir *Isaac Newton*'s Chronological Index, communicated by Abbé *Conti*, confirmed my Opinion of the Advantage that would attend it; especially the Admonition Dr *Halley* gave Father *Souciet*, (' to inform himself in the *Sphæriques*, so as to give ' us the right Ascension of the Stars truly from their given Latitude ' and Longitude') made me yet more sensible how necessary something of this kind was, to let common Readers into the Merits of the Controversy. But it was perfectly accidental, that I ever presumed to mention this Alteration in the Construction of the Globes, which I had so often wished might obtain for the Use of several Sciences. You will receive, with this, one Scheme, among several, which I have projected, that is nearest Mr *Senex*'s, and least defaces the Globe.

Fig. 86. Fig. 86. A Vertical Section of the Globe.

P. P. *The Poles of the diurnal Motion.*

A. *The Axle of the Globe, which terminates in the Poles of the Ecliptic, and receives the other End of the Brass Arms upon each of it's Pivots.*

Æ. *A brass Equator fixed to the brasen Meridian.*

K. K. *A Key, which, on Occasion, being put through a Hole in the Brasen Meridian, is just over the Place where the Poles of the Ecliptic pass, by means of a square Hole in the Head of a Screw, serves to fix that End of the Brass Arm, or give it Liberty to move with Ease: And the Key, being left in when the Screw is slackened, will hinder the Globe from moving on the Poles of it's diurnal Motion, till you have adjusted it to your Mind, straitened the Screw again, and taken out the Keys; as may be seen more plainly in,*

Fig. 87. Fig. 87. Which is nothing but the Windlass Part, or the Arm, Pole, and Part of the strong Axis of the Globe, with the Screw and Key more at large, and separate from one another for the more distinct View.

* *See* Sect. XXXVII. 2.

If

If I may take the Liberty to add any thing farther on this Head, next to the accurate Obſervation of the *Britiſh Catalogue* in placing the Stars themſelves, it ſhould be the Revival of the *ancient Figures* and *Colours*, as far as we can recover them. It is certain the Invention was very ancient, if we ſuppoſe the Deſcriptions *Eudoxus* has given us, taken from Obſervations long before his Time, when the Solſtitial Colure paſſed through the Middle of the *Great Bear*, and the *Crab*, through the Neck of *Hydra*, and cut *the Ship* between the Poop and the Maſt, *&c.*——Now I have mentioned *the Ship*, you will indulge a Conjecture, that the Situation of this [juſt on the Horizon (where they imagined the Sea) in an erect ſailing Poſture for ſome Eaſtern Expedition, and terminating their fartheſt View to the South,] may both give ſome Light into their *Latitude*, that impoſed this Name, and (from that, which muſt have been the Place of the Pole to anſwer this Form) *the Æra of Time*, wherein it was done; for, in the preſent Diſpoſition, the Inhabitants of *Greece* could not have a proper View of that Conſtellation, or be led to form it in the Manner the Ancients have done. I ſhall not here urge all the Difficulties in the old Deſcriptions, that might have a Solution from this Method; but if an Alteration could be made either in the *Colour* or *Attitude* of the *Figures* to anſwer them better, it would add to the Pleaſure of reading ſome Authors, and, together with that new Conſtruction, might afford us ſuch a View of the Heavens, as Mr *Addiſon* had of *Italy*, when he made the Tour of it with the Claſſics in his Hands: And, ſince I have brought thoſe Writings into the Account, you will allow me to cite ſome Paſſages, which might receive both *Truth* and *Beauty* from ſuch an Improvement: Where *Homer* ſays,

> Πληϊάδας θ' Ὑάδας τε, τό τε σθένος Ὠρίωνος,
> Ἄρκτον θ', ἣν καὶ ἄμαξαν ἐπίκλησιν καλέουσιν,
> Ἥτ' αὐτοῦ στρέφεται, καὶ τ' Ὠρίωνα δοκεύει,
> Οἴη δ' ἄμμορός ἐστι λοετρῶν ὠκεανοῖο.
>
> Ἰλιαδ. Σ. 487.

> The *Pleiads*, *Hyads*, with the Northern Team,
> And great *Orion*'s more refulgent Beam;
> To which around the Axle of the Sky,
> The *Bear* revolving points *his* golden Eye,
> Still ſhines exalted on th' ethereal Plain,
> Nor bathes *his* blazing Forehead in the Main.
>
> Mr POPE.

Mr *Pope*, amidſt a ſmall Miſtake of the *Sex*, keeps only the Forehead above Water; but the Poet ſeems to exempt her entirely; and ſo

 does

does *Virgil*, when he makes *Fear* account for the same *Phænomenon*, that *Ovid* (who preserves all the Fable of the Ancients) ascribes to *Force.*

Maximus hic Flexu sinuoso elabitur Anguis
Circum, perque duas in morem fluminis Arctos:
Arctos Oceani metuentes Æquore tingi.

VIRGIL. Georg. Lib. I. 244.

Around our Pole the spiry *Dragon* glides,
And like a winding Stream the *Bears* divides,
The Less, and Greater, who by Fate's Decree
Abhor to dive beneath the * *Southern* Sea.

DRYDEN.

Nuper honoratas summo mea Vulnera Cœlo
Videritis Stellas illic, ubi Circulus Axem
Ultimus extremum Spatioque brevissimus ambit.

OVID. Met. Lib. II. 515.

— — — — New Stars you'll see,
In this approaching Night's Obscurity,
With hateful Beams i'th' *Arctic* Circle shine.

He immediately adds,

At vos si læsæ contemptus tangit Alumnæ,
† *Gurgite cæruleo septem prohibete Triones:*
Sideraque in Cœlo stupri mercede recepta
Pellite, ne puro tingatur in Æquore Pellex.

———527.

Ne'er let those spurious Stars approach the Deep,
Nor in the purging Ocean's Bosom sleep,
But their eternal Stain, their whorish Tincture keep.

And when he describes them as a Team, it is with the same Reserve.

Tum primum Radiis gelidi caluere Triones,
Et vetito frustra tentârunt Æquore tingi.

———171.

Then the Sev'n Stars first felt *Apollo*'s Ray,
And wished to dip in the forbidden Sea.

* Northern. † In the *Ordeal* by Water it was adjured, not to receive the Guilty, in Terms like these.

 All

All which is a proper Hint for the *Disposition of the Globe*, that must correspond to these *Appearances* then, and which can only be obtained by this Method: By the Help of which we may also apprehend the Light these Descriptions give us *into the Age of the Writers.* I may illustrate this from *Hesiod*'s Account of *the Seasons*, of which we have not only a better Idea by this artificial Disposition of the Globe to answer them, but also of *the Time* wherein he lived, when we come to adjust the Heavens to the accurate Instructions he gives us, according to his Latitude at *Ascra*, allowing 50'' *per Annum* for the apparent Motion of the Stars.

> Εὖτ' ἂν δ' ἑξήκοντα μετὰ τροπὰς ἠελίοιο
> Χειμέρι' ἐκτελέσῃ Ζεὺς ἤματα, δή ῥα τότ' ἀστὴρ
> Ἀρκτοῦρος προλιπὼν ἱερὸν ῥόον ὠκεανοῖο,
> Πρῶτον παμφαίνων, ἐπιτέλλεται ἀκροκνέφαιος.
>
> Ἡσιόδ. Ἔργ. Βιβλ. β. 182

> When the glad Sun, approaching with his Rays,
> Has from the Tropic run out Sixty Days;
> *Arcturus*, rising from his sacred Bed,
> Is first discover'd in the Ev'ning Shade.

> Εὖτ ἂν δ' Ὠρίων καὶ Σείριος ἐς μέσον ἔλθῃ
> Οὐρανον, Ἀρκτοῦρον δ' ἰσίδῃ ῥοδοδάκτυλος Ἠώς.
>
> ——— 227.

> But when *Orion*, and the *Dog-Star*, come
> To the Mid-region of the heav'nly Dome,
> The Morn, that blushing draws away the Night,
> Beholds *Arcturus* in the dawning Light.

If we fix the Pole almost in the Mid-way between the Star in the Shoulder of the lesser *Bear*, and another of the *Serpent*, we shall have the Satisfaction to observe all these *Phænomena* answer the Description. I shall not enter into the Calculation; for I would not anticipate the Pleasure, one, that hath no Notion of the Age of *Hesiod*, must have, when he finds himself able, with so much Ease and Precision, to determine it by these Characters *.

Hesiod's

* Since I wrote this, I had the Pleasure to find *Scaliger* concur with me——
Hesiodus florebat eo Sæculo, quo Arcturus ἀκρόνυχος *oriebatur in Bæotia, viij Die Martii. Si quid hoc ad Conjecturam facit, saltem apud excellentes Astrologos, qui ex hoc Parapegmate infra septuaginta plus minus Annos Sæculum Hesiodi deprehendere possunt. Animadvers. in Chron. Eusebii, p.* 67. Edit. Lugd. Batav. 1606.———
The following Passage in Sir *Isaac Newton*'s Chron. *p.* 95. hath come to my Hands since the former. ' *Hesiod* tells us, that, 60 Days after the Winter Solstice, the Star

' *Arcturus*

Hesiod's Account of the *Pleiads* is too particular not to demand our Attention, and require an Explanation in the same way †.

> Πληϊάδων Ἀτλαγενέων ἐπιτελλομενάων
> Ἄρχεσθ' ἀμητοῦ, ἀρότοιο δὲ δυσσομενάων.
> Αἳ δή τοι νύκτας τε καὶ ἤματα τεσσαράκοντα
> Κεκρύφαται. Ἡσιόδ. Ἔργ. Βιβλ. β'. 1.

> Begin the Harvest, as the *Pleiads* rise.
> And take the Plough, when they withdraw the Skies;
> For Forty Days and Nights their glimm'ring Light,
> Obscur'd to us, no longer chears the Sight.

To this I might add *Homer*'s Image of the *Dog-Star*, but especially the exact Description in *Hesiod*.

> Ἀστέρ' ὀπωρινῷ ἐναλίγκιον, ὅς τε μάλιστα
> Λαμπρὸν παμφαίνῃσι λελουμένος ὠκεανοῖο. Ἰλιάδ. Ε'. 5.

> Like the red Star, that fries th'autumnal Skies,
> When fresh he rears his radiant Orb to Sight;
> And, bath'd in Ocean, shoots a keener Light.

> ——————δὴ γὰρ τότε Σείριος ἀστὴρ
> Βαιὸν ὑπὲρ κεφαλῆς κηριτρεφέων ἀνθρώπων
> Ἔρχεται ἠμάτιος, πλεῖον δέ τε νυκτὸς ἐπαυρεῖ.
> Ἡσιόδ. Ἔργ. Βιβλ. β'. 35.

> For then the *Dog-Star* governs in his Course,
> Walks o'er the Heads of Men, who feel his Force,
> Comes in the Day, but chiefly shares the Night.

How beautifully does the same Writer express the Gesture of *Orion*, as he is following the *Pleiads*?

' *Arcturus* rose just at Sun-set; and thence it follows, that *Hesiod* flourished about 100 ' Years after the Death of *Solomon*, or in the Generation or Age next after the *Trojan* War, ' as *Hesiod* himself declares.'

'Tis what we may compute by the present Globe; for, bringing *Arcturus* to the Eastern Horizon, the Sun we shall find in the Ninth Degree of *Aries*. Now it enters ♑ *Dec.* 11. and 60 Days after, or *Feb.* 10. it is in ♓ 2° $30'$ when allowing for the Northern Latitude of *Arcturus* to make it visible on the Horizon, it's Longitude must have been ♍ 14°, &c. whereas it's Place now is about ♎ 20° $27'$ $12''$. And the Difference both ways one Sign 6° $18'$ &c. which makes him to have lived 2614 Years ago.

† *Hisce Signis veteres Agricolæ, & ex eorum Traditionibus Scriptores rei rusticæ, nec non & Medici, Poëtæ, & Historici sunt usi ad Anni Tempestates designandas,* &c. Greg Astron. *p.* 130.

Εὖτ' ἂν Πληϊάδες σθένος ὄβριμον Ὠρίωνος
Φεύγουσαι, πίπτωσιν ἐς ἠεροειδέα πόντον. ———237.

The *Pleiads*, flying from the threat'ning Scourge
Of ſtrong *Orion*, plunge into the Surge.

Perhaps this may give ſome Light to a Paſſage of *Virgil*, that hath very much puzzled his Commentators.

Taygete ſimul Os terris oſtendit honeſtum
Pleias, *& Oceani ſpretos pede reppulit Amnes:*
Aut eadem Sidus fugiens, *ubi Piſcis aquoſi*
Triſtior hybernas Cœlo deſcendit in Undas.
Georg. Lib. iv. 232.

Firſt, when the pleaſing *Pleiades* appear,
And ſpringing upward ſpurn the briny Seas:
Again when their affrighted Choir ſurveys
The watry *Scorpion* mend his Pace behind,
With a black Train of Storms, and Winter-wind,
They plunge into the Deep, and ſafe Protection find.
DRYDEN.

Some, I know, by this *Sidus* underſtand the *Southern-Fiſh*, others the *Hydra*, and ſome the *Sun*; but how Mr *Dryden* came to inſert *Scorpio*, I ſhall not inquire. Nor ſhall I trouble you with any Conjectures with regard to the *ancient Figures*: It is certain there have been *Variations* in this reſpect, ſince *Ptolemy* mentions a Star in the Horn of *Aries*; and it is thought *Hipparchus* reckoned one, that is now in the Line, to the firſt Foot of *Aries* *. Whether the Epithet *Ovid* gives *Capella*, does not imply ſome little Difference, in the Situation of it, from ours, I leave to the Critics.

—*Et* Oleniæ *Sidus pluviale Capellæ,*
Taygetenque, Hyadaſque Oculis, Arctonque notavi.
Met. Lib. III. 594.

* Since I wrote this, I find Sir *Iſaac Newton*, in this way, recover to their former Places the Stars below, by rectifying the Delineation.
' In the extreme Fluxure of *Eridanus*, a Star of the Fourth Magnitude, of late ' referred to the Boſom of *Cetus*.
' In the Head of *Perſeus*, a Star of the Fourth Magnitude.
' In the Right Hand of *Perſeus*, a Star of the Fourth Magnitude.
' In the Neck of *Hydrus*, a Star of the Fourth Magnitude.
' In the Left Hand of *Cepheus*, one of the Fifth Magnitude.'
All whoſe Characters he deſigns from *Bayer*.

—I began

——I began to note
The ſtormy *Hyades*, the rainy * *Goat*,
The bright *Taygite*, and the ſhining *Bears*,
With all the Sailors Catalogue of Stars.

I might inſiſt on the Etymology of *Arcturus*, and others; for it appears from the Accounts the Ancients themſelves give us, there was not always the greateſt Uniformity in their Drawings. *Ovid* ſays of *Bootes.*

— — — — — *& te tua Plauſtra tenebant.*
Lib. 2. 177.

Nay, and 'tis ſaid, *Bootes*, too, that fain
Thou would'ſt have fled, though cumber'd with thy Wain.
ADDISON.

And he lets us know, that *Scorpio* took up 60°.

Eſt Locus, in geminos ubi Brachia concavat Arcus
Scorpios; *& Cauda, flexiſque utrinque Lacertis,*
Porrigit in Spatium Signorum Membra duorum.
——195.

There is a Place above, where *Scorpio*, bent
In Tail, and Arms, ſurrounds a vaſt Extent;
In a wide Circuit of the Heav'ns he ſhines,
And fills the Space of Two celeſtial Signs.

This might be one Reaſon of that Compliment which *Virgil* paid *Auguſtus*, apart from the other, which *Scaliger* aſſigns.—

Anne novum tardis ſidus te menſibus addas,
Qua locus Erigonem inter, Chelaſque ſequentes
Panditur? ipſe tibi jam Brachia contrahit ardens
Scorpius, *& Cæli juſta plus parte reliquit.*
Georg. Lib. I. 32.

Where in the Void of Heav'n a Space is free,
Betwixt the *Scorpion*, and the *Maid*, for thee:
The *Scorpion*, ready to receive thy Laws,
Yield half his Region, and contracts his Claws.

'Tis true, this Poet knew *Libra* very well; but, perhaps, it made no great Figure among the *Aſteriſms* then.

* Elbow'd.

Libra

Libra *die ſomnique pares ubi fecerit horas,*
Et medium Luci, atque Umbris jam dividit Orbem.

——208.

But when *Aſtræa*'s Balance, hung on high,
Betwixt the Nights and Days divides the Sky.

DRYDEN.

How *Taurus* was *painted* at that Time, we learn from his Deſcription.

* Candidus auratis *aperit cum* Cornibus *Annum*
Taurus, & averſo *cedens Canis occidit Aſtro.*

——217.

When with his golden Horns, in full Career,
The *Bull* beats down the Barriers of the Year;
And *Argos*, and the *Dog*, forſake the Northern Sphere.

In the laſt Verſe we have, perhaps, no Occaſion for *Heinſius*'s Correction of *adverſo*, if we compare the Diction here with *Ovid*'s.

Per tamen adverſi gradieris Cornua Tauri.
Met. Lib. II. 80.

The *Bull*'s oppoſing Horns obſtruct the Way.

The Inſtructions *Virgil* gives in the ſame Place, as to Huſbandry, are beſt underſtood from this new Diſpoſition, and may render us ſenſible how much earlier theſe *Phænomena* were then in the Year, to what they are at preſent †.

Ante tibi Eoæ Atlantides abſcondantur, &c.
Georg. Lib. I. 221.

But if your Care to Wheat alone extend,
Let *Maia* with her Siſters firſt deſcend,
And the bright *Gnoſian* Diadem downward bend.

* By reaſon the Firſt Month of the old Luni-ſolar Year (on account of the intercalary Month) began ſometimes a Fortnight after the Æquinox. This may, perhaps, account better for the Propriety of *Virgil*'s Expreſſion *Aperit Annum*, than any of his Commentators have done.

† *Paulatim Obſervatio hujus Ortus & Occaſus neglecta jacet, nec ab aliis uſurpatur, quàm à Poëtis, qui tempora per Circumſtantias tam varii Ortus & Occaſus tot Syderum (quibus nihil pulchrius) deſcribere, & veluti pingere ſolent, quamvis plerumque erroneè, quippe qui Calendarii noſtri Diem per ejuſdem Stellæ Ortum deſcribunt nunc, per quem deſcribebatur tempore Cæſaris, cùm tamen tempora diſcrepent* 14 *diebus ferè.* Greg. Aſtron. *p.* 132.

I know we cannot depend upon all the Exactneſs in a Poet, that might be expected from an Aſtronomer: But, *Virgil* ſeems to have made it his favourite Study.

Me vero primum dulces ante omnia Muſæ,
Quarum ſacra fero ingenti perculſus Amore,
Accipiant; Cœlique Vias, & Sydera monſtrent.
Lib. II. 475.

Would you your Poet's firſt Petition hear,
Give me the Ways of wand'ring Stars to know.

Ovid appears alſo perfectly acquainted with the ancient Figures, and the moſt accurate way of *delineating* them, at the ſame time that he enlivens them with their *Fictions.*

Conſiſtuntque Loco, Specie remanente Coronæ,
Qui medius nixique Genu, anguemque tenentis.
Met. VIII. 181.

— — — — — The Crown retains
It's proper Figure, and a Station gains
Where *Hercules* in bending Poſture ſtands,
And ſtrives to gripe the *Dragon* in his Hands.
Vid. Lib. XIV. 846.

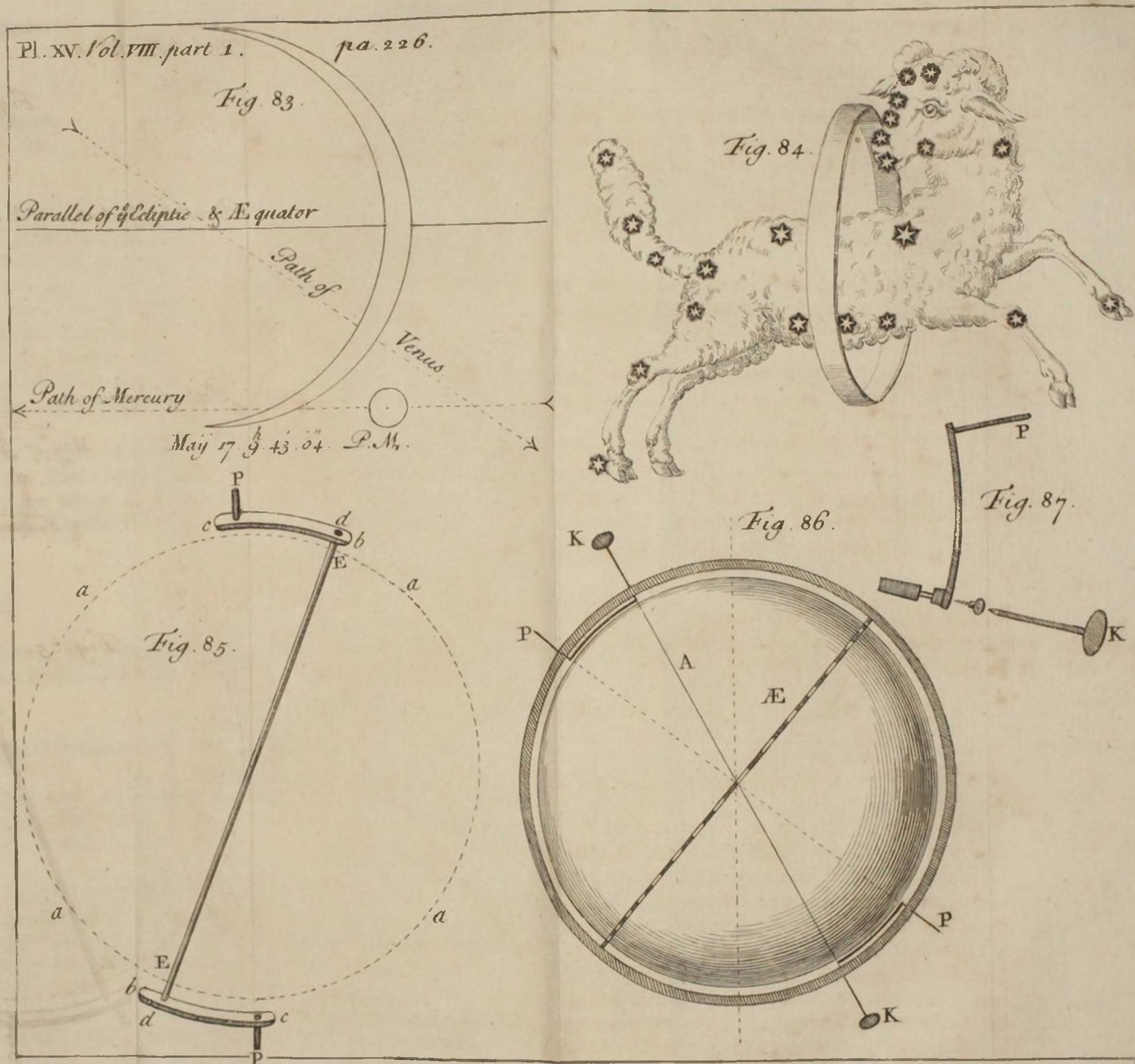
How we came by the Account, it is not material to inquire; but there is *one Line*, wherein he ſeems to have preſerved ſome ancient Tradition, as to the *Pole.*

Quæque Polo *poſita eſt glaciali* proxima *Serpens.*
Lib. II. 173.

The folded *Serpent* next the frozen Pole.

And there is Reaſon to believe one of the Stars of that *Conſtellation* was the *ancient Polar Star*, and might firſt give Riſe to the Denomination; for one in the *Tail of the Dragon*, of *the Third Magnitude*, comes neareſt it of any other. About the Time of *the Flood*, it was within 10′ *of the Pole*, and might paſs for the *Polar Star* a Thouſand Years after among thoſe Writers, from whom *Ovid* copied his Expreſſion. However, this is certain, that *another Star* of that Conſtellation, one of the Fourth Magnitude, was really nearer than any other, when the old *Obſervations* were made, which literally juſtifies *Ovid*'s Account. I might take notice of his exact Repreſentation of the Diſpoſition of *the Ara*, and *Anguis*, when he makes them the two Extremes.

—— *Medio*


Pl. XV. Vol. VIII. part 1.
pa. 226.
Fig. 83.
Parallel of ye Ecliptic & Æquator
Path of Venus
Path of Mercury
May 17. 9h. 43'. 04". P.M.
Fig. 84.
Fig. 85.
P
c
d
b
E
a
a
a
a
E
b
d
c
P
Fig. 86.
K
P
A
Æ
P
K
Fig. 87.
P
K

— — — — *Medio tutissimus ibis :*
Neu te dexterior tortum declinet in Anguem,
Neve sinisterior pressam Rota ducat ad Aram.
Inter utrumque tene.——— ib. 137.

— — — — The middle Way is best,
Nor where in radiant Folds the *Serpent* twines
Direct your Course, nor where the Altar shines.
Shun both Extremes.

But the Inspection of the Globe, when it is fixed in a proper Position, will convey the best Idea of all these Appearances; for we derive this Advantage from the new Construction of it, that it will enable us to place the several *Phænomena before every Eye*; by which means those who have the least Acquaintance with these Studies, must be greatly surprized, and pleased to observe the ancient Accounts minutely verified. It is a sort of living over again the former Ages, *allowing* 1°. 23′. 30″. *for every hundred Years*, according to *Ricciolus* and *Flamsted*, which is a sort of Mean between the other Computations.

I shall not now suggest some other Purposes, that might be served by this Method. It is sufficient to recommend the Invention, that it throws so much Light on the common Classics, to which I have confined this Examination, and which must be my Excuse for the Citations.

PAPERS omitted.

1. A Catalogue of Eclipses of *Jupiter*'s Satellites, for the Year 1734, by *James Hodgson*, F. R. S. Master of the Royal Mathematical School at *Christ's Hospital London*. No. 427. p. 26.

2. The same for the Year 1735. No. 432. p. 279.

3. The same for the Year 1736, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same. No. 436. p. 5.

4. The apparent Times of such of the Immersions, and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1736, together with their Configurations at those Times, represented in a Plate. Ibid. p. 13.

5. An Account of some Observations of the Eclipses of the first Satellite of Jupiter, compared with the Tables, by the same. Ibid. p. 15.

6. The apparent Times of the Immersions and Emersions of *Jupiter*'s Satellites, which will happen in the Year 1737, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same. No. 440. p. 177.

7. The apparent Times of such of the Immersions and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1737, by the same. Ibid. p. 184.

No. 443. p. 501. 8. The Immersions and Emersions of the four Satellites of *Jupiter*, for the Year 1738, computed to the Meridian of the Royal Observatory at *Greenwich* by the same.

Ibid. p. 509. 9. The apparent Times of such of the Immersions and Emersions of *Jupiter*'s Satellites, as are visible at *London* in the Year 1738, by the same.

No. 445. p. 69. 10. The apparent Times of the Immersions and Emersions of *Jupiter*'s Satellites for the Year 1739, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same.

Ibid. p. 76. 11. The apparent Times of such of the Immersions and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1739, by the same.

No. 449. p. 332. 12. The apparent Times of the Immersions and Emersions of the four Satellites of *Jupiter*, for the Year 1740, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same.

Ibid. p. 340. 13. The apparent Times of such of the Immersions and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1740, by the same.

No. 471. p. 602. 14. De Disparitione Annuli Saturni An. 1743, & 1744, ex Epistola a Domino Godofredo Heinsio, ad Dominum Petrum Colinsonum, R. S. S. data.

CHAP. IV.

Of SURVEYING.

A new Plotting Table *for taking* Plans *and* Maps *in* Surveying: *Invented in the Year* 1721, *by* Henry Beighton, F. R. S. No. 461. p. 747. Aug. &c. 1741. Fig. 88. Fig. 89.

IT is a plain smooth Board, about 18 Inches square, and Three-quarters of an Inch thick, Fig. 88. *A B C D*, made of *Mahogany*, *Walnut*, *Pear-tree*, or *Norway Oak*, well clamped at the Ends, or a brass Frame round it, to prevent it's warping, and, as much as possible, shrinking and swelling.

Within Six-tenths of an Inch of two of it's opposite Sides (and parallel to them and one another) are two Grooves *E F*, *G H*, cut on the Face half an Inch deep, to let in two brass Holders in the Shape of *N O*, Fig. 89. which are each of one Piece of cast Brass, like two brass Rulers, joined together at Right Angles. The perpendicular Part is $\frac{1}{10}$ and $\frac{3}{100}$ Parts of an Inch thick, as at *d*, $\frac{1}{2}$ an Inch deep, and a little shorter at each End than the upper Part, which is 17 Inches long, $\frac{4}{10}$ broad, and about 8 Parts of $\frac{1}{100}$ of an Inch thick; about 2 $\frac{1}{2}$ Inches from each End of the Holder, are thick Parts of Bosses in the upright Piece, as at *P* and *Q*, through which are Holes tapped, to receive the Screws *P S*, *Q R*, which Screws go each through a brass Plate as *T* and *V*, fixed by Rivets on the under Side of the Table, and little round Nuts, (as at *a* and *b*) put on them,

to

to confine them to their Shoulders in turning in the Plates, that they neither riſe nor fall; theſe Holders muſt go eaſy in the Grooves, to ſink eaſy and even with the upper Surface of the Table. Then, when the Screws enter the Holes of the Holders, by turning *R* and *S* at the ſame Time forward, the Holders will fall, and pinch down any Papers, *&c.* that are under them; and, turning backward, will riſe and releaſe them. In the Middle of one End of the Table is a Groove to receive the Braſs *W*, which has the ſame ſort of Screw and Fixing as the other, to raiſe or fall it. But the Groove is quadrantal, that the Holder *W* may on Occaſion be turned ſo as to lie all on the outſide the Line *E H*, and to croſs it, in caſe of high Winds, for ſecuring the Papers down, on Three Sides; and a Fourth might be added, but there is ſeldom any Occaſion for it.

To the Centre of the Table underneath, is fixed a braſs Socket, ſo truly made, that the Table may, when ſet, turn round truly horizontally: And a Machine, caſed with Glaſs, in which a Plummet, hangs to ſet the Table level; or the parallel Plates, and glaſs Tubes of Spirit of Wine, may be uſed, to ſet it horizontal, as any one ſees Occaſion to fanſy them.

To any one of the four Edges underneath, is ſcrewed a Box and Needle, ſet to the Variation.

There belongs to this Inſtrument, a ſtrong three-legged Staff, and an Index with plain or teleſcopical Sights, near two Feet long.

The Papers, or Charts, for this Table, are to be either a thin fine Paſtboard, fine Paper paſted on Cartridge-paper, or two Papers paſted together; cut as exactly ſquare as is poſſible, each Side being nearly 16 Inches and an half long, juſt as they may ſlide in eaſily between the upright Part and under the flat Part of the Holders.

Any one of theſe Charts will be put in the Table any of the four ways, be fixed, taken out, and changed at Pleaſure: Any two of them may be joined together true on the Table, if you make each of them meet exact at the Line *L M*, whilſt near one half of each will hang over the Sides of the Table; or by creſting and doubling each, the whole of them ſtill be within the Table. And if Occaſion ſhould happen, as ſeldom it does, by creſting each Paper both Ways through the Middle, four of them may be put on at one time, meeting in the Centre of the Table.

Each Chart is always croſſed by Right Angles through the Middle, for the Purpoſe above, and to make any of them anſwer to the Guide-Lines on the Table, Fig. 88. *I K*, *L M*, drawn quite through the Centre, and the whole Table.——So the grand Objection of ſhifting Papers is obviated. Fig. 88.

It's Facility and Diſpatch,

As alſo it's Certainty, compared with any of the moſt celebrated Inſtruments, I ſhall now briefly ſet forth.

But,

But, in order thereto, it may not be improper to premiſe, or lay down, as *Lemmata*, theſe three Things:

1. *The eſſential Buſineſs or Aim in ſurveying of Lands or Countries, is either to have an exact Plan, or to find the* Area *in ſome known Meaſure.*
2. *Every thing that is ſuperfluous or foreign to ſuch Deſign, is better omitted than taken.*
3. *If a true Survey, and exact Plan be made, every Part will have it's juſt Proportion, and every Angle it's true Opening or Quantity.*

Then what need have we of Degrees, Minutes, *&c.*? They are never made any Uſe of in the Practice of caſting up, or any thing related thereto: For, if from a Station two Lines be drawn by a good Index to two diſtant Objects, will it not be the very Angle, and identically the ſame, as if it had been taken by the moſt celebrated Inſtrument, in Degrees and Minutes, and laid down by a Protractor?

The firſt is much more expeditious, eaſy, and certain, than the other. More expeditious, becauſe thoſe two Lines are ſooner drawn than an Angle can be taken, which done, two thirds of the Work is behind, *viz.* Writing down and Plotting. More eaſy, as done with $\frac{1}{4}$ of the Trouble. More certain, becauſe one may be liable to Miſtakes in taking the Degrees or Minutes; in ſetting down, and in protracting. And if it ſhould ſo happen, that one numerical Angle ſhould be taken, ſet down, or plotted to the wrong Coaſt, (where they depend on one another) ſo great an Error would enſue, that could not be retrievable, but by going on the Spot, and performing the Operation anew. Now the Plotting-Table, after two Stations, proves every thing on the Spot; for, from every Station you are upon, the Index muſt point at the ſame time to any Station on your Map, and it's correſponding Object in the Field; which is a demonſtrative Proof, for nothing but Truth will agree.

In ſeveral Branches of the Mathematics, it is abſolutely neceſſary to take Angles in Degrees, Minutes, and their Subdiviſions, as Aſtronomy, Trigonometry, Navigation, Longimetry, inacceſſible Heights and Diſtances, *&c.* and alſo in taking large Plans, to calculate and prove Things by Trigonometry; which would not only be a Work of Curioſity, but very commendable. But where the Nature of the Thing will admit of as good Proof, with $\frac{1}{10}$ part of the Trouble and Time; it would be like running the Solution of an eaſy Queſtion into a long Proceſs of Algebra or Fluxions, when the plain Rule of Proportion would juſtly anſwer the ſame.

It is objected, That, in ſurveying by the Plotting-Table, the ſhrinking or ſwelling of the Papers, are a great Inconveniency.

In

In Anſwer to this, it may be ſaid, The ſame Inconveniency attends the ſurveying by any other Inſtrument, ſo ſoon as it is plotted; for both Velum and Paper will ſhrink and ſwell in the Houſe on the Alteration of Weather (as well as all Bodies); for a Line of 48 Chains, plotted by a Scale of 3.2 *per* Inch, in a hazy Morning, in a clear Afternoon the ſame Day, meaſured but 47 and an half: And there are various Shrinkings and Swellings, according to the Weather, and Difference of Paper, *&c.*

In the Plotting-Table this Inconveniency is in a great meaſure remedied. For in what State ſoever of the Weather you put Lines on the Chart, the Holders give Marks on the Chart as it then ſtood; if it was moiſt and ſwelled up in the middle Part, you may, when you either caſt up or meaſure Lines, by laying it on a damp Floor, put it in the ſame Condition as it was when you plotted the Lines. If you plotted in dry hot Weather, and are caſting up in damp or moiſt, a little heating by the Fire will reduce it to the ſame State again. Another Remedy I have long uſed is, to plot and meaſure by Scales of the ſame Paper, which will ſhrink or ſwell in proportion as your Map does.

But it will be well to obſerve here, that the ſhrinking and ſwelling alters the Lines only, and not at all the Angles: For, let a Polygon be never ſo much uniformly extended or contracted, each Angle muſt contain the ſame Number of Degrees and Minutes as before. Hence this Objection falls no harder on the Table, than on all other Inſtruments.

And here I intended to have ended this Diſcourſe: But as I have ſome other ſmall Improvements, not only in the Inſtrumental Part, but in a new Method of diſpoſing the Maps, and better adapting them to all ſubſervient Uſes; I proceed.

I ſhould have ſaid before, that each Chart has a *Flower de Lys* on it's North Edge; and, as the Needle is moveable to any Side, Care muſt be taken, that the North End of the Needle, when it ſtands, ſhould point the ſame Way as the *Flower de Lys* on the Charts.

I uſe a Needle about 5 Inches long, placed in an oblong wooden Box, but juſt ſo wide as the Needle may play double the Degrees of the Variation Weſt, *viz.* 30°. In the Middle of one End is the *Flower de Lys*, and the Box is by Studs and Holes always put on the Table oblique to the Quantity of the Magnetical Variation. I make no other Uſe of the Needle, than to ſet the Table in the Meridian, and to prevent any great Miſtakes, in joining or placing the Charts wrong.

I have no more than ½ an Inch of the Needle that appears from under the Table, for the Reaſon it ſhould not be in the Way, or ſo ſubject to be damaged: The making the Box ſo narrow, is to check it's playing, that it may ſooner hang ſtill over the *Flower de Lys.* The wooden Box, lined with Paper, I find preferable to a large braſs Box, and large Glaſs, which in cold and hazy Weather, condenſes the Vapour and Air ſo much, as to make the Needle very languid and dull.

The

Further Uses, by taking a Survey in the new Method by the Plotting-Table.

The Charts, thus taken, are more readily laid together by Numbers on their Edges, which tally, and make up the whole Map in one Plan, or View, and are, in these Squares, more portable.

In the second Place, they are more readily copied, extended, or contracted. For, by having a Frame of Wood that just encompasses a Chart, divided by 19 Threads at equal Distances, and the same at Right Angles, the other Way; each Five or Ten, *&c.* being distinguished by Silk of a different Colour; a Reet is made of 400 Geometrical Squares, from which, having a Velum or Paper so divided by lesser or greater Squares; then drawing or copying by Help of the Lines into those new Squares, you have your true Map contracted or extended.

Large Maps of Lordships are not any ways convenient, or portable, to have recourse to on the Spot or Place they represent; being subject to Damages, unfit to be opened in rainy Weather, very troublesome in the Wind, and very difficult to find out the Part you want. To remedy all these Inconveniences, some Years ago I contrived a new Method of disposing them, in such Manner as makes them more sure, safe, ready, convenient, durable, and portable, than any other Method.

And this is done by imitating the Geography of the World, which first gives the whole, then the several Kingdoms, Countries, Provinces, and minuter Parts and Divisions, severally and more at large.

First, It will be highly necessary, that a *General Map* of the whole Lordship (Country, *&c.*) be drawn in one Sheet of Paper or Velum, to give the Form, Idea, and Proportion, that the Parts bear to the whole, and one another; by which Situations, Bearings of the Towns, Villages, Roads, and remarkable Places, will be seen at one View: And this must be reduced to so small a Scale, as the intended Sheet may comprehend the whole. A Scale of about 11 or 12 Chains in an Inch, will plot a Lordship of more than 2000 Acres, in the Compass of 16 $\frac{1}{4}$ Inches square; which may be a convenient Size to make two Leaves, and open in a Folio Book. This Map may express the Roads, Rivers, Streets, Boundaries, Inclosures, and common Field Lands singly, in case they be not less than 40 or 50 Links in Breadth: The Pieces that contain not less than about 10 Acres, will admit of Room to write the Owners Names and Quantities in Statute Measure, as in Fig. 91 But for all the small Parts, there will not be room to explain them: Therefore I divide the general Map into as many Geometrical Squares, as it took Charts in surveying by the Table, by red Lines, as in Fig. 90 horizontally and perpendicularly, as noted by o,o,o,o, *&c.* which, by a Scale of 32 *per* Inch, may take about 15 Charts in Number: In the openest Place near the Middle of each Square, in a small Circle, I number them with red Figures 1, 2, 3, *&c.* corresponding to the original Charts: And in the Middle of each of their Sides, Numerical Letters, shewing how the particular Maps are to join to each other.

Fig. 91.

Fig. 90.

The

The particular Maps are each as large as the general, and numbered at the Top I. II. III. *&c.* correſponding to the Squares in general, as Fig. 91. where, in the Right Hand Margin, is put V, and at the Bottom IX, ſhewing the Fifth Map tallies to the Side, and the Ninth to the Bottom, or South Part: The general Map being an Index, ſhewing how they join to each other. Fig. 91.

By theſe particular Maps may be ſhewn all the leſſer Quantities, with their Tenure, Owners Names, and Contents; and, by the Scale, are capable of ſhewing the Lengths of any Lines, and the Dimenſions, ſo as to diſcover any Encroachments, and record their Shape and Extents to Poſterity: A moſt valuable Uſe of a Survey and Map.

All theſe Maps are bound up in Order, in a Folio Book, to open freely, which will be not only very portable, but uſeful to have recourſe to on any Occaſion; ſecure from Damages of Weather, as well as more durable and ornamental.

The Terriers to theſe Maps are made in the following Manner; either bound in a Book of a Pocket Size by themſelves, or along with the Maps.

The Names of the Freeholders, Copyholders, Cottagers, Tenants, *&c.* are put in an Alphabetical Order.

Tho. Power.

Refer to the Map.	The Names of the Lands, their Situations, and Boundaries.	Tenure	Freehold			Copyhold				
			a	r	p					
IV. f. 4.	*Calmer-Cloſe* in the Village of *B.*——— the Pariſh of *Gwin W. Townſend* E. Own S.	Freehold	11	1	—	—	—	—		
IV. f. 6.	The Houſe and Home-ſtead called *Broadmoor Horſe Cloſe* W. Own E. N. S.	Copyhold	—	—	—	18	2	28		
IV. e. 6.	*Horſe Cloſe Guinne* W. *Broadmoor* E. *Pitts* N S.	Freehold	17	1	6	—	—	—		

In like Manner, under every different Name, may all the Parcels be expreſſed ſeparately.

To find any Piece or Parcel of Land in the Lordſhip readily, firſt find the Tenant's or Owner's Name in the Alphabetical Order, under which, in the Second Column, may the Parcel be found. The 3d ſhews whether it is Free or Copyhold; the 4th or 5th, the Quantity in Statute Meaſure, either Free or Copyhold.

The numerical Letter in the Margin on the Left IV. ſhews it is in the Fourth particular Map; *f.* 6. refers to the Parts of the Map; find *f.* at the Top, and 6 on the Left Side, and in the Angle of Meeting of thoſe Squares is the Houſe, Cloſe; and ſo for any other.

There is but one Objection I can at preſent foreſee, that can bear any Weight againſt this Method of dividing the general Map, *viz.*

 That

That by dividing the ſame into geometrical Squares, many of the Parcels, Lands, and Grounds, will be cut into two ſeparate Pieces; one Part whereof will lie in one particular Map, and the reſt in another; as in Fig. 91. Map IV. Part of *Calmer* and *Broad-Cloſe* will be in the Vth Map.

In this Caſe, it is uſual to put the Owner's Name, and Quantity, in that which is the greater Part, and in the Terrier refer alſo to the Remainder; where, if the Shape, Lengths, *&c.* are required, they may be diſcovered.

But as this may not be ſatisfactory, or fully anſwer the Objection; the two following Methods will entirely obviate the Difficulty, and make them as fully ſubſervient to all Purpoſes, as any large and entire Map on one Piece.

The 1ſt Method is, to take juſt ſo much in a particular Map as is circumſcribed by ſome known Roads, Lanes, Brooks, Boundaries of particular Owners, or Tenants Lands: This, indeed, will often make the Map very diſproportional, and irregularly ſhaped; but cannot be a material Objection, by reaſon, in Surveys, there is ſeldom any thing regularly ſhaped.

2. The 2d Method is, to have a wider Margin, or rather draw the particular Maps by a ſmaller Scale, as 4 Chains in an Inch, inſtead of 3 Chains 20 Lines; and that will allow Room to add the Parts of the

Fig. 92. Parcels ſo cut off in the Margin, as in Fig. 92. the IVth particular Map varied, where the Whole of *Broadmoor* and *Calmer* is drawn; then in the Vth and IXth particular Map, may the ſmall Parts, which are in the IVth, be drawn in full: Then will they join by indenting or tallying one into anotherr

To reduce a Scale to fit exactly your general Map.

Firſt ſee what Extent the whole Survey takes on the Charts you laid it down by in the Field, *viz.* the greateſt Depth and Breadth, as from the Specimen of the general Map it may appear.

	Depth			Breadth
On the upper Chart is N°	2. = 10 Inches.		N°	8 = 5 Inches
	6. $16\frac{1}{4}\frac{1}{2}$			9 $16\frac{1}{4}\frac{1}{2}$
	11. $16\frac{1}{4}\frac{1}{2}$			10 $16\frac{1}{4}\frac{1}{2}$
	13. $10\frac{1}{2}$			11 $16\frac{1}{4}\frac{1}{2}$
				7 $6\frac{1}{2}$
The Whole	$53\frac{1}{4}$			
		The Whole		$60\frac{1}{2}\frac{1}{2}$ qr.

Then having fixed on the Size of the general Map to be 16,37 Square, I form a Scale of 60 ½ *per* Inch, that may juſt extend the whole Breadth of the 16,37 Inches; by which you may form all the Squares, and Parts of Squares, in Depth and Length, as above; and at Fig. 90. is divided.

The

Pla. XVI. Vol. VIII. part 1. pa. 234.

Fig. 88.

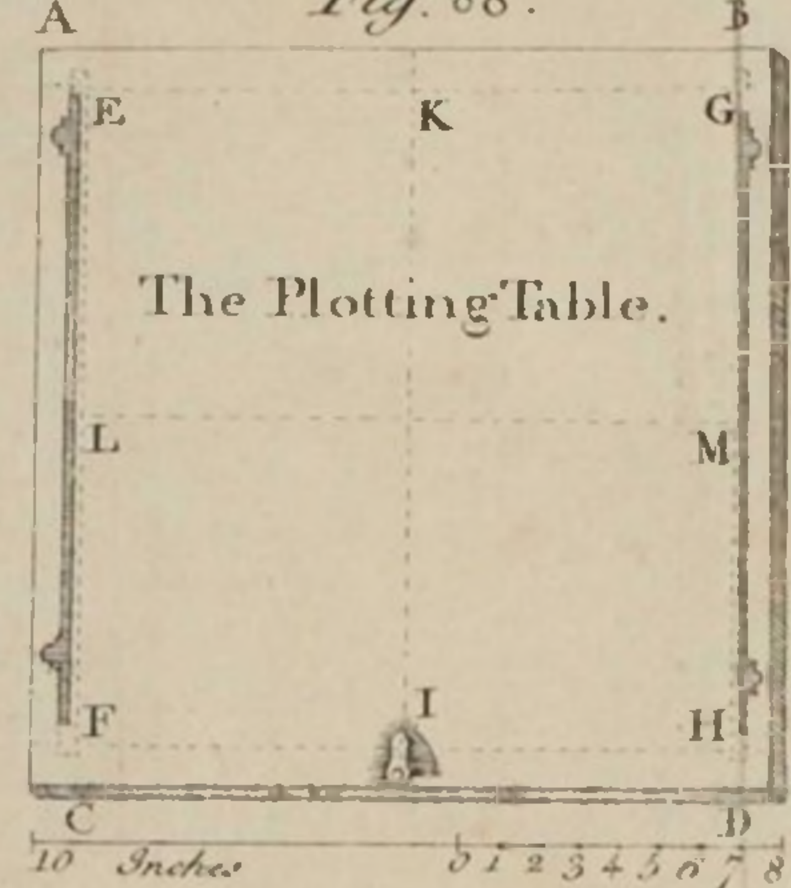


Fig. 89

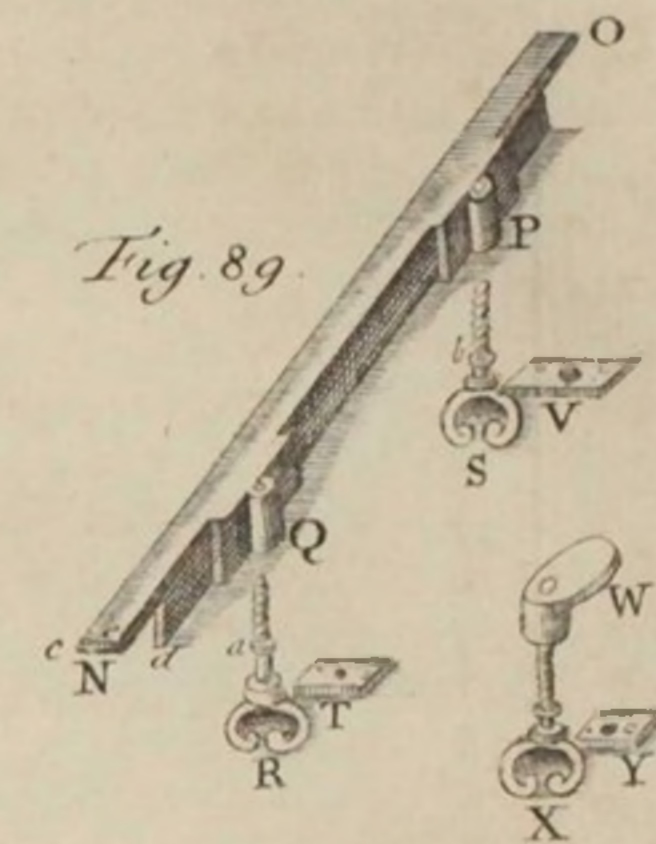


Fig. 90. General Map

Fig. 91. Particular Map No. IV

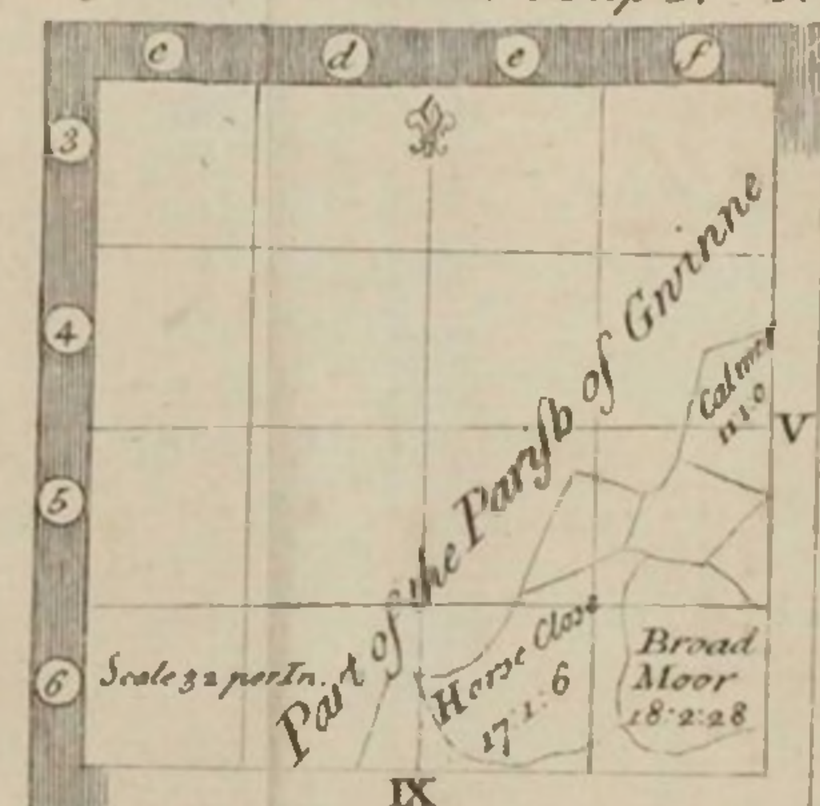
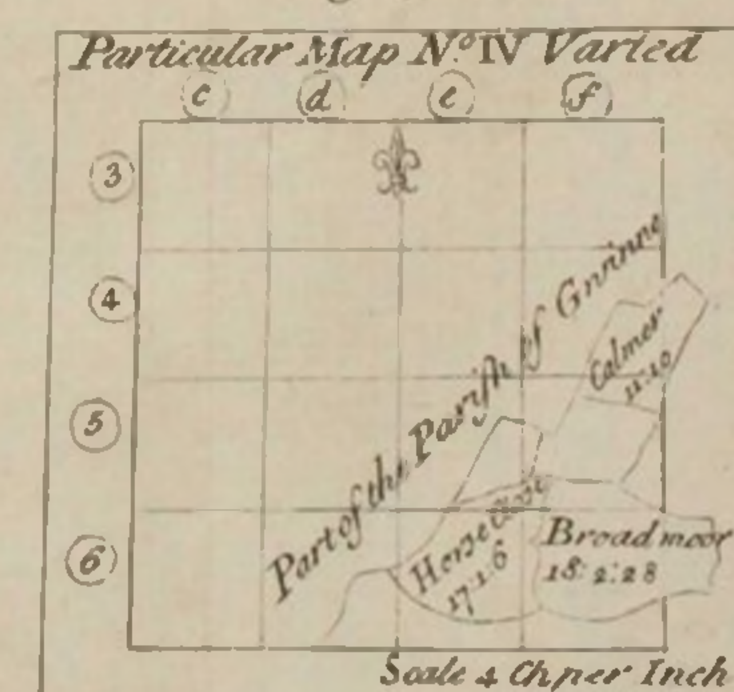


Fig. 92.



16

The Breadth of the whole Map, by a Scale of 32, is 60,62 Inches, which I would reduce into the Compaſs of 16 $\frac{1}{4}$ and $\frac{1}{8}$ = 16,37 Inches.

Divide 60,62 by 16,37, gives 3.7, which multiplied by 3,20, makes the Product 11,84, that is 11 Chains 84 Links in an Inch, the Scale for the general Map.

Thus have I done all I intended; but ſhall obſerve, that ſeveral of theſe Tables have been made, and, as People have fanſied, with Alterations and Additions; but all Variations are not really Improvements. The ſetting it horizontally by Spirit-Tubes, may be curious enough: But as the Difference is very inconſiderable and indiſcernable, when it ſtands 2 or 3 Degrees out of the Level, I ſhall not trouble myſelf or others about it; only further obſerve, that when Grounds are declining much, and very uneven, if the Table ſtands horizontal, unleſs the Sight or Mark on the lower Part is ſo high as it's Top makes a Level with the upper Part of the Table, which is ſeldom done, or practicable, I do not ſee why ſuch a Streſs ſhould be laid on the Inſtrument's being level, when neither the View by the Index, nor the Meaſure of the Line, either can be, or is taken horizontally: If the Sight of the Index ſtand nearly perpendicular at every Obſervation, it is more than ſufficient for any Exactneſs requiſite in a Survey.

CHAP. V.

MECHANICKS.

I. HAVING laſt Year ſhewn ſeveral Perſons in *Holland* the Experiment contrived by Mr *Geo. Graham*, to explain the Doctrine relating to the *Momentum* of Bodies (*viz.* That the *Momentum*, or Quantity of Motion in Bodies, is always as the Maſs multiplied into the Velocity) which Experiment is made with a flat, pendulous Body, that receives the Addition of a Weight equal to itſelf at the lower Part of it's Vibration, and by the Reception of that equal Quantity of Matter always loſes half it's Velocity. Dr *Muſchenbroek*, Profeſſor of Mathematicks and Aſtronomy at *Utrecht*, communicated to me the following Experiment, made in Oppoſition to that which I was ſhewed by Mr Profeſſor *s'Graveſande.* In this laſt a Spring equally bent every time, puſhes forward, unequal Quantities of Matter ſucceſſively, and in every Experiment the Product of the Maſs of the Body by the Square of the Velocity is the ſame; and therefore, as the Quantity of Motion muſt always be the ſame from the ſame Cauſe (*viz.* the ſame Tenſion of the Spring) it follows, by every Experiment, that it is as the Maſs multiplied into the Square of the Velocity.

An Experiment by G. J. s'Graveſande, *Prof. Math.* Leyd. *F..R.S. relating to the Force of moving Bodies, ſhewn to the* Royal Society, *by* J. T. Deſaguliers, *LL.D. F.R.S.* No. 429. p. 143. July. &c. 1733.

Exp. 1.] The pendulous Cylinder is ſhot by the Spring from o Deg. to 7 Deg. meaſured upon a Tangent Line.

Exp. 2.] The Cylinder with a leaden Weight in it that makes it's Weight double, is ſhot forward to 4 Deg. $\frac{9}{10}$.

Exp. 3.] The Cylinder with a Weight in it that made it's Weight triple, was ſhot forward to 4 Degrees and a little farther.

Exp. 4.] The Cylinder with a triple Weight of Lead ſo as to quadruple the whole Weight, was ſhot forward to 3 $\frac{1}{2}$ Deg.

Theſe 4 Experiments at firſt ſeem agreeable to the new Hypotheſis; for according to the old, the Cylinder in the 2d Experiment ought to have gone but to 3 $\frac{1}{2}$ Deg. in the 3d Experiment but to 3 $\frac{1}{3}$ Deg. and in the laſt but to 2 Deg.

But if we take in the Conſideration of Time, all will be reduced to the old Principle. As for Example, let us compare the firſt and laſt Experiments.

In the firſt, the Spring during a certain time acts upon the Cylinder which is driven forward with the Velocity 8. When the quadrupled Weight is driven forward with the Velocity 4 inſtead of 2, it is becauſe the ſame Spring acts twice as long upon the Cylinder before it ceaſes to impel it; and certainly the ſame Cauſe acting twice as long muſt produce a double Effect.

A ſhort Account of Dr Jurin's *Ninth and laſt Diſſertation* De Vi Motrice, *by Mr* John Eames, *F. R. S.* No. 459. p. 607. Jan. *&c.* 1741.

II. The laſt Diſſertation is* new, and treats of the Motive Forces of Bodies, whether they are to be eſtimated by the Velocities, or the Squares of the Velocities, when the Maſſes are equal. The Original of this diſpute among the Mathematicians, the Author aſcribes to a Slip committed by the celebrated Mr *Leibniz*, in the Year 1686, and the Continuance, to the Neglect of the Times, wherein equal Effects are produced. The one Side aſſerts all Cauſes to be equal, whoſe Effects are ſo, whether the Times, during which the Cauſes act, are ſhorter or longer. The other, on the contrary, maintains, that equal Effects may ariſe from unequal Cauſes, if the Times of Action are unequal; that conſequently the Times, as well as the Effects, ought to be taken into the Account.

He wiſhes the Gentlemen on the other Side of the Queſtion would produce ſome Experiment in their Favour, where the Equality of the Times is preſerved; ſince all the Experiments they have hitherto made, and argued from, may juſtly be ſet aſide, as incompetent, on the Account of the Inequality of the Times of Action.

The Author then proceeds to prove the Truth of the common Opinion of the Forces in equal Bodies being proportional to their Velocities. This he does by Three Mediums, the Firſt taken from

* The Eight preceding Diſſertations had been before printed ſeparately; but were now all collected together, with the Addition of this Ninth, and publiſhed in one Volume in *Octavo, London,* 1732.

the Action of a single Spring upon the same Body: The Second from some Experiments of Mr *Mariotte*; the Third from the joint Action of several Springs upon two unequal Bodies.

I. A single Spring, fixed to a moveable horizontal Table, is made to communicate to the same Body, Degrees of Force unquestionably equal, while the Degrees of Velocity communicated at the same time are also undoubtedly equal; therefore the Forces are proportional to the Velocities.

II. In Mr *Mariotte*'s Experiments, the Impressions made upon equal Surfaces in the same Point of Time, are found to be in the Duplicate *Ratio* of the Velocities; but the Masses or Numbers of impinging Particles are in the simple *Ratio* of the Velocities; consequently, the Masses and Velocities conjunctly being in the Duplicate *Ratio*, *i. e.* as the Impressions, must also be as the Forces which made them: Which is the old Opinion.

III. A complicated or bent Spring interposed between two unequal Bodies, acting upon each with an equal Pressure, and during an equal Time, must communicate equal moving Forces to each; but their Velocities are by Experiment reciprocally proportional to their Masses; therefore their Masses, drawn into their respective Velocities, are also equal, as were their moving Forces; and by consequence their moving Forces are as the Masses and Velocities conjunctly: Which is the generally received Opinion.

In the Appendix, the Author answers some of the principal Arguments brought in favour of the contrary Side.

I. The first is drawn from the compound Motion of a Body along the Diagonal of a Rectangle, whose Sides represent the simple Motions. Here it is said, that the simple Forces are no-ways contrary to each other; that being united or added together in the compound Force, that compound Force will not be to both or either of the simple Forces, as the Diagonal is to both or either of the Sides; but as the Square of the Diagonal to the Sum of the Squares of the Sides, or to the Square of either Side respectively. He answers, The simple Forces, while they act in their proper Directions, are not contrary to each other, either Wholly or in Part; but when considered as contributing to the Motion of the Body in the Direction of the Diagonal, Part of the one acts contrary to Part of the other, and destroys it; as is evident, if you resolve each simple Force into two others, one acting along the Diagonal, the other in a Direction perpendicular to it. And then it is to be observed, that the Sum of the two former is equal to the Diagonal (while the two latter destroy each other): Which is perfectly agreeable to the old Opinion, but not at all to the new; for the demonstrating of which this Argument is brought.

II. The second Proof is taken from the equal Compression of 4 equal Springs, before the Force was consumed, by the same Body moving with

with double the Velocity; and labours at the Bottom under the same Parallogism.

III. The last Argument is founded upon the learned and ingenious Mr *Poleni*'s Experiment, wherein equal Cavities are formed in soft Substances, by equal Bodies falling from Heights reciprocally proportional to their Masses. This the Author sets aside, as insufficient, since the Times of forming these equal Cavities are unequal, and unequal Causes may produce equal Effects in unequal Times. The learned Mr *Poleni* does, indeed, reply, and say, that the Formation of these Cavities seems to be instantaneous: But the ingenious Author shews the contrary, and that from a Position allowed of by *Polcni* himself, in his Reply.

Observations made in London, *by Mr* George Graham, *F.R.S. and at* Black-River *in* Jamaica, *by* Colin Campbell, *Esq; F. R. S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of* Isochronal Pendulums *in those Places. Communicated by* J. Bradley, *M. A. Astr. Prof. Savill* Oxon. *F. R. S.* No. 432. p. 302. Apr. &c 1734.

III. 1. Altho' it is now above 60 Years since Mr *Richer* first discovered, that Pendulums of the same Length, do not perform their Vibrations in equal Times in different Latitudes; and tho' several Experiments made since in different parts of the Earth concur to prove, that Pendulums swinging Seconds are in general shorter as we approach the Equator; yet what the real Difference is between their Lengths in different Latitudes, does not seem to have been determined with sufficient Exactness, by the Observations that have hitherto been communicated to the Publick; as may be gathered from Sir *I. Newton*'s *Principia* *, where they are compared as well with each other, as with the Theory of that illustrious Author. It were therefore to be wished, that more of this kind of Experiments could be made with greater Accuracy in proper Places, by such Persons as have sufficient Skill and Opportunities to do it; that we might thereby be enabled to judge with more Certainty, concerning the true Figure of the Earth, and the Nature of it's constituent Parts.

As an Inducement to such as may have it in their Power to put the like again into Practice, I shall lay before the Society, an Account of a very curious Experiment of this Sort lately made in *Jamaica*, by *Colin Campbell*, Esq; He has furnished himself with an Apparatus of Instruments not unworthy the Observatory of a Prince; among which is a Clock whose Pendulum vibrates Seconds, made by Mr *George Graham*, who judging that an Opportunity was now offered of trying with the utmost Exactness, what is the true Difference between the Lengths of Isochronal Pendulums at *London* and *Jamaica*, readily embraced it; and in framing the Parts of the Clock, carefully contrived, that it's Pendulum might at pleasure be reduced to the same Length, whenever there should be occasion to remove the Clock from one Place, and set it up in another.

This Clock being chiefly designed for Astronomical Observations, had no striking Part, and it's Pendulum was adjusted to such a Length, that in *London* is vibrated Seconds, of Siderial, and not of Solar Time.

* Lib. III. Prop. 20.

When

When it was finiſhed, Mr *Graham* fixed it up in a Room ſituated backward from the Street, and on the North-ſide of his Houſe, to prevent it's being diſturbed by Coaches, or other Carriages that paſſed through the Street, and that it might be as little affected by the Sun as poſſible. Having ſet it going, he compared it with the Tranſits of the Star *Lucida Aquilæ* over the Meridian, which paſſed

	th		h.	′	″	
	20	at	8	59	15	
	22	at	8	59	18	
Auguſt,	23	at	8	59	20½	
1731.	25	at	8	59	22	by the Clock.
	28	at	8	59	25½	
	29	at	8	59	26	
	30	at	8	59	27	

Hence it appears, that the Clock gained 12″ in 10 Apparent Revolutions of the Star.

In order to eſtimate how much the Pendulum may be lengthened by greater Degrees of Heat, or how much ſlower the Clock would go on that Account when removed into a warmer Climate, a Thermometer was fixed by the Side of it; and between the Hours of 10 and 11 in the Morning, and at Night, notice was taken at what Height the Spirits ſtood, and the mean Height for each Day was as follows:

	th	*Therm.*	
	21	32 ½	Diviſions.
	22	30 ¾	
	23	28 ¾	
	24	27 ¼	
Auguſt, 1731.	25	28 ¼	Hence the mean Height for all theſe Days was about 28 ½
	26	27 ¼	Diviſions.
	27	27 ½	
	28	27 ½.	
	29	27 ½	
	30	27 ¼	

The Clock-Weight that keeps the Pendulum in Motion is 12 ℔. 10½ *oz.* and is to be wound up once in a Month. The Weight of the Pendulum itſelf is 17 ℔. and (during the Time that the Clock was compared with the Tranſits of the Star) it vibrated each way from the Perpendicular 1° 45′. The Magnitude of the Vibrations was eſtimated by means of a Braſs Arc, which was fixed juſt under the lower end of the Rod of the Pendulum, and divided into Degrees, *&c.*

Auguſt

August 31, Mr *Graham* took off the Weight belonging to the Clock, and hung on another of 6 ℔. 3 *oz.* and with this Weight the Pendulum vibrated only $1^{\circ}\ 15'$ on each Side; and the Clock went $1''\frac{1}{2}$ ſlower in 24 Hours, than when it's own Weight of 12 ℔. 10 $\frac{1}{2}$ *oz.* was hung on.

This Experiment ſhews, that a ſmall Difference in the Arcs deſcribed by the Pendulum, or a ſmall Alteration in the Weight that keeps it in Motion, will cauſe no great Difference in the Duration of the Vibrations; and therefore a little Alteration in the Tenacity of the Oil upon the Pivots, or in the Foulneſs of the Clock, will not cauſe it to accelerate or retard it's Motion ſenſibly; from whence we may conclude, that whatever Difference there ſhall appear to be, between the going of the Clock at *London* and in *Jamaica*, it muſt wholly proceed from the lengthening of the Pendulum by Heat, and the Diminution of the Force of Gravity upon it.

A particular written Account of the Obſervations and Experiments hitherto taken Notice of, was delivered to me by Mr *Graham* in *Sept.* 1731, about the ſame Time the Clock was put on Ship-board to be carried to *Jamaica*. He likewiſe ſent very full Directions to Mr *Campbell*, deſcribing in what manner the Clock was to be fixed up, and how the Pendulum might be reduced exactly to the ſame State as it was when in *England*; but no Intimation was given concerning the going of the Clock, that the Experiment might be made with all poſſible Care, and Caution, and without any Byaſs, or Prejudice, in Favour of any Hypotheſis, or former Obſervations.

In *July* 1732, we received an Account of the Succeſs of the Experiment, by the Hands of Mr *Joſeph Harris*, who was preſent at the making of it in *Jamaica*, whither he went the Year before with Mr *Campbell*, in order to aſſiſt him in his Deſign of erecting an Obſervatory for the Improvement of Aſtronomy, and the promoting other Parts of Natural Knowledge in that Iſland: But his ill State of Health obliging him to return into *England*, he brought with him the Original Journal of the Obſervations of the Tranſits of two Stars (*viz. Syrius* & *β Canis Majoris*) over the Meridian, compared with the Clock, after it was fixed up in *Jamaica*, as Mr *Graham* had directed; together with the Height of the Spirits of the forementioned Thermometer, upon the ſeveral Days of Obſervation.

The chief of thoſe Obſervations are contained in the following Table, the 1ſt Column whereof ſhews the Day of the Month; the 2d, the Name of the Star, and the Time by the Clock of it's obſerved Tranſit over the Meridian; the 3d contains the Hour of the Day, when the Thermometer was obſerved, together with the Height of the Spirit at thoſe Hours; the Morning Hours being denoted by the Letter A, and thoſe of the Afternoon, by the Letter P.

1732

1732	Canis Majoris.	Time of Transit. h	'	"	Hour of Day. h		Thermometer.
Jan. 23	β	11	59	50	10½	A	14¼
	α	12	22	14	9½	P	11
24	Cloudy.				11½	A	15¼
25	β	11	55	40	8½	A	17⅓
	α	12	18	4	9¼	P	11¼
26	β	11	53	35	8	A	20
	α	12	16	00	2	P	8½
					9	P	10
27	β	11	51	31	7	A	17½
	α	12	13	55	2	P	8½
					9¼	P	12½
28	β	11	49	26	7	A	20½
	α	12	11	51	2	P	11
					10	P	12
29	β	11	47	22	6¼	A	19
	α	12	9	46	3	P	9
					9	P	11¼
30	Cloudy.				7	A	20½
					4	P	7
					11	P	13
31	β	11	43	12	7	A	20
	α	12	5	37	9	P	8½
Feb. 1	β	11	41	8½	10	A	18¾
	α	12	3	33	11	P	16
2	β	11	39	0	9½	A	17½
	α	12	1	23½	2	P	9
					5	P	6
					9	P	8½
3	β	11	36	53	8½	A	19
					1	P	9½
					9	P	9
4	β	11	34	46	6¼	A	18
	α	11	57	11	12		9½
					9	P	8

1732	Canis Majoris.	Time of Transit. h	'	"	Hour of Day. h		Thermometer.
Feb. 5	β	11	32	40	7½	A	19¼
	α	11	55	5	3½	P	6
					8½	P	87
6	β	11	30	35	7	A	18½
	α	Cloudy.			4	P	7½
					8½	P	8
7	β	11	28	31	7	A	20½
	α	11	50	55	12		12
					8½	P	8⅓
8	β	Cloudy.			6½	A	21½
	α	11	48	50	8½	P	8¼
9	β	11	24	20	9½	A	14
	α	11	46	44	8½	P	8
10	β	11	22	12½	7¼	A	16
	α	11	44	37	11½	A	10
					3¼	P	3¼
					8½	P	6
11	β	11	20	6	7½	A	16
	α	11	42	30	12		9½
					8¼	P	5¼
12	β	11	18	0	10	A	17½
	α	11	40	24	12		13
					8	P	5¼
13	Cloud.				9	A	17
					8	P	6
14	β	Cloudy.			7½	A	16
	α	11	36	15	12		11
					8	P	10
15	Clouds.				9	A	18
					12		13¼
					8½	P	7½
16	β	Cloudy.			8	A	14
	α	11	32	4	8	P	7
17	β	11	7	34	12		12
	α	11	29	59	8	P	6¼
18	β	11	5	29	12		12¼
	α	11	27	53			

The Pendulum, during this Interval, vibrated about 1° 52' each way from the Perpendicular.

 The

The Tranſits of the Stars over the Meridian, were obſerved with a Teleſcope, fixed at Right Angles to an Horizontal Axis, whoſe Ends lay exactly Eaſt and Weſt; by the turning of which Axis, the Line of Collimation of the Teleſcope, was conſtantly directed in the Plain of the Meridian. This Inſtrument was daily adjuſted to a Mark, fixed in the Meridian: and in the Journal, between the 2d and 3d of *February*, the following Remark was made.

N. B. *This Day was hotter than uſual, as appears by the Thermometer; and the Tranſit Inſtrument had loſt the Level a little, but after we had adjuſted it, it pointed exactly to our Meridian Mark, and therefore we are at a loſs for the Cauſe of this Difference in the Clock.*

From the foregoing Table it appears, that the Clock loſt 54′ 21″ in 26 Revolutions of the Stars; that is, about 2′ 5″ ½ in one Revolution, the Difference from this Medium ſomewhat varying, upon account of a greater, or leſs Degree of Heat on different Days.

The Mean of all the obſerved Heights of the Thermometer from *January* 26th, to *February* 18th, was about 12 ½ Diviſions. Therefore, the Difference between the mean Heights of the Thermometer, at *Jamaica* and *London*, during the Intervals of the reſpective Obſervations, was 15 ½ Diviſions; the Spirits ſtanding ſo much higher in *Jamaica*, becauſe of the greater Heat in that Iſland.

That we might be able to judge, how much the different Degrees of Heat, correſponding to any Number of Diviſions upon this Thermometer, would cauſe the Clock to go ſlower, by lengthening it's Pendulum, Mr *Graham* took Notice of the loweſt Point, to which the Spirits ſunk at *London* in the Winter, 1731; and the greateſt Height to which they roſe in the following Summer; and comparing the Motion of the Spirits in this Thermometer, with the Alterations in another made with Quickſilver, which he has for ſome Years made uſe of; he concluded, that at *London* the Spirits in this Thermometer would ſtand *(communibus Annis)* about 60 Diviſions higher in Summer than in Winter.

By ſeveral Years Experience, he has likewiſe found, that his Clocks (of the ſame ſort with Mr *Campbell*'s) when expoſed, as uſual, to the different Degrees of Heat and Cold of our Climate, do not vary in their Motion above 25 or 30 Seconds in a Day.

From theſe Obſervations and Experiments therefore we may reaſonably conclude, that ſufficient Allowance will be made for the Lengthening of the Pendulum by Heat, if we ſuppoſe the Clock, upon that Account, to go one Second in a Day ſlower, when the Spirits of this Thermometer ſtand two Diviſions higher, and in the ſame Proportion for other Heights.

Admitting then, that the mean Height of the Thermometer, while the Clock was compared with the Stars at *Jamaica*, exceeded that at *London*

London between 15 and 20 Divisions; if we allow 8, or 9 Seconds, upon that Account, the remaining Difference must be wholly owing to the Difference of the Force of Gravity in the two Places.

Upon comparing the Observations, it appears, that in one apparent Revolution of the Stars, the Clock went 2′ 6″ ½ slower in *Jamaica*, than at *London*; deducting therefore 8″ ½, on account of the greater Heat in *Jamaica*, there remains a Difference of 1′ 58″, which must necessarily arise from the Diminution of Gravity, in the Place nearest the Equator.

I have allowed the Clock to have lost somewhat more, on account of the Difference of Heat, than the mean Heights of the Thermometer may seem to require, upon a Supposition, that the total Heat of the Days, compared with the Cold of the Nights, bears a greater Proportion in *Jamaica*, than *London*; but if that Supposition be not admitted, then the Clock in *Jamaica*, must have gone rather more than 1′ 58″ in a Day slower than in *England*.

Mr *Campbell*'s Observations were made at *Black-River*, in 18° North Latitude. Now if we suppose, with Sir *I. Newton*, that the Difference in the going of the Clock, is owing to the greater Elevation of the Parts of the Earth towards the Equator, it will follow from these Observations, and what is delivered by him in *Lib.* III. *Prop.* 20. of his *Principia*, that the Æquatorial Diameter is to the Polar, as 190 to 189; the Difference between them being 41½ Miles; which is somewhat greater than what Sir *I. Newton* had computed from his Theory, upon the Supposition of an uniform Density in all the Parts of the Earth.

I shall not enter into the Dispute about the Figure of the Earth, but at present suppose, with Sir *I. Newton*, that the Increase of Gravity, as we recede from the Æquator, is nearly as the Square of the Sine of the Latitude; and that the Difference in the Length of Pendulums, is proportional to the Augmentation, or Diminution of Gravity. Upon these Suppositions, I collect from the forementioned Observations, that, if the Length of a simple Pendulum (that swings Seconds at *London*) be 39.126 *English* Inches, the Length of one at the Æquator, would be 39.00, and at the Poles 39.206. And (abstracting from the Alteration on account of different Degrees of Heat) a Pendulum-Clock that would go true Time under the Æquator, will gain 3′ 48″¼ in a Day at the Poles; but the number of Seconds which it would gain in any other Latitude, would be to 3′ 48″¼ nearly, as the Square of the Sine of that Latitude is to the Square of the Radius: From whence it follows; that the Number of Seconds which a Clock will lose in a Day, upon it's Removal to a Place nearer to the Æquator, will be to 3′ 48″¼ nearly, as the Difference between the Squares of the Sines of the Latitudes of the two Places to the Square of the Radius. Thus the Difference of the Squares of the Sines of 51°½, and 18°, the Latitudes of *London* and *Black-River* being to the

the Square of the Radius, as 118 to $228\frac{1}{2}$, the Clock will go $1' \, 58''$ in a Day ſlower at *Black-River* than at *London*, as was found by Obſervation.

It may be hoped, that Mr *Campbell*'s Succeſs in this Experiment, and the little Trouble there is in making it, will induce thoſe Gentlemen who may hereafter carry Pendulum-Clocks into diſtant Countries, to attempt a Repetition of it after his manner; that is, by keeping or reſtoring the Pendulums of their Clocks to the ſame Length in the different Places, and carefully comparing them with the Heavens, and at the ſame Time taking notice of the different Degrees of Heat, by means of a Thermometer. From a Variety of ſuch Experiments, we ſhould be enabled to determine how far Sir *I. Newton*'s Theory is conformable to Truth, with much greater Certainty than from thoſe Trials which are made by actually meaſuring the Lengths of ſimple Pendulums; becauſe a Difference of $\frac{1}{100}$ Part of an Inch, in the Length of a Pendulum, correſponds to $11''$ in a Day; and it being eaſy to obſerve how much a Clock gains, or loſes in a Day, even to a ſingle Second; it is certain, that by means of a Clock, compared in the manner abovementioned, we may diſtinguiſh a Difference (in the Lengths of Iſochronal Pendulums) of $\frac{1}{1000}$ Part of an Inch, or leſs; whereas it will be ſcarce poſſible to meaſure their true Lengths, without being liable to a greater Error than that. Beſides, by taking Notice how much a Clock gains, or loſes, upon the falling or riſing of a Thermometer, we can better allow for the different Degrees of Heat in this, than in the other Method of making the Experiment, by actual Meaſurement; ſince it may not be eaſy to determine how much the Meaſure itſelf, which we make uſe of, will be lengthened by different Degrees of Heat.

For theſe Reaſons, I eſteem Mr *Campbell*'s Experiment to be the moſt accurate of all that have hitherto been made, and propereſt to determine the Difference of the Gravity of Bodies in different Latitudes; and therefore I ſhall ſubjoin a Table, which I computed from it, containing the Difference of the Length of a ſimple Pendulum, ſwinging Seconds at the Æquator, and at every 5th Degree of Latitude, together with the Number of Seconds that a Clock would gain in a Day, in thoſe ſeveral Latitudes, ſuppoſing it went true, when under the Æquator; by means of which any one may readily compare other the like Obſervations with his; and thereby diſcover whether the Alteration of Gravity in all Places be uniform, and agreeable to the Rule laid down by Sir *I. Newton* or not.

The

The Latitude of the Place.	The Difference of the Length of the Pendulum in Parts of an *English* Inch.	Seconds gained by a Clock in one Day.	The Latitude of the Place.	The Difference of the Length of the Pendulum in Parts of an *English* Inch.	Seconds gained by a Clock in one Day.
Deg.	Inch.	Seconds.	Deg.	Inch.	Seconds.
5	0. 0016	1. 7	50	0. 1212	134. 0
10	0. 0062	6. 9	55	0. 1386	153. 2
15	0. 0138	15. 3	60	0. 1549	171. 2
20	0. 0246	26. 7	65	0. 1696	187. 5
25	0. 0369	40. 8	70	0. 1824	201. 6
30	0. 0516	57. 1	75	0. 1927	213. 0
35	0. 0679	75. 1	80	0. 2003	221. 4
40	0. 0853	94. 3	85	0. 2050	226. 5
45	0. 1033	114. 1	90	0. 2065	228. 3

Experiments concerning the Vibrations *of* Pendulums. *By the late* W. Derham, *D. D. F. R. S. and Canon of* Windsor. No. 440. p. 201. Jan. *&c.* 1736.

2. The preceding Article brought to my Mind some Experiments I made some Years ago, that may be of Use in Observations of this Nature.

The first that I shall take notice of, shall be some Experiments I made in the Year 1704, with excellent Instruments, concerning the *Vibrations of Pendulums in Vacuo* *. The Sum of which is, That the Vibrations in *Vacuo* were larger than in the *open Air*, or Receiver unexhausted: Also that the Enlargement or Diminution of the Vibrations, was constantly in Proportion to the Quantity of Air, or Rarity, or Density thereof, which was left in the Receiver of the Air-Pump. And as the Vibrations were larger or shorter, so the Times were augmented, or diminished accordingly; *viz.* 2″ in an Hour slower, when the Vibrations were largest, and less and less, as the Air was re-admitted, and the Vibrations shortened.

But notwithstanding the Times were slower, as the Vibrations were larger, yet I had Reason to conclude, that the Pendulum really moved quicker in *Vacuo*, than in the *Air*, because the same Difference, or Enlargement of the Vibrations (as two Tenths of an Inch on a Side) would cause the Movement, instead of 2″ in an Hour to go 6 or 7″ slower in the same Time; as I found by nice Experiments.

The next Experiments I shall mention, I made at several Times, in 1705, 1706, and 1712, by the Help of a good Month Piece that swings Seconds. The Weight that then drove it, was about 12 or 13 ℔, and it kept Time exactly by the Sun's mean Motion: But by hanging on 6 ℔ more, the Vibrations were enlarged; yet the Clock gained but 13 or 14″ in a Day.

And as the Increase or Diminution of the Power that drives the Clock, doth accelerate or retard it's Motion; so, no doubt, doth

* See Vol. IV. Part II. Chap. I. §. 32.

Cleanness

Clamness or *Foulness* affect it, and so doth *Heat* and *Cold*; for all have the same Effect upon the Pallets and Pendulum.

The last Experiments I shall mention, I made in 1716 and 1718, to try what Effects *Heat* and *Cold* had upon *Iron Rods* of the same Length, or as near as I could to those that swing Seconds. I made my Experiments with round Rods of about $\frac{1}{4}$ of an Inch Diameter, and with square Rods, of about $\frac{1}{4}$ of an Inch Square. The Effects on both which were the same.

At first I took the exact Length of the Rods, in their natural Temper. Then I heated them as well as I could in a Smith's Fire, from End to End nearly to a *Flaming Heat*; by which means, they were lengthened $\frac{2}{10}$ of an Inch. Then I *quenched* them in cold Water; which made them $\frac{7}{100}$ of an Inch shorter than in their natural State.

Then I warmed them to (as near as I could guess) the *Temper* of my Body; by which means they were about $\frac{1}{100}$ of an Inch longer than in their natural Temper.

Afterwards I cooled them in a *strong frigorifick Mixture* of common Salt and Snow, which shortened them $\frac{3}{100}$ Parts of an Inch.

Afterwards I measured these Rods, when heated in an *hot Sun*, which lengthened them $\frac{2}{100}$ Parts of an Inch more than their natural Temper.

All these Experiments seem to concur in resolving the Phænomenon of *Pendulum-Clocks going slower under the Æquator* than in the Latitudes from it: But yet I confess, that I have too good an Opinion of Sir *I. Newton*'s Notion of the Sphæroidal *Figure* of the *Earth*, to part easily with it; and therefore I leave it to the Consideration of others, how far the Figure of the Earth, and how far Heat and Cold, and the Rarity and Density of the Air, are concerned in that Phænomenon.

An Account of the Influence which two Pendulum-Clocks were observed to have upon each other, by Mr John Ellicott, *F. R. S.* No. 453. p. 126. April *&c.* 1739.

IV. 1. The two Clocks upon which the following Observations were made, being designed for Regulators, particular Care was taken to have every Part made with all possible Exactness: The two Pendulums were hung in a manner different from what is usual; and so disposed, that the Wheels might act upon them with more Advantage. Upon Trial they were found not only to move with greater Freedom than common, but an heavier Pendulum was kept in Motion by a smaller Weight. They were in every respect made as near alike as possible. The Ball of each of the Pendulums weighed above 23 ℔; and required to be moved about 1° 5′ from the Perpendicular, before the Teeth of the swing Wheel would scape free of the Pallets; that is, before the Clocks would be set a-going. The Weight to each was 3 ℔, which would cause either of the Pendulums in their Vibrations to describe an Arch of 3°. The two Clocks were each in Cases, which shut very close, and placed Sideways to one another, so near that when the Pendulums were at Rest, they were little more than about 2 Feet asunder. The odd *Phænomena* observed in them were these: In less than 2 Hours after they were set a-going, one of them (which I call N° 1.) was found to stop; and

and when ſet a-going again, (as it was ſeveral times) would never continue going two Hours together. As it had always kept going with great Freedom, before the other Clock (which I call N° 2.) was placed near it, this led me to conceive it's ſtopping muſt be owing to ſome Influence the Motion of one of the Pendulums had upon the other; and upon watching them more narrowly, I found the Motion of N° 2. to increaſe as N° 1. diminiſhed; and at the time N° 1. ſtopped, N° 2. deſcribed an Arch of 5°, that is nearly 2° more than it would have done, if the other had not been near it, and more than it did move in a ſhort time after the other Pendulum came to be at Reſt: This made me imagine that they had a mutual Influence upon each other. Upon this I ſtopped the Pendulum of N° 2. leaving it quite at Reſt, and ſet N° 1. a-going, the Pendulum deſcribing as large an Arch as the Caſe would permit, *viz.* about 5°. In about 20 Minutes after, I went to obſerve whether there was any Motion communicated to the Pendulum N° 2. when, to my great Surprize, I found the Clock going, and the Pendulum to deſcribe an Arch of 3°, whereas at the ſame time N° 1. did not move 4°. In about half an Hour after, N° 1. ſtopped, and the Motion of N° 2. was increaſed to very near 5°. I then ſtopped N° 2. a ſecond time, and ſet N° 1. a-going, as before; and ſtanding to obſerve them, I preſently found the Pendulum of N° 2. to begin to move, and the Motion to increaſe gradually, till in 17′ 40″ it deſcribed an Arch of 2° 10′, at which time the Wheel diſcharging itſelf of the Pallets, the Clock went. The Arches of the Vibrations continued to increaſe, till (as in the former Experiment) the Pendulum moved 5°; the Motion of the Pendulum N° 1. gradually decreaſing all the while, as the other increaſed; and in three Quarters of an Hour after, it ſtopped. I then left the Pendulum of N° 1. at Reſt, and ſet N° 2. a-going, making it deſcribe an Arch of 5°; it continued to vibrate leſs and leſs, till it deſcribed but about 3°; in which Arch it continued to move all the time I obſerved it, which was ſeveral Hours. The Pendulum of N° 1. ſeemed but little affected by the Motion of N° 2. I tried theſe Experiments ſeveral times over, without finding any remarkable Difference. The freer the Room was from any Motion (as Peoples walking about in it, *&c.*) I found the Experiments to ſucceed the better; and once I found N° 2. ſet a-going in 16′ 20″, and N° 1, at that time ſtopped in 36′ 40″.

2. In my former Account I took Notice, that the two Clocks were in ſeparate Caſes, and that the Backs of them reſted againſt the ſame Rail; that the Pendulums, when at Reſt, were about 2 Feet aſunder, and weighed about 23 ℔ each, and were made to move with ſuch Freedom, that a Weight of 3 ℔ would cauſe either of the Pendulums to deſcribe an Arch of three Degrees. The moſt remarkable Particulars then obſerved in them were theſe: If the Pendulum of one of the Clocks, which (for Diſtinction ſake) I called N° 2. was left at Reſt, and that of the other, which I called N° 1. was ſet a-going, this would, in about

—Further Obſervations and Experiments; by the ſame. Ibid. p. 128.

16 Minutes,

16 Minutes, communicate ſo great a Quantity of Motion to N° 2. as would make it's Pendulum deſcribe an Arch of above two Degrees, and would ſet the Work a-going: That the Motion of the Pendulum of N° 1. conſtantly decreaſed as that of N° 2. increaſed, and after about 30 Minutes it did not deſcribe an Arch ſufficient to free the Teeth of the Wheel from the Pallets, ſo that the Clock ſtopped. At the ſame time the Pendulum of N° 2. deſcribed an Arch of five Degrees, which was two Degrees more than it would have done, had it not been affected by the Motion of N° 1. Upon leaving the Pendulum of N° 1. at Reſt, and ſetting N° 2. a-going, the Pendulum of N° 1. was found to be but little affected, and never moved ſufficiently to ſet the Work a-going. Theſe ſeemingly different Effects, which the two Clocks had upon each other, I ſhall now endeavour to account for.

The Manner in which the Motion is communicated to the Pendulum at Reſt, I conceive to be thus: As the Pendulums are very heavy, when either of them is ſet a going, it occaſions by it's Vibrations a very ſmall Motion, not only in the Caſe the Clock is fixed in, but, in a greater or leſſer Degree, in every thing it touches; and this Motion is communicated to the other Clock, by means of the Rail, againſt which both the Caſes bear. The Motion thus communicated, which is too ſmall to be diſcovered but by means of ſome ſuch-like Experiments as theſe, will, I doubt not, be judged by many, inſufficient to make ſo heavy a Pendulum deſcribe an Arch of 2°, or large enough to ſet the Work a-going; and indeed it would be ſo, but for the very great Freedom with which the Pendulum is made to move, ariſing from the Manner in which it is hung. This appears from the very ſmall Weight required to keep it going, which, when the Clock was firſt put together, was little more than one ℔. And if the Weight was taken off, and the Pendulum made to ſwing two Degrees, it would make 1200 Vibrations before it decreaſed half a Degree, ſo that it would not loſe the $\frac{1}{3000}$ part of an Inch in each Vibration. Indeed if the Weight was hung on, the Friction would be increaſed, and the Pendulum would not move quite ſo freely; but even in that Caſe it was found to loſe but little more than the $\frac{1}{2000}$ part of an Inch, or about three Seconds of a Degree, in one Vibration; and therefore if the Motion communicated to it from the other, will make it deſcribe an Arch exceeding $3''$, the Vibrations muſt continually increaſe till the Work is ſet a-going. And that the Motion is communicated in the manner above ſuppoſed, is confirmed by the following Experiments:

A Prop was ſet againſt the Back of the Caſe of N° 2. to prevent it's bearing againſt the Rail; and N° 1. was ſet a-going; then obſerving them for ſeveral Hours, I could not perceive the leaſt Motion communicated to N° 2. I then ſet both the Clocks a-going, and they continued going ſeveral Days; but I could not find they had any Influence upon each other. Inſtead of the Prop againſt the Back of the Caſe, I put Wedges under the Bottoms of both the Caſes, to prevent their bearing againſt the Rail; and ſtuck

a Piece

a Piece of Wood between them, juſt tight enough to ſupport it's own Weight. Then ſetting N° 1. a-going, I found the Influence ſo much increaſed, that N° 2. was ſet a-going in leſs than ſix Minutes, and N° 1. ſtopped in about ſix Minutes after. In order to try what Difference would ariſe, if the Clocks were fixed on a more ſolid Floor, I placed them (exactly in the ſame manner as in the laſt Experiment) upon the Stone Pavement under the Piazza's of the *Royal Exchange*, and ſtuck the Piece of Wood between them, as before; and ſetting N° 1. a-going, the only Difference I could perceive, was, that it was 15 Minutes before N° 2. was ſet a-going, and N° 1. continued going near half an Hour before it ſtopped. From theſe Experiments I think it plainly appears, that the Pendulum which is put in Motion, as it moves towards either ſide of the Caſe, makes the Preſſure upon the Feet of the Caſe to be unequal, and, by it's Weight, occaſions a ſmall Bearing or Motion in the Caſe on that Side towards which the Pendulum is moving; and which, by the Interpoſition of any ſolid Body, will be communicated to the other Clock, whoſe Pendulum was left at Reſt. The only Objection to this, I conceive, is the different Effects which the two Pendulums ſeemed to have upon each other. But this I hope to explain to Satisfaction.

For, notwithſtanding theſe different Effects, I ſoon found, by ſeveral Experiments, that the two Clocks mutually affected each other, and in the ſame Manner, though not with equal Force; and that the Varieties obſerved in their Actions upon each other, aroſe from the unequal Lengths of their Pendulums only.

For, upon moving one of the Clocks to another Part of the Room, and ſetting them both a-going, I found that N° 2. gained of N° 1. about one Minute 36 Seconds in 24 Hours. Then fixing both againſt the Rail, as at firſt, I ſet them a-going, and made the Pendulums to vibrate about four Degrees; but I ſoon obſerved that of N° 1. to increaſe and that of N° 2. to decreaſe; and in a ſhort time it did not deſcribe an Arch large enough to keep the Wheels in Motion. In a little time after it began to increaſe again, and in a few Minutes it deſcribed an Arch of two Degrees, and the Clock went. It's Vibrations continued to increaſe for a conſiderable Time, but it never vibrated four Degrees, as when firſt ſet a-going. Whilſt the Vibrations of N° 2. increaſed, thoſe of N° 1. decreaſed, till the Clock ſtopped, and the Pendulum did not deſcribe an Arch of more than one Degree 30 Minutes It then began to increaſe again, and N° 2. decreaſed, and ſtopped a ſecond time, but was ſet a-going again, as before. After this N° 1. ſtopped a ſecond time, and the Vibrations continued to decreaſe till the Pendulum was almoſt at Reſt. It afterwards increaſed a ſmall matter, but not ſufficiently to ſet the Work a-going. But N° 2. continued going, it's Pendulum deſcribing an Arch of about three Degrees.

Finding them to act thus *mutually* and *alternately* upon each other, I ſet them both a-going a ſecond time, and made the Pendulums describe as large Arches as the Cases would permit. During this Experiment, as in the former, I sometimes found the one, and at other times the contrary Pendulum to make the largest Vibrations. But as they had so large a Quantity of Motion given them at first, neither of them lost so much during the Period it was acted upon by the other, as to have it's Work stopped, but both continued going for several Days without varying one Second from each other; though when at a Distance, as was before observed, they varied one Minute 36 Seconds in 24 Hours. Whilst they continued thus going together, I compared them with a third Clock, and found that N° 1. went $1'\ 17''$ faster, and N° 2. $19''$ slower, than they did when placed at a Distance, so as to have no Influence upon each other.

Upon altering the Lengths of the Pendulums, I found the Period in which their Motions increased and decreased, by their mutual Action upon each other, was changed; and would be prolonged as the Pendulums came nearer to an Equality, which from the Nature of the Action it was reasonable to expect it would. This discovers the Reason why the Pendulum of N° 2. when left at Rest, would be set a-going by the Motion of N° 1. whereas if N° 1. was left at Rest, it would not be set a-going again by the Motion of N° 2.

For I found by several Experiments, that the same Pendulum, when kept in Motion by a Weight, would go faster, than when it only moved by it's own Gravity. On this Principle, which may easily be accounted for, it follows, that during the Time in which the shortest Pendulum, N° 2. was only acted upon by N° 1. it would move slower, and the Times of it's Vibrations approach nearer to an Equality with those of N° 1. than after it came to be kept in Motion by the Weight; and by this means the Time which N° 1. would continue to act upon it, would be prolonged, and be more than was required to make the Pendulum describe an Arch sufficient to set the Work a-going. But on the contrary, while the Pendulum of N° 1. which was the longest, was only acted upon by N° 2. as it would move slower, the Difference of the Times of the Vibrations would be increased; and consequently the Time which N° 2. would continue to act upon it, would for this Cause be shortened, so that before the Pendulum of N° 1. would describe an Arch sufficient to set the Work a-going, the Period of it's being acted upon would be ended, and it would begin to act upon N° 2. at which time it's Vibrations would immediately decrease, and continue to do so till it came to be almost at Rest. And thus it would continue sometimes to move more, and at other times less, but never sufficiently to set the Clock a-going.

Some Considerations, whether Pendulums are disturbed by any centrifugal Force; by Jo.

V. The Method used in discovering the centrifugal Force has always been, to compare Observations made in Countries lying at a vast Distance from each other. But I have begun to think, whether the same end might not be obtained, tho' there was no Distance of Country between the Observations made. But in order to explain my Thoughts the more

easily,

easily, I shall begin with mentioning what the learned *Huygens* has laid down, in his *Dissertation on the Cause of Gravity*, when he endeavoured to discover how much a Pendulum ought to be shortened, which is carried from *France* to the Equator. But as his Figure is so constructed, that all the Lines seem to be in the same Plane, I have endeavoured to form a new Scheme to represent scenographically a Part of an armillary Sphere, which will help the Imagination better, and at the same Time be more convenient for the Addition of those Parts, which serve to explain what I propose. annes Marchio Poleni, *F. R. S.* No. 468. p. 299. Jan. 1742-3. Fig. 93.

The Circle P A Q E (says *Huygens*) *represents the Earth, cut by a Plane passing thro' each Pole* P Q (therefore this Circle will be a Meridian). *The Centre is* C: *the Equinoctial Circle* E F A G: *The Parallel of Paris* D N O: *Paris* D: K H *represents a Rope sustaining a leaden Weight* H: *which recedes from the Perpendicular* K D C, *because it is thrown back by the circular Motion, according to the Line* D M, *which I suppose to pass thro' the Weight* H. Now D M is a Tangent to the Circle D N O, the Parallel of *Paris*, in the Point D.

Now if we would know what should be the Situation of the Thread K H, *and how much less the Lead* H *should gravitate, than if it hung perpendicularly according to* K D, *we must consider the Point* H, *as if drawn by 3 Threads* H C, H M, H K; *of which* H C *draws toward the Centre of the Earth with the whole Weight which the Plummet would have, if the Earth stood still:* H M *draws according to it's proper Direction, with the (centrifugal) Force given by the Motion of the Earth, in the Circle* D N O: *and* H K *is drawn, or draws, with that Force which is sought. Therefore if* C H *be produced, and* K L *drawn parallel to* D M, *it is known that the 3 Sides of the Triangle* H L K *are proportional to the Powers which draw the Point* H; *and that the Side* L H *answers that which draws by* H C; *the Side* K L *to that which draws by* H M; *and the Side* H K *to that which draws or sustains the Plummet by the Thread* K H. *But the Triangle* K D H *is imagined to have it's Sides equal to the Sides of the Triangle* H L K; *because* C H L *is as it were parallel to* C D K. *Therefore the Sides of the Triangle* K D H *answer to the same Powers: namely, the Side* K D *to the absolute Gravity of the Weight* H, *which it would have, if the Earth stood immoveable*; D H *to the Power which the daily Motion* (producing the centrifugal Force by the Tangent D M) *gives it*; *and* K H *to the Gravity sought.* But I consider the *Power* of the centrifugal Force, namely that which answers to the Tangent D H.

Thus far I have laid down from *Huygens*'s Method what greatly related to my Purpose; but so far only as *is necessary to consider* the Plummet H, *as drawn by the 3 Threads* H C, H M, H K; when the Plummet H is held immoveable by these 3 Threads, or by these 3 Powers. But if it must be moved; that is, if the Pendulum oscillates; I suspect that new Considerations must be had of that Motion of Oscillation: Therefore I shall make a step towards them, and now treat of the Parts, which must be added to the Figure.

But before I speak of these, I shall observe, that I have made use of a Figure accurately formed of solid Parts of a thicker iron Wire. I shall also observe (considering the *Hypothesis* of the Earth's being moved) that one and the same Arch is not perfectly described in the same Plane, in one Oscillation of the *Pendulum*, from it's Centre; and at the same time, as the differences thence arising do not disturb my Purpose, that I may safely neglect them,

With regard to my Figure, I desire it may be perfectly understood, that thro' the Point H a Plane is drawn parallel to the Meridian P A Q E, and that in this Plane an Arch is marked B T V; which, when the Pendulum K H oscillates in such a manner, that the Centre of the Plummet H never departs from that Plane, would be described by the same Centre in the same Plane. Let this Arch B T V be called the first Arch.

Then imagine another Plane to be extended thro' the Tangent D M and the *Radius* D C, and the Arch R I S to be delineated on this Plane, which, when the Pendulum K H oscillates in such a manner, that the Centre of the Plummet H never departs from this Plane, would be described from the same Centre in this Plane. Now it is manifest, that those 2 Arches B T V, R I S intersect each other in right Angles at H.

These things being thus laid down, two Cases worthy of particular Attention occur; or 2 Directions of oscillating Pendulums are chiefly to be considered: one by the first Arch B T V, the other by the second R I S.

As to the first; when the Pendulum oscillating by the first Arch BTV, is moved in a Plane which is always equidistant, by the Space of the Length of the Line D H, or is always so much distant as the value of the whole centrifugal Force by the Tangent D H; it seems to be clear in this case, that the Power of the centrifugal Force by D H, the Power of the Gravity by H C, and the Power of the Thread by H K, notwithstanding the Oscillation of the Pendulum, are always tempered together by the same Proportion, which *Huygens* has explained, serving also to keep the Pendulum immoveable, as I mentioned before.

As to the other; in which the Pendulum is moved by the second Arch R I S in the same Plane, in which the Line of Direction of the centrifugal Force is D M. In this Case that Force does not seem to act, so as to endeavour to draw the Centre of the Plummet H from this it's Plane; but whilst the Pendulum tends from R to S, this Force seems also (as it acts in the same Plane by it's Direction from D to M) to concur in increasing the Motion of the Pendulum. But, on the contrary, whilst the Pendulum retires from S to R, that Force seems, by it's Direction from D to M, to retard the Motion of the Pendulum.

Therefore the proper Motion of the Pendulum, or that which would be referred to one central Gravity acting according to D C, in the first Case

Caſe of the Excurſion by the Arch B T V, is varied by the centrifugal Force, becauſe it is affected by the Motion ariſing thro' D H, from that centrifugal Force, with which Motion it muſt neceſſarily be compounded. But in the ſecond Caſe of the Excurſion by the Arch R I S, it is varied, becauſe in one entire Excurſion toward S, it is accelerated by the ſame Force, directed from D to H; but it is retarded alſo by the ſame Force in the contrary return towards the oppoſite Side R.

Therefore as it ſeems conſonant both to Reaſon and Calculation, that the Variation made in the Arch B T V is not equalled by that made in the Arch R I S; it alſo becomes probable, that there muſt be ſome difference between thoſe 2 Caſes, namely, between the Motions of an oſcillating Pendulum according to the ſecond Arch R I S, and thoſe according to the firſt Arch B T V.

Theſe few things being now propoſed, I have ſufficiently ſhewn what I think I have diſcovered, by the Obſervation of that difference. For I think I have diſcovered a Method of finding, by the help of Obſervations, ſomething about the centrifugal Force, which has been applied to the Rotation of the Earth about it's own *Axis*, tho' no Alteration of Place is made between the making of the Obſervations.

To the Caſe of the firſt Arch will anſwer a Pendulum placed upon any Meridian Line, ſo that the Oſcillations may be made as near as poſſible according to that Line: And to the Caſe of the ſecond Arch a Pendulum will be accommodated, if it is ſo placed, that the Line of the Oſcillations is at right Angles with the Meridian Line. We might have longer Pendulums to our Clocks for ſuch Experiments, namely, of the Length of 9 horary Feet.

VI. Dr *Jurin* having propoſed * two Queſtions in Gunnery to be examined, the *Society* was pleaſed to appoint a Committee for that Purpoſe.

The Report of the Committee of the Royal Society *appointed to examine ſome Queſtions in Gunnery.* No. 465. p. 172. *Read* Nov. 4 1742.

The Queſtions were,

1. *Whether all the Powder of the Charge be fired, before the Bullet is ſenſibly moved from it's Place?*

2. *Whether the Diſtance to which the Bullet is thrown, may not become greater or leſs, by changing the Form of the Chamber, though the Charge of Powder and all other Circumſtances continue unchanged?*

At the Meeting of the Committee it was propoſed to divide the Firſt Queſtion into two Parts.

1. *Whether all the Powder of the Charge be fired?*

2. *Whether all the Powder that is fired, be fired before the Bullet is ſenſibly moved from it's Place?*

As to the Firſt part of the Firſt Queſtion, *the Committee are of Opinion, that all the Powder of the Charge is not fired.*

They found their Opinion upon the following Experiments:

* *June* 24. 1742.

Pieces